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THE

BRITISH AND FOREIGN MEDICAL REVIEW,

FOR JULY, 1844.

PART FIRST.

Analytical and Critical Reviews.

Art. I.

Clinique Chirurgicale de l'Hôpital de la Pitié. Par J. Lisfranc. Tome troisième.—*Paris*, 1843.

Clinical Surgery of the Hospital of La Pitié. By J. Lisfranc.—*Paris*, 1843. Third Vol. 8vo. pp. 748.

The present volume, which completes M. Lisfranc's work, is—with the exception of two chapters, one on tumours adjacent to but not connected with the female organs of generation, the other on hemorrhoids,—entirely devoted to certain diseases of the uterus and of its appendages. We shall not lay before our readers a detailed analysis of the entire volume, as the greater part of its contents necessarily consist of generally known facts, but we shall endeavour to notice whatever seems valuable either from being more or less novel, or from bearing on important or unsettled points of pathology or practice.

Fibrous tumours of the uterus. The first chapter treats of fibrous tumours of the uterus, a disease not yet fully elucidated, notwithstanding the labours of Bayle, Bichat, Roux, Cruveilhier, Dupuytren, Lee, and others. The history given of the pathological anatomy of the affection agrees in the main with that given by the writers whom we have just enumerated, and as anything on that head requiring remark may be more conveniently noticed when considering the chapter on polypus of the uterus, we shall only advert to M. Lisfranc's views respecting the symptoms, diagnosis, and treatment of the malady.

The account of the symptoms produced by fibrous tumours, though rather diffuse, is not very accurate. M. Lisfranc does not, for example, mention how materially the symptoms vary according to the situation of the tumour, and it might be readily inferred from his account, that those symptoms are the same, whether the growth projects towards the peritoneum or encroaches on the cavity of the uterus; while it is to the latter form of the disease that the description given is chiefly applicable. On such points, however, we shall not dwell; our object is not to examine the history of the affection in detail, but to state M. Lisfranc's doctrines respecting its symptomatology and treatment, which, though scattered through fifty-two pages, are comprised in the three following propositions: First, that there are no signs by which the existence of a fibrous tumour of the uterus, can in any case be positively ascertained during life. Secondly, that, in consequence of the impossibility of forming an accurate diagnosis, every supposed case of fibrous tumour of the uterus should be treated as if it were a curable induration of that organ. And thirdly, that, though the true fibrous tumour is always incurable, its progress may often be arrested for years, by means of such treatment. M. Lisfranc claims those opinions as his own undoubted discoveries, and alleges, as we shall see, that he has sustained no small persecution for having been bold enough to promulgate them. How far this claim is founded, we shall briefly examine, after having stated the facts and reasoning adduced by our author in support of his views, and this we shall do as nearly as possible in his own words.

The rational symptoms produced by fibrous tumours of the uterus, merely announce the existence of some disease of that organ, without specially indicating any particular malady; but there are three symptoms, to any of which, where present, M. Lisfranc attaches very considerable importance, viz. the occurrence of expulsive pains at variable periods, but more especially a few days before or after menstruation; the existence of menorrhagia, usually very obstinate, and, when controlled, extremely liable to return;—and above all, to this, viz.

> "That the volume of womb may diminish under appropriate treatment, and after a time again augment in size. It is not very unusual to see this diminution and subsequent increase of the hypertrophy of the uterus occur several times in succession. The phenomenon, I think, may be thus satisfactorily explained. The medicines administered disperse, but only for a time, either partially or entirely, the hypertrophy of the tissue of the uterus, caused by the presence of the fibrous tumour. These last-mentioned signs are, in my opinion, very important, they are not, it is true, pathognomonic, but they afford very strong presumptions in favour of the existence of a fibrous tumour." (p. 13.)

M. Lisfranc next considers the physical signs, or those ascertainable by the sight and the touch, and maintains that they are equally inconclusive as the rational signs. The uterus may be enlarged in size, we may detect with the finger or speculum, or both, hard tumours more or less circumscribed, but we cannot with any certainty determine whether such tumours are fibrous growths, or indurations consequent on simple inflammation of the womb.

> "When we discover," says M. Lisfranc, "one or several tumours, more or less rounded and circumscribed, situated on the womb, it is supposed that those tumours are always fibrous. From this grave error it follows that a merely palliative treatment is adopted, and thus the opportunity is frequently lost of curing tumours, whose progress, because of the inertness of the treatment, remains unchecked, and usually terminate in the death of the patient. It is necessary to prove the truth and value of this opinion, and I shall, therefore, here reproduce the evidence so often adduced at my clinique.
>
> "As inflammation of the uterus may exclusively affect the neck or the fundus of the organ, why may it not also occupy still more limited portions of the womb? and why may it not terminate in indurations more or less rounded and circumscribed?

"We know that inflammation, affecting the thigh for example, may disappear almost completely, only leaving here and there isolated indurations more or less rounded and circumscribed. May not inflammation of the uterus terminate in the same way? Reasoning and analogy favour this view, which is also sanctioned by experience. When I was in the habit of exhibiting operations on the dead body, I have opened the bodies of many women who died of chronic metritis or metro-peritonitis, and frequently observed this to be the fact. Do we not also know that true abscesses, sometimes containing very concrete pus, have been found in the parietes of the uterus? I have met with such an abscess in the centre of a hard tumour, perfectly rounded and circumscribed.

"I have demonstrated round and circumscribed indurations of the uterus, lobulated or uneven on the surface, like fibrous tumours, which they also perfectly resembled in hardness.

"When I commenced practice, I was misled by received opinions, and set down these tumours as fibrous, but I soon discovered that their size in many instances diminished under the influence of antiphlogistics and narcotics, employed with the view of relieving inflammatory and nervous symptoms. I was thence induced to administer discutients when the inflammation had been completely subdued, and thus frequently effected their complete resolution. Need I say, that it is impossible to disperse a fibrous growth? From the foregoing data then I conclude that simple inflammatory indurations of the uterus have often been mistaken for fibrous tumours. I am in possession of numerous cases which bear out this opinion, of which the following are examples." (pp. 17-18.)

The cases here referred to are twenty-six in number, and are given at such length, that they occupy twenty-three pages. It would be superfluous to analyse them. Suffice it to say, they afford examples of indurated tumours of the uterus, closely resembling fibrous tumours, but which yielded to the treatment to be presently mentioned.

"From these observations," says M. Lisfranc, "to which, I repeat, I could add many others, I conclude that there are no certain signs of the existence of fibrous tumours of the uterus previous to their assuming the form of polypi. For the expulsive pains merely raise a strong presumption of the existence of those tumours, inasmuch as those pains may be caused by clots or other foreign bodies in the cavity of the organ, and are sometimes even occasioned by simple tumefaction of the uterus, a fact which I have ascertained by post-mortem examination; the same, as I have already stated, holds good of obstinate and frequently recurring sanguineous discharges, continuing for a long period, and also of augmentation of the bulk of the womb, after its volume has been reduced by appropriate treatment.

"In this uncertainty of diagnosis what course should the practitioner pursue? He should employ the treatment suitable for simple inflammatory induration of the uterus, and by so doing, will frequently succeed in curing the disease. Numerous facts, I cannot too often repeat, sanction this novel opinion, which has proved offensive to many, and has exposed me to numberless attacks.

"Furthermore, fibrous tumours usually cause the same symptoms as simple tumours of the uterus, and it is therefore sound surgery to adopt the same treatment in both cases. If, in fact, we have to deal with a fibrous tumour, we mitigate or even remove the symptoms it excites, and may possibly to a certain extent, prevent their recurrence; finally, we may retard or render stationary its dangerous progress, without obstructing its natural cure by the conversion of the tumour into a true polypus; so far indeed, from the treatment interfering with this salutary result, it, on the contrary, favours its occurrence; if, however, the tumour consists of an hypertrophy either simple or indurated, it has been shown that it may be cured." (pp. 41-3.)

"In some cases, under the influence of the treatment, the apparently fibrous tumour remains stationary, and the patients may live for years, experiencing generally but little inconvenience, though liable to the supervention of severe symptoms which however, usually admit of being remedied. I have met with several cases of this kind, but shall cite only one." (p. 44.)

The history of this case is interesting, inasmuch as the existence of a true fibrous tumour was ultimately ascertained by dissection. During a period of fifteen years, however, its progress was arrested, several intercurrent attacks of subacute inflammation, accompanied with very considerable enlargement and induration of the uterus, were successfully treated, and the patient passed a life of tolerable comfort, until finally carried off by pneumonia.

The treatment pursued by M. Lisfranc in these cases, consists in first dissipating any inflammation or congestion that may be present, by abstinence from sexual intercourse, rest, a non-stimulant regimen, one or more bleedings at the arm to the extent of three or four ounces each, emollient and nearly cold injections into the vagina, laxative enemata, the occasional use of Seltzer water and other mild aperients, and the administration of cicuta, belladonna, and opium, by the mouth or by the rectum, as circumstances may require. When this first indication has been fulfilled, iodine is to be administered in the form of frictions with iodide of lead ointment on the groin, either alone or conjointly or alternately with iodide of potassium internally, and sometimes an issue or seton is inserted in the region of the uterus, or of the ovaries. This of course is a mere outline of the treatment, which must be suspended, resumed, and varied according to the progress of the case. Unwearied perseverance is essential. In one of the cases, the patient was under treatment for thirty-seven months, in the others, a cure was effected in seven, ten, five, eight, and ten months respectively.

As to M. Lisfranc's claim to priority in advancing the foregoing views, it is, so far as we know, partly founded, and partly the reverse. We do not know that any previous author has formally maintained that we can *never* discriminate a fibrous tumour of the uterus, from a simple induration of that organ, though the obscurity of the diagnosis in many instances, and the impossibility of effecting a diagnosis in very many others, is of course well known, and generally admitted. But as to the practice to be adopted in *hard* tumours of the uterus, the French surgeon has been anticipated by Dr. Ashwell, in papers published in 'Guy's Hospital Reports,' seven or eight years since. Dr. Ashwell, indeed, has not enunciated his views with the same precision as the French author; in one place he speaks of *hard* tumours, and then again of *fibrous* tumours. M. Lisfranc, it will be observed, considers the fact of a tumour being absorbed as satisfactory proof that it was not a fibrous growth, an opinion in which we most fully concur. Some English authors, on the contrary, admit the possibility of the absorption of fibrous tumours, but what Dr. Ashwell's opinion on this point is does not very clearly appear. In one place he says, "in hard tumours of the walls or cavity of the uterus, resolution or disappearance *is scarcely to be expected;* since the growths are adventitious or parasitic, and are not imbedded in glandular structure. Here the prevention of further deposit—in other words, the restraint of the lesion within its present limits, and the improvement of the general health—will be the extent of the benefit derived." (Guy's Hospital Reports, 1835, and Practical Treatise on the Diseases peculiar to Women, part ii, 1843, p. 293.) But in two other passages, he informs us, first, that "a hard or fibrous tumour has once, in my own practice, disappeared without the use of any medicine," and secondly that he cannot "think it impossible that this same agent (iodine) may produce absorption." (Practical Treatise, &c. pp. 297-9.) Moreover, Dr. Ashwell (*id. opus*, p. 370) includes within the category of *hard* tumours scirrhous tumours of the uterus, as to the curability of which, we are utterly incredulous. We might readily extend these remarks, but we shall have occasion to notice more fully Dr. Ashwell's labours in this and other respects, when his practical treatise on female diseases shall have been completed, and in the meantime, our object is merely to observe, that, despite of much vagueness and want of precision, he has anticipated, at least in print, the practical conclusions of M. Lisfranc.

Polypus of the uterus. M. Lisfranc admits, with M. Malgaigne, five varieties of uterine polypus. The *vesicular*—the *cellulo-vascular*—that consisting of *hypertrophy of the tissue of the uterus*—the *moliform*—and the *fibrous*. Nothing can exceed the tedious prolixity, the fatiguing repetitions, and the defective arrangement of this chapter; but on that very account, and as it must be interesting to know the views of a surgeon of such celebrity as M. Lisfranc, respecting so important a subject, we shall collect into somewhat of a connected form the scattered fragments relating to perhaps every point of sufficient prominence to require notice.

Though the account given of the pathological anatomy of fibrous polypi of the uterus contains scarcely a new fact—does not even give a full view of the state of knowledge on the subject, it will yet be convenient to give a brief abstract—not indeed of all that is said respecting the structure of those tumours but merely—of the statements relating to certain points which, with a few exceptions we select,—not we repeat, because of their novelty, but because they directly bear on certain practical points afterwards adverted to by the author.

Fibrous polypi are covered by an *envelope*, which varies in structure; sometimes it seems to be derived from the mucous membrane of the uterus. Its thickness varies, but it is generally thinner and more adherent at the base of the tumour, and is elsewhere more loosely attached, occasionally so much so, that enucleation can be effected with facility. It may be white, smooth, polished, apparently fibrous, and without a trace of vessels; or, on the contrary, red, rugous, and presenting on its surface a vascular network interspersed with prominent veins, and containing arteries of some magnitude. The envelope very generally alone forms the peduncle of the tumour, and it is not unfrequently perforated or absorbed, so as to expose the polypus at one or several points, and in a few rare cases, M. Lisfranc has seen it completely removed almost to the point of attachment to the tumour. In other cases the envelope is exclusively formed by a large layer of the tissue of the uterus, which has been protruded before the tumour during its progress towards or into the vagina, and then the peduncle consists of the proper substance of the womb; in this case also, the envelope is often perforated to a greater or lesser extent by the included fibrous body; or again, these two structures may be combined, as the investing layer of the uterus may be covered by the preternaturally vascular and thickened lining membrane of the organ. (pp. 58-9.)

Fibrous polypi occur in one of three states. 1st. *Fleshy*—when they are either red and soft like muscle, or firmer and paler, perhaps yellowish gray or even white. Their tissue consists of a mass of interlaced fibres, presenting here and there indurated points, probably formed by a more compact interlacement of the fibres. The consistence of the tumour may be very soft, and it may contain distinct blood-vessels as well as a considerable quantity of loose cellular tissue placed between the fibres by which it is essentially formed. 2dly. The tumour may be more or less completely converted into *fibro-cartilage*, a change which commences in the more indurated points above mentioned, and may gradually invade the entire mass. And 3dly. The fibrous growth may become partially or entirely *ossified*. (pp. 61-2.) But polypi, properly so called, scarcely ever undergo this bony degeneration, which is almost peculiar to fibrous tumours, while impacted in the cavity of the parietes of the uterus. When uterine calculi occur loose in the organ, M. Lisfranc is of opinion that they never were pedunculated, as, in three dissections, he found such tumours projecting partially and quite denuded into the cavity of the womb,—in the first stage of their escape in fact; for had the process been continued they must of course have soon become detached. (p. 82.) In one instance however, the only case of the kind M. Lisfranc is aware of, he removed a pedunculated polypus about the size of a hen's egg, and apparently of the usual consistence and density of such tumours, but on dissection, a bony concretion, lodged in a kind of smooth polished cyst, was found within an envelope formed by the tissue of the uterus. (pp. 117-18.) With reference to the once much disputed point, whether fibrous polypi are ever supplied by distinct blood-vessels communicating with those of the uterus, M. Lisfranc adds another case to those already known, which determine the question in the affirmative. (pp. 63, 72.)

Whatever the original condition of a polypus may have been, its structure may be found materially changed by the morbid alterations to which those tumours are liable; and there is considerable difference of opinion as to what those alterations may be. Some maintain that fibrous polypi rarely, or even never, inflame, ulcerate, or suppurate, or at least that those changes are confined to the vascular covering of the tumour; but M. Lisfranc, in common with some previous observers, Dupuytren, for example, has *several times* found abscesses in the very substance of the tumour. (p. 63.) Inflammation may cause polypi to adhere to the neighbouring parts, e. g., to the neck of the uterus, and thus a communication may be established between the uterine vessels and those of the polypus (p. 81); or if two or more polypi coexist, they may, from the same cause become adherent to each other, and give the appearance of tumour with several peduncles. (p.78.) M. Lisfranc has also repeatedly seen fibrous polypi undergo cancerous degeneration, and observes, that the notorious difference of opinion respecting this point "might well excite astonishment, were we not aware how frequently accident influences individual experience, a circumstance very liable to impede the progress of knowledge, unless we are cautious not to deny the reality of facts, merely because they have not happened to fall under our own observation." Occasionally fibrous polypi become infiltrated with fluid, soft, pulpy, and easily broken up. This alteration, which may materially influence the treatment, may result, M. Lisfranc conceives, either from the neck of the uterus constricting the peduncle, and causing congestion of blood in the tumour, or simply from an alteration of its vital properties. (p. 63.)

Some, for example, Cruveilhier, maintain that a fibrous tumour cannot become pedunculated, while it is within the cavity of the uterus, as the parietes of the organ then compress it equally on every side, but that the moment the resistance of the cervix uteri is overcome, a peduncle is formed. (Anat. Path. Livr. xiii.) The inaccuracy of this opinion has been shown by the observation of several cases in which intra-uterine polypi were perfectly pedunculated; and M. Lisfranc adds two additional examples of this to those already known. In dissecting two women, he discovered two large fibrous polypi, both completely incarcerated in the uterus, and both attached by a very *slender* peduncle. This also disproves the notion that the peduncle is formed by the constriction exerted by the cervix uteri. (pp. 73, 150.) M. Lisfranc, however, admits that the pressure of the neck of the uterus causes the deep circular depressions occasionally met with on polypi, and which, on a superficial examination, might seriously embarrass the diagnosis, as the portion of the tumour above the depression, especially when it is very deep, might be mistaken for the uterus. (pp. 61-73.) When a fibrous tumour has passed the orifice of the uterus, it very generally assumes the shape of a polypus, but the author has seen several cases in which the tumour projected a considerable distance into the vagina, without being in the slightest degree pedunculated; in such a case, enucleation may occasionally be very easily effected. (p. 74.)

It is generally admitted that polypi are more frequently connected with the body than with the neck of the uterus; M. Lisfranc has, on the contrary, found even fibrous polypi much more commonly attached to the latter part of the organ. Polypi of the uterus may grow from any point of its internal surface, but, according to Boyer, they are usually inserted somewhere between the orifices of the fallopian tubes, an opinion, which is confirmed by M. Lisfranc's experience, as the tumour occupied this situation in forty-three out of sixty cases, which he either operated on or dissected. (pp. 85-6.)

Formerly, polypus of the uterus was thought to be a very rare disease; M. Lisfranc thinks its frequency is, on the other hand, now greatly overrated, and founds this opinion on the result both of his dissections and of his examination of the living. A large number of females affected with diseases of the uterus attend as out-patients at La Pitié, and from twenty-five to thirty women labouring under similar maladies are constantly inmates of the hospital; his private practice in this class of cases, is also very extensive; and yet during the last fifteen years, he has only seen, on an average, twenty cases of polypus of the uterus yearly. This statement, if it could in any way bear on the question of the frequency of uterine polypi, which obviously it cannot, would disprove the very point our author is attempting to establish, for so far as we are aware, it enormously exceeds what has fallen within the experience of any other practitioner; for example, it is *only double* the number of cases that fell to the share of Dupuytren. (Leçons Orales, t. iii, p. 450.) M. Lisfranc confesses that he has not "dressé une statistique" of his post-mortem examinations bearing on this point, and we suspect he has made the same omission with respect to his living patients. (p. 66.)

In treating of the symptoms and diagnosis of polypi of the uterus, M. Lisfranc especially insists on the following points as being both novel and of practical importance. Thin, narrow, riband-like polypi, and also those of small size, with a long filiform peduncle, are difficult to be detected, as they offer scarcely any resistance to the finger, and consequently communicate to it a very obscure sensation; but by prolonging the examination, the polypus may at length be fixed against the neck of the womb, and its presence be detected by gentle lateral motions of the finger. If the speculum is applied, the riband-like polypus may readily escape observation, in consequence of being glued, as it were, by glairy mucus to the neck of the uterus, but by carefully wiping the parts with a dossil of lint, the morbid growth is detached from its temporary adhesion and brought into view. The existence of a very small polypus with a filiform peduncle, may have been satisfactorily ascertained, both by the finger and by the speculum, and yet on a subsequent examination it may be quite impossible to discover it, as tumours of this kind occasionally reascend into the uterus, and become for a time, inaccessible, because of the orifice of that organ contracting; or if, on the other hand, the neck of the uterus is much dilated, the tumour may be pushed through it before the finger, without being felt. In such cases the first diagnosis must not be hastily assumed to have been incorrect; in either case, the tumour will soon descend again, and be detected on repeating the examination, perhaps, after the lapse of several days. (p. 106-14.) The fact that the inferior orifice of the uterus is more dilated during and for a few days before and after menstruation, and that intra-uterine polypi are consequently then most readily detected, is stated amongst the circumstances grouped together as new, (p. 107;) but has been long since specially stated by other authors, e. g. Dupuytren. The same observation applies to several other statements, which we abstain from noticing.

The differential diagnosis between polypus and inversion of the uterus is considered at great length; and the following observations contain everything, we believe, that is said on this head, to which it is necessary to advert. It is very generally said, that a polypus is insensible, and that an inverted uterus is sensible to the touch. The inaccuracy of this distinctive sign is, no doubt, known. Dr. Johnson, for example, mentions a case, in which a polypus was very sensitive to the touch, (Dublin Hosp. Reports, vol. iii,) and Dupuytren says that polypi cease to be painless when their surface becomes inflamed. M. Lisfranc says that he has *very frequently* met with polypi which were painful on pressure, and assigns, we have no doubt, the true explanation of the fact, viz. that the tumour is frequently covered by the proper tissue of the uterus, though even then the lower part of the tumour may be insensible, on account of the envelope having become very thin, or even perforated in that situation. (p. 133.) When a polypus, or an inversion of the uterus, as the case may be, has scarcely dilated the neck of the womb, it is supposed to be impossible to distinguish the two affections.

> "If," says M. Lisfranc, "the symptoms were very serious, and the life of the patient in danger, it would be our duty could we ascertain the existence of a polypus, to attempt to remove it; would it not then be allowable to seize the neck of the uterus with Museaux's forceps, and depress it to the vulva? and if on then

passing the finger into the rectum, the body of the uterus was felt to be considerably enlarged, might we not conclude that we had to deal with an uterine polypus? . . . . . but, if on the contrary, it was found that the size of the uterus was not increased, that its longitudinal diameter was diminished, would not this indicate that the case was one of inversion? Guided by these new data in a case such as is now under consideration, I succeeded in removing a polypus, and rescued the patient from apparently certain death." (p. 136.)

In partial inversion of the uterus, M. Lisfranc thinks favorably of the mode of examination proposed by M. Malgaigne, which we shall describe, as we do not recollect that it has appeared in this Journal. In this affection, the bladder and a portion of the intestines are lodged in the concavity formed by the depression of the fundus of the uterus; if then, a curved catheter is passed into the bladder with its concavity downwards, and the beak of the instrument is directed to the most depending part of the bladder, its extremity will be readily felt by the finger in the vagina, if the case is one of inversion, unless, indeed, the intestines have become adherent to the womb in such a way as to prevent the catheter penetrating into the depression formed by the inverted organ, a circumstance of very rare occurrence. (p. 137.) But M. Lisfranc thinks that the best way of discriminating between polypus and inversion of the uterus, is by a mode of examination similar to that above recommended, in the case of an intra-uterine polypus or of a commencing inversion. If we seize and depress the tumour with two fingers passed into the vagina, and then introduce the index-finger of the other hand into the rectum, no tumour can be felt through the gut above the one which is grasped in the vagina, if the case is one of inverted uterus. But if, on the contrary, we feel through the rectum a second tumour similar in shape to the uterus, above the vaginal tumour, then this latter tumour is a polypus. In one instance, indeed, M. Lisfranc was misled by this mode of examination; he diagnosticated inversion of the uterus, but the patient having died, a small fibrous tumour was discovered implanted on the uterus, which was flattened and reduced to the tenth part of its natural size. (pp. 139, 174.) It appears that attempts have been made to defraud the author of the honour due to this suggestion, as he subsequently "begs leave to thank the authors who have appropriated his ideas, or with characteristic candour cited them as dating from the eleventh century." (p. 385.) It is not stated who are the delinquents here alluded to, and we are not able to supply the omission.

A great number of circumstances connected with the shape and disposition of the tumour may render the exact nature of a case more or less obscure. Under this head we shall only notice the following case, as we do not recollect having met with an account of one similar to it. A polypus was distinctly felt in the vagina, apparently attached to a peduncle, which could be traced for about half an inch through the neck of the uterus, but its point of insertion could not be reached. The tumour was easily drawn through the vulva, and on then passing the finger along the supposed peduncle, it was found to be attached to and surmounted by, or rather to expand into, a second fibrous tumour. So far the case is merely an example of one of those circular depressions on a polypus, which have been already adverted to. But, on examining the upper portion of the tumour, the finger could be carried fairly round it, without discovering any point at which it adhered to the womb. During the protracted examination that now took place, the uterus was considerably depressed, and at length an adhesion was felt between the inner surface of that organ and the supposed peduncle; on dividing which, the tumour was found to yield, and was soon extracted. The adhesion in fact, which was thought to be accidental, was the peduncle of the tumour. M. Lisfranc supposes that the polypus, during its development, had grown partly towards the vagina, and in part towards the cavity of the uterus. (p. 74.)

When a polypus of the uterus is detected, the indication is to remove it if possible, for with some exceptions the symptoms increase in severity, and a *spontaneous cure* is a very rare event indeed. This interesting point in the natural history of the disease is considered very imperfectly by M. Lisfranc. He mentions two circumstances under which the natural cure may occur: 1st. The peduncle may become elongated and slender, and finally break from the mere weight of the tumour, an event which *he has seen occur in four cases*, where the neck of the uterus was so dilated that it could not have exerted any pressure on the peduncle. (p. 80.) 2d. The pressure of the neck of the uterus on the polypus may act as a ligature, and thus produce a spontaneous cure; this, M. Lisfranc observes, is a very rare occurrence, but he witnessed it in one case which is recorded. (p. 81.) It would thus appear that the author has observed *five* examples of spontaneous cure, but he subsequently informs us, "A spontaneous cure is a very uncertain event. I have only seen *three* cases in which it occurred; a few others have been recorded." (p. 155.) No allusion is made to other modes in which fibrous tumours and polypi have occasionally been naturally gotten rid of, such as their expulsion during parturition, and their separation in consequence of absorption or rupture of the cyst or envelope by which they are covered. Small fibrous polypi occasionally cause but little inconvenience. This is a fact well known; but M. Lisfranc mentions a very remarkable circumstance, that he has had several patients who would not submit to any operation, though they long suffered from attacks of severe uterine hemorrhage; in some of these cases the bleeding disappeared gradually, in others suddenly, and has not returned during three, four, or even five years; the other symptoms underwent a corresponding amelioration, and the patients continue to enjoy tolerable health. (p. 156.)

With respect to the palliative treatment of uterine polypi, we shall only advert to M. Lisfranc's practice in certain cases where hemorrhage has reduced the patient almost to extremity; but we first state his views as to the source of the hemorrhage, as it is on that the practice is based. We shall make no array of the conflicting authorities respecting this point; M. Lisfranc agrees with those who refer the hemorrhage principally to the surface of the tumour. "Some polypi," he says, "do not bleed, and then the very vascular membrane which usually covers them is entirely wanting, and their surface is generally covered *by the tissue of the uterus alone*." (p. 58.) And again, "The source of hemorrhage is almost always the mucous membrane enveloping the tumour, and only occasionally, in some extremely rare cases, the polypus itself: the uterus, however, may unquestionably be implicated as a cause: 1. Because it is often congested, and congestion is alone sufficient to excite an afflux of blood. 2. Because the irritation of the polypus may produce the same effect, and the womb has been found in a state similar to that which it presents during menstruation." (p. 99.) We shall only remark on these passages, that we think M. Lisfranc under-estimates the importance of the altered condition of the uterus as a cause of hemorrhage, as it is well known that that organ when affected with polypus often becomes hypertrophied, softened, and permeated with vessels resembling the sinuses of the gravid uterus, a condition in which the layer of its tissue *covering* the tumour and forming its peduncle has been distinctly seen to participate; but we shall not dwell on this, as our object is merely to come to the practical application of M. Lisfranc's views. If then a patient labouring under polypus is almost exsanguine and reduced to a state of extreme debility, the exertion and fatigue consequent on an operation may be not exempt from danger, the trifling loss of blood that may follow either excision or the application of a ligature might prove fatal, and though the debilitated condition of the system is unfavorable to the development of inflammation, yet if inflammation were to follow the operation it would probably terminate in death.

> "Under such circumstances," says M. Lisfranc, "if the polypus is not too large, if it is covered by the vascular membrane which almost always alone yields the hemorrhage; I tear off that membrane when I can reach it with my fingers, and when it is not too firmly adherent to the subjacent parts; or I apply the largest possible speculum, and cauterize the surface of the tumour freely with the acid protonitrate of mercury; by these means I have always, hitherto, restrained the hemorrhage; and it is superfluous to add, that I operated on the tumour at a subsequent period with a much better chance of success." (pp. 160.)

If a polypus completely within the cavity of the uterus is accompanied with uncontrollable menorrhagia, an operation may appear urgent, but is greatly more difficult and dangerous than when the tumour is in the vagina; in this case the neck of the uterus is very frequently dilated, and M. Lisfranc took advantage of this circumstance in several cases to apply, with the aid of the speculum, protonitrate of mercury to the small accessible portion of the tumour. This practice has almost always succeeded in his hands, especially when repeated twice or thrice within twenty-four hours,—in fact, whenever the bleeding returned. He is inclined to explain the efficacy of cauterizing so very limited a portion of the tumour, by saying that the polypus is compressed by the parietes of the uterus over its entire surface except at the point corresponding to the orifice of the uterus, in which situation consequently it can alone bleed. On removing polypi which had been thus cauterized, a slough was found a few lines deep, and a considerable portion of the tumour was inflamed. M. Lisfranc has never tried this practice except in the case of fibrous tumours, and cannot say how far it might be serviceable with cellulo-vascular and red soft spongy polypi. (pp. 159-67.)

A variety of circumstances may have to be considered before we undertake the removal of a polypus by operation. A polypus may coexist with rounded hard tumours of the uterus, which may be either fibrous or merely the consequence of a long antecedent chronic inflammation; but be their nature what it may, their presence does not contraindicate the removal of the polypus, for if the tumours are not fibrous they will probably disappear even spontaneously, and if they are, though their presence certainly renders the operation more dangerous, yet far greater danger would be incurred by non-interference. (pp. 43, 145.) If a polypus is complicated with diffuse induration of the uterus, occupying perhaps the greater portion of its anterior or posterior surface, as is frequently the case, or even involving the entire organ, the question arises whether we should first treat the polypus or the hypertrophy of the womb, and the answer depends on the presence or absence of chronic inflammation, the existence of which we should always suspect and carefully look for in such cases. If chronic inflammation is present it must be dissipated by appropriate treatment before the polypus can be safely operated on, for if this precaution is neglected an attack of acute inflammation would probably supervene and place the patient in the greatest danger, or at all events symptoms of uterine disease would continue to the distress of the patient and discredit of the practitioner. Nevertheless, if the symptoms are urgent, if the patient is being run down by obstinate menorrhagia, this risk must be encountered and the operation cannot be deferred. If no signs of chronic inflammation can be detected, or when they have been dispelled, the polypus should be at once removed, as its presence irritates and congests the organ. Sometimes the hypertrophy rapidly disappears after the operation; in others it yields slowly, or may even permanently remain. (pp. 145-9, 158, 176, 219, 228.)

Ulcers of the neck of the uterus are often completely masked by a polypus, and the surgeon should therefore be cautious in promising speedy and complete relief from uterine distress as the result of an operation. Many are of opinion that the vaginal discharge diminishes but does not entirely cease for some time after the removal of a polypus, M. Lisfranc has almost invariably observed that the discharge ceases promptly and completely, unless the neck of the uterus was ulcerated or the organ was obviously enlarged. (144-8.)

Anemia, extreme debility, emaciation, colliquative diarrhea, &c., do not contra-indicate an operation provided no organic disease of any important viscus can be detected. (p. 147.) On this last point, especially as regards disease of the lungs, the author lays peculiar stress; he states that he has *frequently* seen females affected with tubercles of the lungs, in whom, on the appearance of a polypus of the uterus, with its accompanying irritation and discharge, the progress of the thoracic disease was so completely suspended, and its symptoms so much amended that the patient forgot, as it were, that it had ever existed, and the surgeon could not even suspect its existence without a most careful inquiry into the history of the case, and a scrupulous examination of the thorax. In such cases, if the symptoms caused by the polypus are mild and not progressive, they must not be interfered with, as experience has shown that if we unfortunately succeed in mitigating them to any considerable extent, the symptoms of phthisis supervene and soon prove fatal. But if menorrhagia, ulceration, or hypertrophy endanger life, our object should be to ameliorate the symptoms, but not to remove them, nor even, if possible to materially mitigate them. M. Lisfranc states that he has seen the lives of "*a great number*" of patients prolonged by such management (*une thérapeutique de bascule*) for four or even six years. (p. 230.)

The selection of the period for operating on a polypus is of great importance. Useless delay would be dangerous, we should not operate within ten days before or eight after the period of menstruation; M. Lisfranc has several times witnessed serious and even fatal consequences from the neglect of this rule. (p. 219.)

Polypi situated entirely within the cavity of the uterus have been successfully removed by Herbiniaux, Desault, Deguire, Dupuytren, Recamier, and others, but as they generally make their way into the vagina, that event should be waited for, unless the circumstances of the case imperatively require interference. If the incarcerated tumour has become soft and pulpy, it may be removed by *avulsion* (*arachement*), and this is the only case in which M. Lisfranc recommends this mode of operation: what he terms *avulsion* is indeed a combination of that process with *laceration* (broiement). He thus succeeded in breaking up two intra-uterine polypi, and having removed as much of the mass as was practicable, left the rest to slough away. (pp. 63, 64-8.) In one instance he unintentionally removed a polypus by true avulsion. While attempting to depress the tumour for the purpose of excising it, a sound, as of something giving way, was heard, and the polypus was found completely detached. (p. 39.)

M. Lisfranc has on several occasions removed by *enucleation* both polypi and fibrous tumours which were not pedunculated, whether situated completely within the cavity of the uterus, or having partly (or in the case of polypi entirely) made their way into the vagina. To use his own words, he "dwells on this important point of practice which he believes to be new." (p. 177.) We need not occupy space in showing that the practice is not new, but as we believe M. Lisfranc has adopted it with more boldness than his predecessors, and under circumstances in which it was not previously applied, we shall give a summary of a few of the cases by which he illustrates this practice.

In one case having drawn a fibrous polypus almost entirely through the vulva, he perceived that its envelope, which consisted of a thin layer of the tissue of the uterus, was lacerated, and passing the index-finger through the rent, enucleated the tumour with the greatest facility. (p. 172.) In another case enucleation was effected almost accidentally: M. Lisfranc, while examining a polypus, found the envelope give way beneath the nail of the index-finger, and by an easy manipulation enucleated the tumour in a few seconds. On examining the uterus immediately afterwards, he found that the part of that organ to which the polypus had been attached, had singularly contracted, that the depression caused by the tumour had diminished greatly in depth, and at least two thirds in breadth, it seemed to be diminishing while the finger was in contact with it, and in ten hours the uterus had regained its natural size, and the cervix would not admit the finger. (p. 173.) We mention these latter facts, as we conceive they have an important bearing on the question of hemorrhage after excision of polypi. M. Lisfranc has also frequently enucleated with the nail of the index-finger, small cellulo-vascular polypi occupying the neck of the womb. (p. 176.) In a case where a fibrous tumour as large as the clenched hand projected into the vagina, its envelope was lacerated with the nails, and the contained tumour turned out. (p. 73.) But enucleation must generally be preceded by an exploratory incision; and by this combination of means, M. Lisfranc has removed fibrous tumours while still completely included within the cavity of the uterus. A lady was reduced almost to extremity, by protracted uterine hemorrhage caused by a fibrous tumour, which could be felt through the dilated cervix uteri. The neck of the uterus was seized with Museaux's hook, depressed almost to the vulva, and a more perfect examination being then practicable, the tumour was found to extend from the middle of the body of the uterus almost to its lower extremity, and to be lodged in its posterior wall, from which it was commencing to disengage itself. With a straight blunt-pointed bistoury passed along the forefinger, a vertical incision was slowly and cautiously made over the tumour until the finger was enabled to be insinuated beneath the envelope and complete the enucleation, which was not accomplished without some difficulty. Occasionally enucleation may be more easily achieved by substituting a spatula for the finger. (p. 179.) If it is necessary to enlarge the incision in order to effect the removal of the tumour, a grooved director will often guide the knife more conveniently and safely than the finger. In some cases, where the cervix uteri was insufficiently dilated, M. Lisfranc divided it anteriorly. (p. 181.) Whenever the peduncle of a polypus is very broad, we should incise the envelope, and endeavour to enucleate the tumour; in this, however, we cannot always succeed. If the tumour is removed, the envelope sometimes contracts and cicatrizes, sometimes sloughs in whole or in part. (pp. 211-14.)

Ligature. We need scarcely say M. Lisfranc rejects this, in common with almost all French surgeons, as a general mode of removing uterine polypi. In this opinion we altogether coincide, and confess our inability to understand the preference given to this method by many respectable British practitioners. It would be out of place to recapitulate the well-known and often repeated objections to the use of the ligature as compared with the operation by excision, as M. Lisfranc adds nothing to the perfectly conclusive arguments long since adduced in favour of the latter proceeding. In certain cases, however, the ligature should be employed: thus, if a polypus of moderate size is completely included within the uterus, and is implanted high up, especially at the summit of the organ, and if the symptoms imperatively demand an operation, a ligature should be applied *if its application is possible*, as it occasionally is when a sufficiently small peduncle can be detected. (p. 217.) Or if a patient is so exsanguine, that the smallest loss of blood is to be dreaded, we should employ the ligature unless the peduncle is too thick, or unless we are unable to bring it when bulky, fairly within our reach, and pierce it with several needles, each armed with a double ligature, and thus tie it in two or more separate portions. (pp. 192-218.) M. Lisfranc disapproves of excising the polypus below the ligature immediately after its application, as we may be compelled to remove it in consequence of its producing serious symptoms, but when the vitality of the tumour is destroyed, as much as possible of it should be cut away. (p. 213-4.)

How should we proceed if an artery is felt pulsating in the peduncle of a polypus? This question has been considered by Dupuytren, and is now discussed by M. Lisfranc, but both authors apparently deal with it merely as a matter of speculation, as neither appear to have met with a case of the kind. Dupuytren recommends excision, having previously applied a ligature of reserve to be tightened in the event of hemorrhage occurring. To this M. Lisfranc objects that the precaution would be illusory, as hemorrhage sometimes supervenes several hours after excision, (p. 214,) and that as the ligature of reserve must of course be left loose, it would slip off were the peduncle very short, or if the peduncle, however long, consisted of the tissue of the uterus, it would probably retract to such an extent, as to escape from the ligature, a circumstance sufficiently exemplified in what has been said respecting enucleation. (p. 217.) In the case now under consideration, M. Lisfranc, if he determined to use a ligature, would at once tighten it so as to thoroughly strangulate the peduncle, then excise the polypus, and eight or ten hours subsequently remove the ligature. Hemorrhage, he is satisfied, need not be then apprehended, for in amputation of the neck of the uterus, the uterine arteries are often opened, and he has frequently restrained the consequent hemorrhage by plugging, but never found the bleeding return on removing the plug after five or six hours; and he has no doubt that such would be the case were an artery in the peduncle of a polypus divided. (p. 214.) If the artery felt in the peduncle was not large, M. Lisfranc would at once perform excision, being perfectly convinced that plugging would certainly control hemorrhage, should it supervene. (p. 216.)

In removing a polypus either by *ligature* or by *excision*, it has been much debated whether it is necessary to divide the peduncle close to the uterus, in order to prevent a return of the disease. Levret and many subsequent writers assert that the entire of the peduncle dies, no matter at what point it may have been tied. Dubois, on the contrary, is of opinion that the portion of the peduncle above the ligature dies only when the ligature is placed near its point of implantation, and that it lives when tied low down. Dupuytren too insists on the necessity of cutting the peduncle as close as possible to its insertion, as he is of opinion that any portion of it which may have been left, is very likely to grow again. This opinion, it is clear, Dupuytren founded on an erroneous view of the pathology of the disease; for we conceive that the peduncle always contains fibrous tissue, that the globular portion of the tumour in fact is connected to the womb by means of a slender fibrous stem, occupying the centre of the peduncle, a circumstance which, though it sometimes occurs, is very rare—is in truth not the rule but the exception. M. Lisfranc has occasionally been compelled to tie polypi at the centre of their longitudinal diameter, and found that the *whole* of the tumour died. In one instance, an enormous polypus filled the vagina, there was a deep circular depression at the centre of the tumour, round which a ligature was passed, in the expectation that when the lower part of the mass was removed, the residue would be more easily dealt with, but the portion above the ligature sloughed as well as that situated below it. (pp. 189-90.) He also witnessed several other similar cases disproving Dubois's opinion. We also find a case in which an attempt was made to excise an intra-uterine polypus, but the peduncle could not be detected, and a portion of the tumour, estimated at about half its bulk, was cut away; the part left behind, however, sloughed, and the patient recovered perfectly. (pp. 242-8.) It cannot, however, be denied that the tumour occasionally grows again when a portion of the peduncle has been left, and this difference of result in different cases cannot be foreseen or explained in the present state of our knowledge. (p. 212.)

As to the after-consequences of operations on uterine polypi, bad symptoms seldom supervene unless a *ligature* or *caustic* has been employed; and from the cessation of the discharges, the relief from suffering, the return of appetite, &c. the patient imagines she is perfectly restored to health, an opinion in which the surgeon unfortunately too often coincides. This impression, if prematurely acted on, may lead to disastrous consequences, because a slight degree of sub-acute inflammation may latently exist, or even if it does not, yet metritis may be readily excited by early indulgence in the most moderate exercise, by any error of diet, by a chill, &c. M. Lisfranc consequently recommends that absolute rest in the horizontal posture should be observed for three weeks after operation, that the diet should be cautiously regulated for some time, and that the patient should be enjoined to resume her ordinary habits very gradually and cautiously. (pp. 219-22.)

Hemorrhage after excision is scarcely to be apprehended. M. Lisfranc, after referring to Dupuytren's experience of the rarity of its occurrence, states that he has himself performed the operation 165 times, and that hemorrhage followed in but two cases. In one case scarcely a drop of blood flowed at first, but in three hours violent bleeding came on, and was restrained by plugging; in the other the hemorrhage was not alarming until five hours after the operation, and was similarly restrained; in both the plug was removed on the evening of the day of the operation without any recurrence of the bleeding. (p. 219.) Not unfrequently a slight sanguineous discharge occurs either immediately after, or one or two days after the operation. It usually comes from the wound inflicted, but may come from an ulcer or from a second polypus that escaped detection. If the patient is not very weak, and if there is no pain, &c., it is well to let this discharge continue for some days, as the patient is thereby gradually accustomed to the cessation of an habitual hemorrhage, and congestion of the uterus may be relieved if it exists, or warded off if it does not. But if the slight flow of blood continues five or six days, or if it is accompanied with pain, shivering, heat of skin, or a sense of weight in the pelvis, interference is requisite, as congestion of the womb exists, is becoming aggravated, and may terminate in inflammation, to prevent which we should adopt the treatment indicated for menorrhagia. If the hemorrhage is caused by a second polypus, by hypertrophy, or by ulceration of the polypus, it may come on unexpectedly when the patient seems almost completely recovered. In the first case it is very obstinate; in the second it usually yields very readily to treatment; in the third, cauterization at once restrains it unless the disease is very extensive. (pp. 125-6.) When no hemorrhage follows the operation, as is usually the case, the sudden suppression of the habitual drain predisposes the patient to visceral congestions and inflammations, and in any case when health has been reestablished, the symptoms of general plethora and of local congestion should be carefully watched for, and combated by venesection and an appropriate regimen. The return of natural menstruation does not sufficiently guarantee the patients from those mischiefs, and they are still more to be apprehended when menstruation is scanty or suppressed. (pp. 229-31.)

The wound caused either by ligature or excision generally heals rapidly, but sometimes remains ulcerated for a long time—for upwards of a year, as M. Lisfranc has witnessed not unfrequently. The wound, or rather ulcer, may then cause congestion of the womb and leucorrhea, or it may even degenerate into cancer. It is therefore necessary sedulously to examine the progress of the wound caused by those operations. (pp. 193, 201, 228.)

Hypertrophy of the uterus, if not indurated, frequently disappears spontaneously in twenty or thirty days after the removal of a polypus. If the tumefaction is hard, but recent and of slight extent, it also usually yields under simple attention to the general health. But occasionally M. Lisfranc has seen indurations, apparently of the simplest kind, become aggravated, and even terminate fatally, and he thence inculcates the necessity of carefully watching those cases, and attempting to disperse the induration by treatment, if it shows no disposition to disappear spontaneously. The surgeon should never tell his patient "you are cured," until after he has thoroughly examined the condition of the uterus. (p. 227.)

Some females after the cure of a polypus, or of any other disease of the uterus, fall into bad health, without any viscus being affected, and without the functions of the uterus being at all deranged, and yet general treatment fails to restore health. Under such circumstances M. Lisfranc thinks an issue in the arm indispensable. Some prefer inserting the issue in the leg or thigh, but M. Lisfranc is satisfied, from repeated observations, that the issue is there positively injurious, as it excites an irritation and congestion of the lower extremity, which solicits a flow of blood to and causes congestion of the uterus. (p. 233.)

Polypi of the vulva. M. Lisfranc has frequently seen tumefaction or prolapsus of the mucous membrane of the urethra, mistaken for polypus, but on compressing the tumour between the finger and thumb, it is generally much diminished in size, and can often be returned into the urethra, and if a catheter is then passed into the bladder, no foreign body, and sometimes even no thickening of the parietes of the urethra can be felt. If pressure fails to reduce the tumour, it is sometimes, at least to a considerable extent, returned by passing a large catheter. The protruded mucous membrane may become indurated, and the diagnosis is then beset with difficulty, but this is of little importance as the tumour must be removed, if it cannot be returned. If the protrusion can be reduced, scarifying the internal surface of the urethra is often beneficial, but the best mode of treatment when the tumour is not indurated or affected with acute inflammation, is to touch it lightly with nitrate of silver every fifth or sixth day. Polypi of the vulva are seldom fibrous; they are sometimes mucous or cellulo-vascular, but are more commonly red, and similar to those termed "*vivaces*" by Levret. They should be removed with the knife and not by ligature, as they are seldom superficially attached, but almost always extend into the subjacent parts to a considerable depth, and are consequently apt to return when removed by ligature; but M. Lisfranc has never known a relapse occur when they were excised, and care was taken carefully to dissect out their ultimate ramifications. (pp. 248-54.)

Polypi of the vagina. These growths resemble in structure those of the vulva. They are much less likely to bleed than uterine polypi. If at all bulky, they usually cause partial inversion of the vagina, in which a portion of the rectum or of the bladder may be included, and apparently constitute the peduncle of the tumour, and consequently either of those organs might be opened, if the surgeon, while operating, did not bear this important fact in mind, and guide his incision by passing into the pouch the finger in the rectum, or a catheter in the bladder, as the case may be. When superficially attached to the mucous membrane they are easily removed, but when more deeply connected, they are liable to relapse unless thoroughly extirpated, a proceeding which endangers the rectum or the bladder. The ligature is objectionable for the reason assigned in the last paragraph, and also because the vesico-vaginal, or recto-vaginal septum, might be included in it. (pp. 255-64.)

Tumours in the recto-vaginal or vesico-vaginal septum. These tumours have not been much investigated. They may be situated between the vagina and the rectum, or between the vagina and the bladder, or they may occupy the thickness of the parietes of any one of these organs. The vagina covering them may be either thickened, or the reverse. In one instance M. Lisfranc states that the tumour had perforated and projected into the rectum, and in another it had partly made its way through the anterior wall of the vagina. M. Lisfranc has very seldom seen tumours *between* the vagina and the bladder; he has met with eleven *between* the rectum and the vagina, *three* of which were fibrous, *two* were enlarged lymphatic glands, *four* were simple white indurations, the result of subacute inflammation, and two were scirrhous. Tumours occupying the proper coats of the vagina or of the rectum are much more common; but M. Lisfranc has never seen a fibrous tumour in these situations. The existence of one of these tumours is easily detected, but it may be difficult or impossible to determine its nature. The treatment, however, should always be directed to effect its restoration; and if this cannot be effected and the tumour is very painful, the author recommends that an exploratory incision should be made over it, provided it is rounded and circumscribed. He has thus succeeded in enucleating two fibrous tumours. It may be objected, that the growth may be scirrhous, and if so would be thus converted into open cancer. To this M. Lisfranc replies, that he only advises the practice when the tumour is painful and the symptoms cannot be relieved, and that there is a chance of meeting with a fibrous tumour.

Occasionally the distress in those cases is caused by the pressure of the cervix uteri on the tumour. A lady suffered the most intolerable pain, radiating from the pelvis to the loins and down the thighs, whenever she attempted to take exercise. Anteversion with prolapsus of the uterus was discovered, and a pessary was applied, but could not be borne for a moment. M. Gensoul was now consulted, and detected in the posterior wall of the vagina a tumour the size of an almond, and exquisitely sensitive on pressure; he applied the pessary "*en poire aplatié*" of M. Mayor of Lausanne, which, from its peculiar shape, rectified the position of the uterus without exerting any pressure on the tumour. While this instrument was worn the patient could walk with comfort, but when it was removed, her sufferings returned. In a second case of M. Gensoul's, relief was obtained by the use of an ordinary pessary. And in two patients under M. Lisfranc's care the same end could only be achieved by the application of M. Mayor's instrument. (pp. 264-75.)

Moles. A mole is defined by M. Lisfranc to be "a fleshy, organized, insensible mass, usually soft, but sometimes more or less hard; its shape is exceedingly variable; it is formed and developed within the womb, from which it is generally supposed to be expelled a longer or shorter period after its formation." (p. 275.) Many authors consider that moles must necessarily be products of conception; but M. Lisfranc coincides with those who maintain that this is by no means uniformly the case. If the embryo dies, it may become extremely atrophied, or even disappear completely; while the placenta, which still retains its connexion with the uterus, may increase and become altered in structure. M. Lisfranc has no doubt that moles are sometimes thus formed, as he has dissected several wombs which contained placenta in which not a vestige of an embryo could be discovered. (p. 281.) But it is indisputable that moles may be formed independently of conception or sexual connexion, and M. Lisfranc then attributes their origin to coagula of blood which have become organized. (pp. 120-1, 282.) Thus the author has seen females long affected with menorrhagia, during which they abstained from sexual intercourse, subsequently void moles; and on dissecting a young girl who died from the consequences of imperforate vagina, he discovered a mole in the uterus. (pp. 282-3.) But few cases are known in which more than one mole existed in the uterus: M. Lisfranc gives a case in which two moles were expelled within half an hour. (pp. 280-90.) M. Lisfranc discusses at considerable length the various circumstances that have been supposed to distinguish moles from pregnancy, and comes to the conclusion that the diagnosis of the former affection is difficult, or, more properly speaking, impossible; the utmost that can be done is to calculate probabilities, after having carefully weighed all the symptoms. (pp. 292-9.) The period that moles remain in the uterus is very uncertain, but they are usually expelled after from sixty to ninety days, though sometimes not for six or seven or even eight months. Their expulsion is often difficult, attended with as much or even more suffering than parturition, and accompanied and followed by hemorrhage. The hemorrhage is sometimes extreme, so that life would be perilled if the surgeon did not promptly extract the foreign body. When the mole has been gotten rid of the lochia flow and the breasts swell, and are supplied with milk usually just as after labour. (pp. 299-301.) As to treatment, moles should not be interfered with so long as they occasion no bad symptoms. If hemorrhage occurs while the mass is still within the uterus, and the neck of that organ is not dilated, and if the means ordinarily adopted fail to restrain the bleeding, so that life is endangered, the hand should be passed into the uterus, even if a certain degree of violence is requisite in so doing; for, however dangerous this resource, it presents a chance of safety. Incision of the neck of the uterus is imperatively requisite if adhesions exist, or if it is impossible to pass the hand or a suitable instrument into the womb. (p. 307.)

The chapter on "*coagula of blood in the uterus*" (pp. 309-17) contains nothing necessary to be here adverted to.

Physometra or tympanitis of the uterus. This disease, it is said, may arise from three causes: distension of the womb from the entrance of atmospheric air; dilatation of the organ by the elastic fluids (sulphurated hydrogen, it is supposed,) generated during the decomposition of organic matter, such as the placenta, a fetus, false membranes, coagulated blood, &c.; or, finally, expansion due to a secretion of gas from the internal surface of the uterus. Whatever may be the source of the elastic fluid, it must be assumed that the cervix uteri is occluded, whether by spasm, a clot, inspissated mucus, a false membrane, a polypus, &c.; or from displacement of the uterus bringing the orifice of the cervix in close and direct apposition with the vagina. It will, we believe, scarcely be denied that gas may accumulate in the uterus in consequence of the putrefaction of a fetus; but the existence of what we may term *dynamic* physometra has been questioned on high authority. Though Frank, Mauriceau, and Delamotte have recorded cases in which the disease was *dynamic* or caused by a morbid secretion of air, yet, as M. Lisfranc says, he has "in this chapter treated of a disease the existence of which is denied by some physicians." M. Lisfranc does not name the physicians to whom he alludes; but we conjecture that he refers to MM. Stoltz and Naegele, two of the most celebrated practitioners in the diseases of females of the present day, who, at the medical congress held at Strasbourg September 1842, expressed their belief that tympanitis of the uterus was impossible, and that the alleged cases of its occurrence were apocryphal. M. Lisfranc has seen several cases in which physometra was caused by the decomposition of extraneous matter included in the uterus, (p. 229;) but only one of these cases is stated. In it the womb extended three inches above the pubis, and occupied nearly the whole transverse diameter of the hypogastrium; on examining the uterus with the finger in vagina, the other being applied on the hypogastrium, he felt a tumour of extraordinary elasticity; during the manipulation the neck of the uterus suddenly dilated, a volume of gas escaped with considerable noise, and the abdomen resumed its natural size; the uterus, however, remained slightly dilated, and at short intervals audibly expelled portions of air; after the lapse of a few days a fleshy mole was expelled. (pp. 321-2.) M. Lisfranc argues, that as the mucous membrane of the intestinal canal indisputably often secretes air, it is unreasonable to deny that the lining membrane of the uterus may also do the same; and in answer to the objection, that any gas generated in the womb when its cervix is not mechanically obstructed must escape, he observes, that every surgeon who has had much experience in examining the uterus must have often observed the remarkable facility with which the inferior orifice of the uterus contracts. We must, however, observe, that M. Lisfranc does not appear to have actually witnessed any case in which uterine tympanitis was purely *dynamic*, that is, unconnected with the presence of any extraneous substance in the womb: we therefore are induced to refer to a case recorded by M. Tessier, (Gazette Médicale de Paris, 5 Janvier, 1844,) which has fallen under our notice while writing this article. A woman aged forty-three suspected that she was pregnant, in consequence of suppressed menstruation, coinciding with a gradual increase of the size of the abdomen, for at least six months; by which time the uterus had ascended almost to the umbilicus. M. Tessier diagnosticated tympanitis of the uterus, because of the remarkably clear sound yielded by the abdomen on percussion, the absence of the stethoscopic sounds peculiar to pregnancy, and the lightness of the tumour formed by the uterus. The patient was at length attacked with pains similar to those of labour, which were soon followed by the expulsion of a large quantity of fetid gas, with relief of all the symptoms; "no clot, no mole, nothing which could refer the origin of the gas to the putrefaction of any foreign body included in the uterus was expelled. Three years have elapsed since the first expulsion of gas, and though the patient has not since been pregnant, the abdomen has frequently become swollen, and again in a few days subsided after the expulsion of a certain quantity of gas." M. Lisfranc does not absolutely deny the possibility of the uterus being distended by the entrance of atmospheric air; but, looking to the structure of the organ, he is strongly inclined to doubt that the affection can ever occur from this cause, except when the womb has been previously amplified beyond its usual dimensions, as, for example, after pregnancy. As to the treatment of the affection, it must obviously be modified according to its exciting causes; and we find but two points necessary to be adverted to. In this, as in every other uterine disease, M. Lisfranc strenuously objects to the employment of intra-uterine injections, inasmuch as they are, however bland the fluid may be, calculated to excite metritis or metro-peritonitis; the recommendation to promote the expulsion of the gas by efforts on the part of the patient he also considers very injudicious; but, with these exceptions, the practice recommended is similar to that usually inculcated. (pp. 317-30.)

Noisy expulsion of gas contained in the vagina we find designated by the uncouth phrase, *œdopsophie.* This disease cannot occur when the inferior orifice of the vagina is largely open; but if it is obstructed by inflammation, or is naturally very narrow, gas may accumulate in the canal, just as injections are often long retained in it, though the patient stands or even walks. The gas may be generated by putrescent matter, may pass from the rectum when a fistula exists, may be simply atmospheric air, which readily finds entrance when the lower portions of the labia, are drawn outwards, as occurs during walking or sitting, if the pelvis is moved laterally, especially in lame females. The gas may also be secreted by the vagina. Usually the presence of the elastic fluid is only known by the noise consequent on its escape; but occasionally there is some uneasiness and pain, with difficulty in passing the feces and urine, and on passing the finger up the rectum the parietes are found in contact from the pressure of the dilated vagina. M. Lisfranc quite agrees with Frank that this affection is very common, though seldom complained of, because it very seldom produces sufficient inconvenience to require treatment; but the author has repeatedly observed air incarcerated in the vagina while examining patients labouring under other diseases of the uterine organs. When air is secreted by the vagina M. Lisfranc thinks the part is irritated or inflamed, and recommends an antiphlogistic plan of treatment. As the escape of air is not under the patient's control, she may be precluded from society; but M. Lisfranc has always succeeded in relieving this inconvenience by directing the patient to give exit to the gas by passing the finger into the vagina previously to mixing in company. (pp. 331-6.)

Hydrometra or dropsy of the uterus. Although a considerable number of cases of uterine dropsy have been recorded by very eminent practitioners, MM. Naegele and Stoltz (to whose opinions M. Lisfranc does not advert) deny the possibility of the occurrence of the disease except in connexion with pregnancy:—because the uterus is lined by a mucous and not a serous membrane; because its dense structure would present an insurmountable obstacle to its dilatation by the fluid; because the fluid would escape; and because, as they allege, no authentic case of the malady has yet been recorded. We shall neither discuss these theoretical objections, nor attempt to refute the latter assertion by a reference to any of the previously-reported cases; but merely observe that M. Lisfranc details two cases of hydrometra, in both of which "the absence of pregnancy was satisfactorily ascertained." (p. 347-8.) We need scarcely further advert to the contents of the chapter on this subject; but may observe that the author dwells on the necessity of removing congestion or chronic inflammation of the womb, if present, before any attempt is made to expel the fluid, whether by baths, purgatives, ergot of rye, or operation; for if this precaution be neglected, metro-peritonitis may be excited and prove fatal, as occurred in one case mentioned by M. Lisfranc, (p. 344,) from the untimely administration of ergot of rye. If, however, the symptoms are menacing and rapidly progressive, the womb must be emptied, notwithstanding the danger thereby incurred. The fluid is best evacuated by means of a gum elastic catheter; if this fails to overcome the obstruction, a silver canula or the finger must be employed, and occasionally we are driven to pass a canula through the cervix uteri. (p. 348.)

Hydatids of the uterus and of the vagina. Our observations on this chapter may be very brief. The great majority of writers maintain that hydatids of the uterus must be the result of conception; M. Lisfranc admits that the clustered hydatid, somewhat resembling a bunch of grapes, has always this origin, but that true hydatids may exist in the virgin uterus, and refers to a case recorded by Percy as indisputably proving this fact. (p. 355.) We may take this opportunity of noticing a statement frequently repeated in this volume: the author deprecates the use of intra-uterine injections in any case, as being eminently calculated to excite inflammation; and contrasts their influence in this respect with the fact that the introduction of instruments into the cavity of the womb is very rarely productive of bad consequences. (p. 361.)

Inversion of the uterus. M. Lisfranc agrees with those authors who consider distension and, perhaps, softening of the parietes of the uterus (whether from pregnancy, the presence of a polypus, &c.) an essential pre-requisite for the occurrence of inversion. Baudelocque, it is true, observed inversion of the uterus in a girl aged fifteen, who had certainly never been pregnant; but M. Lisfranc asks, may not the patient have been affected with retention of the catamenia, hydrometra, hydatids of the uterus, &c.? As to A. Dubois' suggestion, that Baudelocque, in this instance, mistook a polypus for inversion of the uterus, M. Lisfranc rejects it, as purely gratuitous and inconsistent with the great skill and judgment of that eminent practitioner. (p. 379.) The cases in which inversion of the uterus has been supposed to occur several days, weeks, or months after delivery, in M. Lisfranc's opinion, prove nothing, as the affection, he is satisfied, existed in the first instance, but was overlooked in consequence of being incomplete and causing little inconvenience; this position, we may observe, was long since advanced by Mr. Newnham, and perhaps by others, but is here given as if it originated with the author. (p. 383.) Though inversion of the uterus is too frequently fatal, from its immediate or remote consequences, or else produces great misery and distress, yet several cases have been recorded in which it produced but trifling inconvenience; and M. Lisfranc adds a very remarkable instance of this kind to those already recorded. A woman aged seventy, while convalescent from bronchitis, was permitted to remain in La Pitiè until she could be transferred to Salpêtrière; "she exercised in the court of the hospital the greater part of the day; her appetite and digestion were excellent; she experienced no pain in the pelvis, and had no vaginal discharge." A second attack of bronchitis came on and terminated fatally. On dissection, the uterus was examined, as it were, accidentally, and M. Lisfranc "was utterly astonished on discovering complete inversion of the womb." (p. 379.) When the uterus becomes inverted after parturition, and the placenta is adherent to the organ, Clark, Burns, Blundell, Gooch, Newnham, &c. recommend that we should try and reduce the inversion without removing the placenta, chiefly from the apprehension of thereby augmenting the hemorrhage; Baudelocque, Gardieu, Capuron, Boivin and Dugès advise, on the contrary, the removal of the placenta; and M. Lisfranc advocates this practice, because the diminution of the volume of the tumour and the consequent facilitation of its reduction far counterbalances the danger of hemorrhage. (p 393.)

Prolapsus of the uterus. Displacements of the uterus, M. Lisfranc observes, are of extraordinarily frequent occurrence; and he entirely dissents from the generally-received opinion that they are *essential*. Prolapsus, anteversion, retroversion, and lateral deviations of the uterus are excessively rare, the author maintains, when the organ is exempt from hypertrophy; for among the immense number of females that the author has examined, he has met with but very few in whom these affections existed uncomplicated with augmentation of the size of the uterus. The direction of the displacement is governed by the situation of the hypertrophy. If the hypertrophy involves the entire circumference of the uterus, procidentia or prolapsus follows; if the front of the organ is affected there is anteversion, or if the posterior part of the womb is increased in size retroversion, on the contrary, occurs; does the induration occupy either side, the deviation is to that side. "The most elementary knowledge of mechanics enables us to see that a pyriform, slightly-flattened body, suspended in the pelvis from four cords, must, if it becomes sufficiently thickened, have its summit carried towards the symphysis pubis, and *vice versâ*." It may be asked, is the hypertrophy coexisting with displacement of the uterus primary or secondary? but the fact that these displacements are rarely found essential or uncomplicated with hypertrophy, sufficiently answers this question; for as those displacements cause distress they would, on examination, be frequently detected in their first stage, before the hypertrophy had supervened, supposing it to be consecutive; but this is precisely what does not occur. In further proof of his opinion, the author appeals to the fact that if, when a diseased womb is hypertrophied, we treat and remove the hypertrophy the organ nearly resumes its natural position. (pp. 409-12.) But when the displacement is essential or primitive, hypertrophy of the womb may supervene as a secondary affection. (p. 417.) Essential or primary displacements of the uterus are generally very difficult to cure, because the constitution of the patient is greatly deteriorated, and because when the ligaments of the uterus are relaxed or stretched from any other cause than the sheer weight of the organ, it is difficult to restore them to their natural condition. If, however, hypertrophy of the uterus is the cause of the displacement, a cure can generally be effected. M. Lisfranc has not as yet met with a single case in which, when he could succeed in removing the hypertrophy, the womb did not resume very nearly its natural position, and occasionally the restoration was perfect. The treatment must therefore vary with the cause of the affection. (pp. 418-19.) We do not enter into the details respecting the treatment, because, as regards hypertrophy, they are more fully discussed in the chapter on *chronic metritis*, in the second volume of the work; and in other respects we find nothing novel except, perhaps, the following suggestion. Prolapsus of the uterus frequently impedes more or less the functions of the bladder and urethra, especially during pregnancy, and the emission of urine is greatly facilitated by passing the finger through the vagina, behind the symphysis pubis, the uterus is thus pushed back, and the compression it exerted diminished or even completely removed: the patient can herself readily practice this manœuvre. A frequent desire to pass urine commonly attends displacement of the uterus, pregnancy or simple hypertrophy of the organ; the patient may be compelled to pass water fifteen or twenty times in the night; M. Lisfranc has repeatedly relieved this symptom by directing a small enema, nearly cold and containing three or four grains of camphor, dissolved in yelk of egg, and a few drops of tincture of opium to be injected night and morning. (pp. 428-9.)

Retroversion of the uterus. This disease M. Lisfranc says was known to the ancients. But he adds, repeating the assertion of Sabatier and Boyer, it was first clearly described by Gregoire. We believe, on the contrary, that William Hunter, who is not mentioned by M. Lisfranc, was the first who published a case of the malady with a precise detail of the symptoms. (p. 431.) Boyer and others say that retroversion seldom occurs except during the early period of pregnancy; but this, according to M. Lisfranc, is a great error, the most frequent cause of the displacement being hypertrophy of the posterior part of the uterus. (p. 432.) The author has frequently met with cases simulating retroversion, but yet very distinct from it. In those cases the fundus of the uterus was perhaps inclined slightly backwards; the posterior surface of the organ was considerably hypertrophied; and the cervix being considerably congested, as in the third or fourth month of pregnancy, was elevated and applied against the symphisis pubis, but without losing its parallelism to the axis of the pelvis, while the rest of the womb in contact with the sacrum, and raised in consequence of the hypertrophy, lay in the same direction; consequently the fact of the cervix uteri being thrown very considerably forwards is not a proof of the existence of retroversion. (pp. 433-4.) Tumours behind the womb may elevate its cervix and depress the functions, and though the author has always found the womb in such cases slightly hypertrophied, yet we can always determine the nature of the case by ascertaining the condition of the cervix uteri. In fact the cervix uteri is never obliterated by a tumour external to the womb, as it is in pregnancy and in considerable hypertrophy of the organ, it is found in these cases in the same condition as in the third or fourth month of pregnancy; if then we have this condition of the cervix, coupled with a large tumour and absence of the signs of pregnancy, the tumour is external to the uterus. (pp. 435-6) When retroversion is caused by hypertrophy, it is difficult and dangerous to restore the uterus to its natural position; but if we remedy the hypertrophy the retroversion often disappears. (p. 438.) In some cases of difficult reduction, Evrat succeeded by passing a slender cylinder of wood into the rectum, and pressing the fundus uteri upwards, while the cervix was depressed with two fingers in the vagina; M. Lisfranc has effected the reduction by passing a cylinder, of less diameter than that used by Evrat, up the vagina, and therewith elevating the body of the womb, its neck being at the same time pushed downwards and backwards with one finger. (pp. 441-2.) If adhesions exist between the neck of the uterus and the vagina, the displacement is generally supposed to be irremediable; but M. Lisfranc has divided those adhesions in several instances. In one case there was a slight interval between the cervix uteri and the vagina, and the cicatrix was divided with a blunt scissors directed on the index-finger; in two other cases the neck of the womb and the vagina were in close contact; the cervix uteri was seized with Museax's hook, and depressed until it projected externally to a sufficient extent, (a condition indispensable to the performance of the operation): in order to avoid perforating the rectum, bladder, or peritoneum, the dissection was performed very slowly and cautiously, and especial care was taken to carry the incision exclusively through the tissue of the uterus, but as close as possible to the adhesion; when the uterus was set free, the small portion of the uterus left adhering to the vagina was removed with a scissors curved on the flat. (pp. 433-4.) When retroversion of the uterus is complicated with pregnancy, and reduction is impossible, it may be necessary to puncture the uterus notwithstanding the danger of such a proceeding; M. Lisfranc prefers, with Viricel, performing this operation through the anterior wall of the rectum, as we thus penetrate the uterus at its most depending point; if, however, the puncture is practised through the vagina, we should perforate the body of the womb, and not the cervix, as this latter part is too thick and hard. (pp. 445-50.)

Anteversion of the uterus. This is universally stated to be greatly rarer than retroversion; but M. Lisfranc, on the contrary, maintains "from hundreds, he might say thousands of observations," that it is *infinitely* more frequent, a circumstance which is, he thinks, easily explained. In common with Boyer, Dugès, and others, (to whom he does not refer,) the author attributes the affection chiefly to the weight of the anterior parietes of the uterus being increased from hypertrophy. But the anterior surface of the uterus is more exposed than the posterior surface to injury, and therefore it is much more frequently hypertrophied; moreover, females pass urine very frequently, and thence the bladder being often empty the summit of the uterus readily falls forward, the displacement is further facilitated by the great prevalence of constipation in females, and the consequent accumulation of feces in the rectum. (p. 453.) As to the symptoms enumerated by M. Lisfranc, we shall only advert to one not generally sufficiently dwelt on: "The patients very frequently experience considerable uneasiness in the rectum; on examination it is ascertained that the rectum is perfectly healthy, but nevertheless it is often by no means easy to convince the patient that such is the case; they usually persist in saying that the womb is not affected, ***that the disease is situated in the anus.***" (pp. 455-6.) We mention this the rather that more than one case has fallen within our own knowledge, in which certain of the tribe of rectum-doctors, far from trying to disabuse the patient of her erroneous idea, have, we must hope through ignorance, confirmed her in it. Complete anteversion is extremely rare; M. Lisfranc during a post-mortem examination met with one case, in which the capacity of the urinary bladder was diminished to a twentieth part of its natural size, and the fundus of the uterus was rotated forwards and downwards to such an extent that it had become nearly parallel to the axis of the pelvis. (p. 457.) The displacement is generally easily reduced, but occasionally great difficulty is experienced in accomplishing it. In one case where the operation seemed impossible, M. Lisfranc effected it by means of the cylinder of wood mentioned in the last paragraph. (p. 459.) A bandage or belt applied over a pad placed on the hypogastrium has a powerful influence in diminishing the pain caused by anteversion, and in facilitating its reduction even when the uterus does not rise above the symphysis pubis. After reduction a pessary, if it can be borne, may be useful. (p. 460.)

Inclination or obliquity of the uterus. This may occur when the womb is empty, or when it is distended either from pregnancy or a tumour. The presence of the sigmoid flexure of the colon and of the upper portion of the rectum at the left side and the prevalent habit of sleeping on the right side, render right lateral version the more common; the most frequent causes of the displacement when unconnected with pregnancy are, lateral hypertrophy, adhesion of the cervix uteri to one side of the vagina, and partial atrophy of the uterus occurring about the critical period of life. Besides causing leucorrhea, &c. this affection may prevent impregnation. The prognosis is favorable, except occasionally when acute inflammation exists. Reduction is easily effected, unless it is obstructed by the existence of adhesions or of hypertrophy, and a pessary frequently retains the uterus in situ. The indications of treatment may be collected from what has been said respecting other displacements of the uterus. (pp. 461-70.)

Anteflexion and retroflexion, or incurvations of the uterus. Incurvation of the uterus occurs rarely; M. Lisfranc has met with but six cases of it. We do not find anything added to what has been already stated respecting these affections by Denman, Deneux, Boivin and Dugès, and L'Heritier.

On the mechanical means of treating displacements and enlargements of the uterus. Sponges. Astringent injections are frequently employed concurrently with the various mechanical means destined to remedy displacements or hypertrophy of the uterus: M. Lisfranc has almost uniformly found such injections hurtful, when the patient was not much debilitated even though the parts were not inflamed; but they are, on the contrary, useful in relapsed languid constitutions, notwithstanding the existence of a slight amount of local inflammation. As a general rule the author prefers emollient injections. The introduction of sponge usually causes a good deal of uneasiness at first, though the genital organs may be quite free from inflammation, but nevertheless it should not be too hastily removed as the parts generally became accustomed to its presence. Neuralgic pains are very often removed by the employment of sponge, which should therefore be empirically applied when the existence of neuralgia is suspected. Sponges are useless in prolapsus uteri; they occasionally succeed in anteversion and retroversion; but are especially suitable in slight procidentia of the womb, and in prolapsus of the vagina, in which affections they often answer when pessaries fail. (pp. 475-82.)

Pessaries are employed to support a hypertrophied uterus, and to aid by the slight pressure they exert in dispersing the hypertrophy, (p. 475;) they are also fitted to counteract various displacements and relaxations of the uterus and of the vagina. Caoutchouc pessaries are greatly preferable to those manufactured from any other material except when we want to exert pressure on an hypertrophied uterus, in which case an ivory instrument is the best. (p. 499.) Pessaries are indiscriminately rejected by some practitioners, and as indiscriminately applied by others. The following are the rules, according to M. Lisfranc, which should govern their employment. They are contraindicated: 1st. When the patient expresses great repugnance to their use, (unless the circumstances of the case forbid delay,) because mental uneasiness exerts a most prejudicial influence on diseases of the uterus. 2d. When the genital organs are unusually sensitive or the seat of much pain, except in certain cases of neuralgia which are cured by the application of a pessary. 3d. When the instrument excites much pain, which sometimes does not happen for some time after its application, and is then almost uniformly due to the aggravation of a previously existing, but latent inflammation. 4th. When chronic vaginitis or metritis are present, and also for at least three weeks after they have been completely dispelled. 5. When the vagina or cervix uteri are ulcerated. Leucorrhea does not contraindicate the use of a pessary, except when it is complicated with chronic inflammation. (pp. 486-90.) Some practitioners recommend that the patient should be made to cough, sit, or even walk, in order to ascertain whether a pessary has been efficiently applied, but this is very injudicious, as being likely to aggravate the pain so commonly experienced during the first few days the instrument is worn; the patient should, on the contrary, be restricted to the recumbent posture for at least five or six days. When a large pessary is requisite, it very frequently cannot be tolerated if applied at once, but by first introducing a small instrument, and at suitable intervals replacing it by others of larger size, one of sufficient magnitude may ultimately be borne. (pp. 494-5.) Nothing can be more mischievous than to leave a pessary in the vagina for any considerable period; it should be removed every fifteen or twenty days, and after being thoroughly cleaned, immediately replaced. (p. 498.) There is a separate chapter *On the bad consequences resulting from the presence of pessaries in the vagina*, when they have been allowed to remain too long in that canal. On this chapter we need only observe that M. Lisfranc disapproves of removing a pessary during the existence of any considerable inflammation that its presence may have excited, as the manipulations necessary for its extraction, always aggravate the mischief: the inflammation should be reduced before the foreign body is extracted. (p. 510.)

We pass by the short and unimportant chapter on *Elevation of the uterus.*

Preternatural fixity of the uterus. An examination per vaginam will not always enable us to detect this affection; M. Lisfranc has not unfrequently met with patients in whom the uterus apparently retained its natural mobility, but on attempting to depress that organ for the purposes of operation, it was found to be quite immoveable; on the other hand, in cases where the womb when examined by the finger, seemed immoveably fixed, the cervix uteri was readily depressed so as to protrude through the vulva. (pp. 516-17.)

Preternatural redness, excrescences (*boutons*), *aphthæ, granulations of the cervix uteri.* Preternatural redness must not be confounded with the increased redness of the cervix uteri that exists during pregnancy, and for some days before and after menstruation. It occurs either in discrete or confluent patches, which are usually slightly prominent, or it may assume a punctated aspect resembling leech-bites; it may be superficial, or extend to some depth, and when limited to the surface, disappears momentarily on pressure; it more frequently occupies the posterior lip of the cervix, and usually occurs in adults before the critical period of life, after which it is rare. This affection is usually caused by congestion of the uterus, or by the contact of irritating liquids, whether secreted or introduced ab externo; it is usually permanent, but is sometimes fugitive; it may or may not be complicated with hypertrophy or inflammation, and when it exists for a considerable period, it commonly terminates in ulceration. The cervix uteri may seem perfectly healthy to the touch, when affected with preternatural redness, or even when eroded or superficially ulcerated; whenever therefore leucorrhea, anomalies of menstruation, &c. exist, the speculum should be applied, the red patches are then easily recognized, and in order to ascertain whether their surface is abraded, the part should be firmly wiped with a dossil of lint; if it is not stained with blood, there is no excoriation. . . . . Excrescences (boutons) often coexist with preternatural redness, or with hypertrophy of the cervix uteri; if neglected they may ulcerate, suppurate, or coalesce and form a tumour: they almost uniformly yield to antiphlogistics, combined with and followed by narcotics. . . . . When morbid redness is not complicated with inflammation or congestion, or when these complications have been dispelled, it should be cauterized with protonitrate of mercury, otherwise it may terminate in ulceration. A few cauterizations almost always suffice. The caustic should be applied with a camel-hair brush, which after being immersed in the solution, should be shaken or even gently wiped, so that a very minute portion of the fluid shall adhere to it; the neck of the uterus having been wiped dry through the speculum, the brush is once only, and as lightly and rapidly as possible passed over about one fourth of the diseased surface, and the speculum then filled with nearly cold water. When the redness is disseminated in isolated patches, it is sufficient to cauterize the entire surface of a single patch, provided it occupies about a fifth of the cervix. The treatment of granulations is exactly similar. (pp. 519-32.)

Benign ulcers of the uterus. Though ulcers are infinitely more common on the external surface of the cervix they occasionally occupy its inner surface, and cannot be then seen through the speculum, unless we dilate the cervix by gently opening the blade of a forceps previously passed into it. (p. 534.) According to M. Lisfranc, the affection is equally frequent at every age between the first appearance and ultimate suppression of menstruation, and its occurrence is not influenced by any peculiarity of temperament or of constitution; leucorrhea probably predisposes to the disease; and in hypertrophy of the womb, the existence of ulceration is the rule, its absence the exception. Displacements of the womb, a conical cervix, and painful menstruation are frequent causes of ulceration, and M. Lisfranc is satisfied that it is an hereditary disease; to this last opinion he attaches the utmost importance, because the simple, if neglected, is exceedingly prone, he says, ultimately to become malignant, and then the patient labours under cancer which is regarded as the more formidable and hopeless, because it is supposed to be of hereditary origin. Cancer, M. Lisfranc insists, is never hereditary, but simple ulceration, on the contrary, very frequently is; if an ulcer, indeed, is neglected, it may and often does, become cancerous, but it is always the ulceration, never the cancerous degeneration, that is hereditary. (pp. 535-8.) We pass by the description of the local appearances of ulcers of the uterus, which are very variable indeed. Their progress may be rapid, but is usually slow; the author has seen them remain stationary for ten years. They have little tendency to cicatrize, for though they often go on healing very rapidly for some time, the process is after a time arrested. Cicatrization is greatly slower in winter than in summer. (p. 540.) Some superficial ulcers bleed with the greatest facility; simply exposing them to the air, though the speculum does not touch them, causes blood to exude from their surface; this symptom indicates congestion of the womb, and though it usually yields to venesection and cauterization with protonitrate of mercury, still its presence gives grounds for apprehending the formation of a varicose, or a fungous, or an erectile tumour, or a fungus hematodes, for the author uses those four phrases to designate the degeneration which he says may follow this particular condition of ulcer. (pp. 541-3.) Simple ulcers of the womb occasionally simulate cancer so closely, both in appearance and in all their symptoms that the diagnosis can only be established by the result of the treatment. In other cases again, their progress is insidious and almost latent; there may be slight leucorrhea unattended with pain, or a sense of weight, and some trifling pain in the pelvis, there is no emaciation, no loss of appetite, and if an examination with the finger and the speculum is neglected, incurable cancer often intervenes. M. Lisfranc has seen several cases, which, in common with several very eminent surgeons, he pronounced to be incurable cancer of the uterus, and yet they perfectly recovered without any treatment save attention to cleanliness and observance of hygienic rules; he is inclined to think that ulcers of suspicious character, when they occupy an organ very liable to cancer, are too frequently assumed to be cancer; the odour *sui generis* of cancer is the only characteristic sign of the disease, but it is not constantly present, and sometimes never exists throughout the entire course of the most genuine cancer. (pp. 543-9, 553-8, 620.) The existence, the complete absence; or the peculiar character of the pain is of no value as a diagnostic sign; in simple ulcers the pain is often lancinating (p. 545), while cancer may run to a very advanced stage without more than a trifling amount of suffering having been sustained. (p. 567.) Non-malignant ulcers of the neck of the uterus are almost always curable. Congestion, induration, or hypertrophy of the womb retard the cure. The treatment usually occupies from three to six months, but may require a much longer period. (p. 539.) The general principles of treatment recommended by M. Lisfranc are, to remove congestion or inflammation when present, by rest, laxative enemata, narcotics by the mouth, rectum, or endermically, cold cataplasms on the abdomen, emollient vaginal injections, regulated diet, warm baths, venesection, and such other antiphlogistic treatment, as the circumstances of the case may require. Cold, whether applied locally or by general baths, and warm hip-baths are uniformly prejudicial, inasmuch as the former favour internal congestion, and the latter promote an afflux of blood to the pelvis. Immersing the upper extremities in hot water exerts, on the contrary, a very salutary derivative effect from the diseased organ. (pp. 557-68-71-4.) When congestion and inflammation have been dispelled, or when they do not exist, tonic and astringent injections, such as infusion and decoction of cinchona, oak-bark, rhatany, &c. or solutions of lead, zinc, or alum, may be thrown up the vagina. Pessaries are almost uniformly prejudicial when ulceration exists. Iodide of potassium administered internally is generally very advantageous when inflammation is either absent or very moderate, and is especially useful when the patient is debilitated. Bitters are often beneficial, as is iron also, when the blood is impoverished, but iron should not be continued too long, for though it invigorates the system, and restores the function of the stomach in particular, yet its protracted use almost uniformly causes dangerous congestion of the womb. (pp. 570-7.) But though these means may succeed, ulcers of the uterus, whether recent or old, seldom cicatrize, unless they are cauterized; caustic should not be applied when inflammation or congestion exists, except in those rare cases where the ulcer continues to extend, notwithstanding the employment of antiphlogistics and narcotics, or when an ulcer of bad character remains three or four weeks without any appearance of amendment. As a general rule, the caustic (protonitrate of mercury) should be applied in the manner described in the last section; but if the ulcer is deep, is covered with very luxuriant granulations, or has a very hard base the caustic should be applied more freely, but in no case should dossils of lint impregnated with the caustic solution, be left in contact with the ulcer for several minutes, as some recommend, for serious and too often fatal consequences may result from such practice; caustic should not be used for five or six days previous and subsequent to menstruation. It should usually be repeated every eighth day, but if the sore is indolent, its base hard, its surface granulating luxuriantly, the operation may be repeated twice or even thrice weekly. As cicatrization advances, the intervals may be prolonged, but if the ulcer begins to spread or remains stationary, the treatment must be resumed. However extraordinary it may appear, cauterizing one ulcer when several exist usually exerts a favorable influence on all. (pp. 581-97.) When copious leucorrhea or hemorrhage are running down the patient, or when the ulcer is spreading rapidly, particularly when it threatens to extend to the vagina, where it can scarcely ever be safely attacked either with caustic or the knife, and when the caustic proves inefficient, amputation of the neck of the uterus is indispensable. This practice was recommended by Dupuytren, and has on several occasions been successfully adopted by M. Lisfranc. (p. 557.)

Cancer of the uterus. Cancer of the uterus very seldom exists without ulceration. According to Bayle, it almost invariably commences as a simple ulcer, and M. Lisfranc entirely coincides in this opinion.

Occult cancer, which is fortunately a very rare disease, occurs indifferently on the cervix or on the body of the uterus, and it is very difficult or rather impossible to distinguish it from other indurated tumours of that organ, for lancinating pains, great hardness, a mammillated surface, &c. and the same train of general symptoms may equally attend both; and further, not only do simple indurations tolerably often present all the phenomena of *occult cancer*, but this latter affection occasionally appears under the most benign aspect: the diagnosis is therefore impossible, and from this circumstance, coupled with the fact that occult cancer of the uterus is an extremely rare disease, the treatment should always be grounded on the assumption that the tumour may be curable. If fluctuation is detected in the neck of the uterus, the tumour should always be allowed to open spontaneously, unless the violence of the symptoms forces us to evacuate the fluid by an incision; for if we unfortunately cut into an occult cancer, from that moment the patient becomes a victim to all the miseries of open cancer, which also uniformly runs an extremely rapid course. (pp. 597-609.) As already stated in the last paragraph, simple ulcers of the uterus may be attended with every symptom of open cancer, except the odour *sui generis* of the latter disease. (pp. 603, 621.) M. Lisfranc's opinion as to the age at which cancer of the uterus most frequently occurs is, we believe, peculiar to himself. He believes that it is most liable to affect females between the ages of eighteen and thirty-five, and that the general opinion that it most commonly sets in about the period of the cessation of menstruation is decidedly erroneous; he avers that he saw a girl aged fifteen affected with cancer of the cervix uteri. (p. 610.) This statement seems only to relate to cancer supervening on a simple ulcer, for we are elsewhere informed that the author has never observed *occult* cancer, except in females past forty. (p. 598.) M. Lisfranc, as we have seen repeatedly, insists that cancer of the uterus is very rarely a primary affection, and here again reverting to this doctrine, he inculcates as a rule of practice, "that the slightest symptoms either indicating uterine derangement, or even appearing to be connected therewith, imperatively require that an examination per vaginam should be made, both with the finger and the speculum." (p. 613.) It is owing to the neglect of this precept that incurable cancer has not unfrequently been detected, when its existence was antecedently not even suspected, and it is such cases that have given origin to the alarming doctrine that cancer sometimes occurs in a perfectly latent form: M. Lisfranc is however convinced that there is no disease of the uterus, be it what it may, which is not uniformly and in every case attended with symptoms perfectly appreciable and distinctly indicating that the uterus may be diseased; whenever this is even *suspected* to be the case, an examination should be forthwith instituted, and M. Lisfranc is decidedly of opinion that since this practice has been somewhat generally adopted in Paris, incurable diseases of the uterus have become greatly less frequent. Formerly eight or ten females, scarcely one of whom had been previously examined, applied every week at La Pitié labouring under cancer of the womb; at present not more than two or three are seen in a month. M. Lisfranc seems to anticipate a period when malignant diseases of the womb will scarcely be met with, in consequence of the curable affection in which he believes they almost uniformly originate, being detected and treated in their early stage. When the ulcer is not too large, it should be treated as if it was curable; protonitrate of mercury should be applied in the manner already described, but more freely and more frequently; and iodide of potassium should be administered internally. Even in apparently desperate cases, where the use of caustic is out of the question, iodide of potassium should be prescribed, as it sometimes unexpectedly effects a cure, and still more frequently allays pain and retards the progress of the disease. Caustic sometimes alleviates, but more frequently exasperates the pain, and should not therefore be applied, if the suffering is great, and uncontrollable by narcotics. (pp. 623-8.)

Amputation of the neck of the uterus. It is well known that M. Lisfranc, in June 1834, presented a memoir to the Academy of Sciences, in which he recommended amputation of the cervix uteri in certain cases of cancer of that part, and gave a very flattering account of the success of the operation, alleging for example, that 84 out of 99 patients recovered, that hemorrhage seldom occurred, and when it did, was always readily restrained, &c. It is equally well known that M. Pauly (Maladies de l'Uterus, 8vo, Paris, 1836,) accused M. Lisfranc of deliberate falsehood, of suppression of truth, stating that 53 operations only had been performed, that several unsuccessful cases which had been treated in hospital were not recorded, that of 19 private patients, whom M. Pauly names, *one* only recovered, and *four* died within twenty-four hours, that in 12 patients the disease returned immediately, that of 9 patients operated on in his own presence, 6 had alarming hemorrhage, and 3 died in less than twenty-four hours; moreover, M. Pauly specified by name several individuals on whom M. Lisfranc said he had operated, though, in point of fact, no operation had been performed on any of them, and added much more to the same effect, which we need not recapitulate. Now M. Lisfranc never made any reply to M. Pauly's allegations, and we can scarcely say that he does so in the present work. We are indeed told that he never *published* his memoir; that it was a *secret* document intended to support his claims, when a candidate for election at the Institute; that it was obtained from the archives of that body, without his knowledge, and was shamefully misrepresented; that he has since *withdrawn* the memoir, but will not enter into explanations of the misrepresentations he complains of, because "professional honour imperatively forbids such a course. I would not for any consideration betray the secrets of my patients." (pp. 666-7.) M. Lisfranc, it would appear, thinks least said soonest mended, and it would have been better for his character to have fully acted up to that maxim, and not have alluded at all to this ugly business. His own version of the transaction comes simply to this. He knew that his memoir could never bear publication, but he hoped that it might, as a *secret* document, influence some members of the academy to vote for his election. After such a humiliating exposure of misrepresentation and fraud, we can of course attach but little importance to statements respecting this operation, but as rash operative interference is one of the besetting sins of the present day, it will be useful to show how greatly M. Lisfranc has modified his practice respecting this operation, though he would fain make it appear that his opinions have undergone no change.

M. Lisfranc says that amputation of the neck of the uterus may be attempted: 1. When well characterized cancer extends too deeply to be destroyed by caustic. 2. When the disease has not spread above the attachment of the vagina to the uterus. 3. When, though the existence of carcinoma is doubtful, the health is rapidly giving way in spite of treatment. (p. 636.) 4. Hypertrophy of the body of the uterus does not contraindicate the operation, unless the tumour is very hard, or about double the natural size of the organ. 5. Even if the tumour exceeds this size, yet if it is scarcely painful, the cancer of the cervix may be removed, as the operation affords a chance of recovery, seeing that the body of the uterus is often only affected with simple white induration. (pp. 637-8.) The fact stated by Bayle that the neighbouring lymphatic glands and adjacent parts are much less likely to become affected in cancer of the uterus, than in cancer of any other organ, is a circumstance very favorable to the success of the operation. (pp. 609, 638.) 7. As is also the fact that cancerous degeneration often affects but a limited portion of the tumour. (p. 638.) 8. Moderate enlargement of the ovaries does not forbid the operation. 9. Neither does the existence of enlarged lymphatic glands, provided they are recent, few, and not too bulky or hard. (p. 639.) When the disease has been but partially removed, the portion left behind has sometimes been cauterized, but this proceeding is usually fatal, and M. Lisfranc was thence induced in several cases to dissect out the morbid structure from the parietes of the womb, and he avers that he in this way succeeded in several cases. (pp. 631-42-49.) Hemorrhage, we are told, rarely follows the operation, and when it does is always easily restrained by plugging; the plug should not, however, be too hastily applied, as a moderate loss of blood tends to prevent metritis, unless the patient was very feeble, or the hemorrhage sudden, M. Lisfranc would not plug until twelve ounces of blood were lost. (pp. 651-2.) The wound is often cicatrized in six or eight weeks, but may not heal for several months. The operation in no way interferes with utero-gestation; parturition is attended with less difficulty than previously, and the patient menstruates as usual. (p. 659.) From the foregoing abstract of the most important statements made by M. Lisfranc respecting this operation, it might be inferred that it is rather frequently indicated, but this we are told is not the case; M. Lisfranc himself now performs it very rarely, not it appears from any distrust in its safety or efficacy, but simply because cancer of the uterus is much less frequent now than it was a few years since. We trust that we need not occupy space in exposing this miserable and uncandid subterfuge.

The sections on *extirpation of the uterus*, (pp. 667-75,) on *contraction and obliteration of the Fallopian tubes*, (pp. 675-7,) and on *inflammation of the ovaries, of the Fallopian tubes, and of the ligaments of the uterus*, (pp. 677-91,) do not contain anything requiring to be noticed here, and with respect to the chapter on *ovarian dropsy*, (pp. 691-720,) we find but one point necessary to advert to. M. Lisfranc disapproves of extirpating ovarian cysts. Unless the tumour is very moveable, we have no reason to assume that extensive adhesions do not exist, but this character of mobility cannot be satisfactorily detected unless the tumour is very small; and if the tumour is very small, experience tells us that it may remain stationary, that its progress may be very slow, and that even should it increase greatly, the patient may nevertheless sometimes live a long time without enduring much suffering; against these *chances* we have to balance some *extraordinary* rare cases of successful operation, which, in M. Lisfranc's opinion, do not turn the scale, an opinion in which we most fully coincide. (p. 711.)

On some tumours adjacent to, but not connected with the genital organs. Cancer high up the rectum may push the uterus forwards, and if we confined ourselves to an examination per vaginam, we might suppose that the patient was merely affected with displacement and hypertrophy of the womb; but the true nature of the case is at once detected on passing the finger up the rectum. (p. 720.) Tumours, whether the result of inflammation or of any other cause, occasionally form external to the womb; they often acquire a considerable size, cause extreme pain, and are accompanied with almost all the symptoms of disease of the uterus. In most of those cases, the diagnosis is extremely difficult, not to say impossible, for the tumour is confounded with and seems to be a part of the uterus; but M. Lisfranc has observed that when the womb is *considerably* enlarged, the cervix is almost uniformly shortened or even effaced; whenever then the neck of the uterus retains its natural length, there is reason to believe that the tumour is external to the womb. (pp. 721-2.) Diseases of the womb are frequently attended with exquisitely acute pain, which cannot be allayed by the most powerful narcotics or other remedies; in such cases magnetism is a potent means of relief: "far be it from me" says M. Lisfranc, "to admit the reveries of the magnetisers; but beyond all doubt the influence of mesmerism on the nervous system of females thus circumstanced may be extremely salutary; I am satisfied of this from the result of numerous cases; I have seen the pains disappear as if by enchantment." (pp. 724-5.)

Some considerations on hemorrhoids, and on their treatment. M. Lisfranc has scarcely ever seen hemorrhoids consisting of erectile tissues, and he thinks they are very rarely formed by dilated veins. He does not give a very clear explanation of what he conceives to be their usual structure, but seems to regard them as hypertrophied cellular tissue, either congested or infiltrated with blood. (pp. 731-3.) When hemorrhoids produce very considerable distress either from being irreducible or descending and causing much pain every time the patient goes to stool, and the ordinary palliative treatment fails to give relief, M. Lisfranc recommends the patient to be bled at the arm, one or several times, according to his strength, state of health, &c., and then to direct a shower-douche of tepid water on the anus; this is to be replaced after a time by a similar douche of Barege water, and finally by a douche from a single jet, which, on occasion, may be passed up the rectum; "these means are heroic; I have seldom seen them fail, and we are thus enabled to avoid the performance of a dangerous operation." (p. 739.) When compelled to excise hemorrhoids, M. Lisfranc proceeds thus:—Having fixed the tumour with a hook, he circumscribes it with two semilunar incisions, and then, instead of cutting it away with one or two strokes of a scissors, he dissects it slowly off by several successive incisions; as the parts are kept down, any vessels that may be opened can be easily twisted or tied, and before completely detaching the tumour, we should ascertain whether an artery runs in the portion still adherent, by grasping it between the finger and thumb. The operation should be terminated very deliberately, and but a minute portion of the tissues be divided at once. The author says that by proceeding in this way, we incur no danger of hemorrhage. (pp. 743-4.)

Our readers are now in a condition to form a fair estimate of the merits of the present volume. We have been very sparing of comment, because we preferred curtailing our own observations to omitting anything that seemed important in a work purporting to contain the matured experience of a practitioner of great reputation, and who has enjoyed vast opportunities of observing diseases. That the present volume contains much that is valuable, we do not pretend to deny, if it did not we should not have occupied so much of our time and space with it; but it is disfigured with numerous and unpardonable faults. Nothing but a perusal of the original could convey an idea of the inflated and exaggerated laudation of self, and of the acrimonious depreciation of others that pervade the entire book. M. Lisfranc, to believe himself, is the great regenerator of modern practice; others are the blind slaves of an antiquated routine; he alone has laid the foundation of the true precepts of scientific surgery, by combining in harmonious union anatomy, physiology, and pathology, and being the first to conjoin medical and surgical therapeutics in the treatment of disease. The author also indulges in the most ludicrous over-estimate of the merits of the most trifling point that he conceives to originate with himself. We believe we have extracted almost every prominent statement in the volume, and the profession can judge how far the author overrates his own achievements.

Art. II.

Report of the Commissioners appointed to take the Census of Ireland for the year 1841. *Presented to both Houses of Parliament by Command of Her Majesty.—Dublin*, 1843. Folio.

It is not our intention to present an analysis of all the matters comprised in this very valuable and voluminous statistical survey of Ireland, comprising ample and (excepting where circumstances precluded perfect accuracy,) precise information on the domestic, social, and sanitary condition of her population. We find, however, so much medical and historico-medical information in the 'Report on the Tables of Death,' from the pen of Mr. Wilde, which it comprises, that we consider that we should be failing in our duty as journalists did we not furnish some view of it to our readers.

The materials whence his remarks are derived are ample, dealing as Mr. Wilde does with the mortality of Ireland for the ten years ending on the 6th of June, 1841—a mortality of about 1,187,374, and probably even exceeding this.

Of this general mortality, 911,619 occurred in the open country, including minor towns and villages; 234,173 deaths took place in towns; and 41,582 deaths in the different hospitals and sanitary institutions. The proportionate deaths in the two sexes were 100 males to 91·24 females in the general mortality, with some variations in these proportions in the different districts.

We are assured, however, that the 1,187,374 are not all the deaths that occurred in Ireland during the ten years specified; and it can only be conjectured what proportion of the total decennial mortality they constitute, for there existed no proper annual registration of the kingdom.

They appear, according to Mr. Wilde's opinion to be *minus* about one fourth. He remarks that, taking an average population between the census of 1831 and 1841, and the average of the entire deaths returned upon the present inquiry for that period, we find the annual proportion of deaths to be 1·48 per cent., or one death in every 67·13 (we would say 67·56,—Rev.) of the population. Supposing, then, the actual annual mortality in Ireland to be 2 per cent., or 1 in 50, in round numbers, then these deaths are, as a whole, *minus* about one fourth. The statement of the census commissioners on this inaccuracy show it to be very considerable indeed. In 1834, in a population of 7,943,940, the mortality returned was 26,623, being 1 to 82·2 or 1·22 per cent. There can be no doubt that this is very incorrect; and the inaccuracy becomes very glaring indeed when we find that in 1839, in a population of 8,092,947, the deaths were 140,239, or 1 in 57·7, or, in other words, 1·73 per cent.; and that in 1840, the population being 8,133,934, the mortality was 141,536, or 1 in 57·5, or 1·74 per cent. The commissioners propose the same corrections of the inaccuracy as Mr. Wilde, and on the following grounds;

> "In propositions founded on numbers so imperfect, we may take the last as the most correct. Still, as the errors of voluntary returns are naturally corrected by being spread over a number of years, it is probable that the most satisfactory result will be obtained by using the average population and average deaths of the years from 1836 to 1840, which we believe to be of very equal value; thus: 1839, population 8,052,170, deaths 135,359, average mortality 1 in 59·5, or 1·68 per cent.; and as there can be no reason for supposing the mortality less than 1 to 45—on the contrary, many circumstances would appear to place it higher—we are led to apprehend a deficiency in the death returns amounting to about one fourth.
>
> "If we apply this correction to the total number of deaths, we shall have the following data for computation:

Population of 1831 . . .	7,767,407	
Deduct the army and followers then in Ireland	29,486	
		7,737,915
Add births between 1831 and 1841 . . .		2,496,826
		10,234741
Deduct deaths per table . .	1,187,374	
Ditto, increased by a fourth . .	296,843	
		1,484,217
Total		8,750,524

> "This total would be the computed population for 1841, and the difference between it and that given by enumeration—viz. 575,400—would remain to be accounted for by emigration or other causes. This number will be found to agree very nearly with that taken as the amount of the subtraction from the population between 1831 and 1841, at which we arrived by independent and totally different means.
>
> "These calculations, however, interesting as they may be, are inserted here, not so much for their own value (not being based on numbers positively ascertained,) as to show how deficient we are in the data requisite for the correct deduction of the value of life assurances and similar subjects in Ireland, which recent information and better materials in other countries, more especially in England, have shown to vary in different circumstances and conditions of life, to an extent wholly unknown before." (Census Commissioners' Report, p. xliv.)

A sketch of the history of Irish medicine, which will be read with interest, serves as a preface to Mr. Wilde's nosological table and classification of the enormous mass of materials he has to deal with.

"Herb-doctors," it seems, existed in Ireland during the last and beginning of the present century, and some of them acquired reputation even in the metropolis. These persons acquired their information from Irish medical MS. productions of the middle ages, many of which are still extant in the Royal Irish Academy and the King's Inns Library, Dublin. Mr. Wilde enumerates and describes several of them. They bear a considerable resemblance to the works of those persons who, in various parts of Europe, have been called *Arabistæ*, men who in the middle ages occupied themselves with translating, commonly into Latin, but sometimes into the vernacular of their country, and commenting upon the writings of the Arabians, such as Mesue, Haly Abbas, and Avicenna, and whose knowledge of Greek and Roman medicine reached them through Arabian channels. Their works, or considerable portions of them, are to be found in the collections of Luisinus and Gruner, especially of the latter. England owned some of them, as the 'Rosa Anglica' of John of Gaddesden, (Professor of Merton College, Oxford,) written about 1305, proves. Such works seem to have abounded in Ireland, showing the love of letters for which her highly-gifted population has ever been distinguished; for though many are mentioned by Mr. Wilde, he expressly states that he has specified only those from which he has derived his Irish nosology, (so valuable and interesting a feature in his Report,) but he has not given a catalogue of any portion of Irish medical literature—thus indicating the great amount of the whole stock. We consider the circumstance of the Irish herb-doctors deriving their information from the medical literature of a *bygone* day confirmatory of an opinion we have ever entertained, that popular medicine is the cast-clothes of the profession; the public move in the same track as ourselves, but at a great distance behind. Mr. Wilde says:

> "During the fourteenth and fifteenth centuries the Irish possessed considerable knowledge of botany and the medicinal virtues of herbs and plants. [This was probably the period of the labours of the Arabistæ.] Boate and Sir William Petty followed in the seventeenth century; the former added in his works the knowledge of his brother, then physician-general to the English forces in this country. In the seventeenth and beginning of the eighteenth centuries some distinguished Irish scholars, Threkeld and Molyneaux in particular, availed themselves of the popular knowledge of the Irish people with regard to the medicinal virtues of plants; and the former states that he was much indebted to the Irish MSS. for information on that subject. These writers were followed by K'Eogh, but he added little to the labours of his predecessors; and the principal materials of his work on botany and the medicinal virtues of plants are evidently derived from those of Threkeld, Boate, and Molyneaux. These were followed by O'Connell and Rogers, of Cork, and Drs. John Rutty, Bryan Robinson, Garret Hussey, and others, from the beginning to the middle of the eighteenth century." (Report upon the Tables of Death, pp. iii-iv.)

Of the Irish medical worthies of the eighteenth century here mentioned, one, Dr. John Rutty, has always impressed us by the worth and, at the same time, singularity of his character. Besides having published many works on natural history and medicine much esteemed in their day, he kept a diary, less of what he did than of what he thought and felt, during the last twenty-two years of his life, from 1753 to 1774. This diary was published after his death. It presents an example of the most stringent self-catechising ever witnessed, generally expressed in a tone so quaint and humorous as strongly to arrest attention. It has been quoted and praised by that distinguished lover of all curious reading, Sir Walter Scott. The author often accuses himself of having eaten "swinishly" or "piggishly." On other occasions he "was sinfully dogged and snappish." One complaint he makes with which the profession will be sure to sympathize: "Eight patients and never a penny." His love of natural knowledge he regards, if not with self-censure, certainly with jealousy: "Moderation in my calling is witnessed; may it be extended to my two darling objects, natural history and the materia medica." From some passages in the Diary the reader might be led to think the author "a self-tormenting sophist;" such, however, is not his character, as displayed by the general tenor of the work. This reflects the image of a man of great benevolence; one aiming incessantly at the improvement of his moral nature, and, in every misfortune that may overtake him, having ever a ground of consolation at hand. "I have laboured," says he, "considerably, but not suffered much: perhaps a little in the withholding of fees *for telling truth ;* but what then! this is only ridding of superfluous wealth, an incentive of luxury."

The classification adopted by Mr. Wilde in his statistical nosology is, with slight variations, the same as that of Mr. Farr in his Reports to the Registrar-general. Mr. Wilde's nosology consists of four columns. In the first are given the terms used in the abstracts; in the second, the synonymes, analogous diseases, popular and provincial terms; in the third, the Irish names; and in the fourth, the English translations of these names. The Irish names appear from the translation, our sole means of judging, to be very expressive, and, in some examples, boldly figurative. For instance, we have prolapsus uteri; Irish name, tuitim an mhaclaigh; translation, falling of the place of the son. The causes of death are arranged under ninety-four heads. To some of these we shall now advert, and shall select the twelfth head, fever—a subject of much interest in Ireland and elsewhere.

The maculated or spotted fever, the true *typhus Hibernicus*, is recorded in the early Irish mss. under the term of fiabrhus morgaighthe, or the putrid fever; and also fiabrhus righim, the lingering or low nervous fever. It is probable that this disease has always been endemic in Ireland; for Gerald Boate, in the seventeenth century, classes it among Ireland's *morbi endemii* under the name of the Irish ague, which all authorities now agree was the synochial and typhus fever. But the first defined period of epidemic fever in Ireland is that chronicled by Rogers in Cork and its vicinity, in 1708, and again 1718-21, and 1728 to 1731. These dates accord with the estimate of Mr. Wilde, that fever has raged nearly decennially in Ireland for the last 150 years. He does not intend to speak precisely; but would say, as a general rule, that fever has become epidemic from the eighth to the twelfth year, with an interval of from six to eight years; and facts during the eighteenth century establish his case very well, with the exception of two lapsed periods. Thus there was an epidemic in 1708, one in 1718-21, 1728-31, 1740-43, 1763-64, 1771-73, and 1817-21. The lapsed periods—that from 1743 to 1763, and that of thirty-four years, from 1773 to 1817—he seems to think may arise from deficiency of records for the period; admitting, however, that the accustomed visitations did fail in the periods mentioned, yet is the recurrence nearly at the decennial period sufficiently marked during much the greater part of an entire century to be, as he expresses it, curious and unaccountable; and we fully concur in the striking analogy he discovers between the course of epidemics and another natural phenomenon, as depicted in the language of Scripture: "The wind bloweth where it listeth, and thou hearest the sound thereof, but canst not tell whence it cometh and whither it goeth."

Though acknowledging fever to be the plague of Ireland, and from the period of historical record the most prevalent and fatal (epidemic) affection to which the country has been subject, Mr. Wilde very properly guards his readers from exaggerations founded on rough guesses. In the most fearful epidemic of the last century, that of 1740 and 1741, which was recorded by Rutty and O'Connell, the latter writer states the number that perished at 80,000; whilst the accurate Rutty says it was computed, *though probably with exaggeration*, that one fifth of the inhabitants died of fever. Again, in more recent times it has been asserted before a select committee of the House of Commons (in 1830,) that 65,000 died of fever in 1817. What reliance, asks Mr. Wilde, can be placed in such guesses, when it is discovered by statistics derived from sources much more accurate than former days possessed, that the mortality from fever was in ten years little more than 112,000, and cholera, in its three years' progress, carried off little more than 45,000.

One of the most appalling epidemics that ever invaded Ireland took place in 1817-18-19, and had Drs. Barker and Cheyne for its historians. In their report it was stated, that, "assuming the population of Ireland to amount to six millions, it will be no exaggeration to state, that a million and a half of persons suffered from an attack in the time included *between the commencement of the years* 1818 *and* 1819. In the course of the two years commencing with September 1817, more than 42,000 persons were admitted into the hospitals." During this epidemic the total number of persons admitted into the hospitals of Ireland (both temporary and permanent) was 100,737, of whom 4349 died.

This we consider a small mortality relatively to the number attacked; being but 1 in 23. The mortality was greater among males than females, in the proportion of about 60 to 51. In the mortality from fever during the decennial period embraced in the present report, (from June 1831 to June 1841,) it is somewhat curious to observe that the same proportionate preponderance of male over female deaths is observed, the proportion being 100 males to 86·14 females, or, within a very insignificant fraction indeed, the same as in the former case. During the ten years comprised in the report, the deaths from fever amounted to 112,672; and there was an epidemic of the disease of a malignant nature as an accompaniment to the influenza of 1836-1837.

Consumption, Mr. Wilde says, is by far the most fatal affection to which the inhabitants of Ireland are subject, exceeding the returns from fever by 23,518 deaths during the ten years. It is reported to have destroyed 135,590 of the population of those families from whom the returns were received on the 6th of June, 1841,—being to the deaths from all causes 1 in 8·75; to those of the entire class of diseases of the respiratory and circulating organs 1 in 1·31; to the total number of deaths from epidemic, endemic, and contagious diseases, as 100 to 281·17; and to fever alone as 100 to 82·65. The sexes are 100 males to 113·07 females, being the only one of this class of diseases, except asthma, where the females preponderate. The mortality from this melancholy cause appears from the returns to be very steady, for in 1836, the deaths were 14,603; in 1837, 16,078; in 1838, 15,481; in 1839, 16,673; in 1840, 16,126, and in the first half of 1841, 8201. He says:

> "*A vast multitude of deaths* occurring in extreme infancy, and during the very early periods of life, were returned as having been caused by consumption, decline and decay (the two latter being the most common terms for this disease among the lower orders); now, although it cannot be denied that tubercular phthisis may occur at any age, (instances are recorded of tuberculous matter, and even cavities, being found in the lungs of new-born infants); yet, as it is not a general disease of very early life in this country, it became necessary, in arranging the registration, to limit the age at which deaths were to be registered as having been caused by phthisis pulmonalis. That age has been made the 10th year, and all cases returned of an earlier age have been placed under a separate head, that of marasmus, No. 50 (which see, page xxxii), for the reasons there detailed, as being the best term under which to register cases of the tubercular disease of early life (*phthisis infantum.*") (Report on the Tables of Deaths, p. xxviii.)

When we turn to the section "Marasmus" referred to, we learn that the term is used as a generic name, under which to class all those various affections of infancy and early youth returned in the census papers as consumption (infantile), wasting, decay, decline, emaciation, general debility, loss of strength, &c., and not intended to apply to *tabes mesenterica,* or any other state of atrophy in particular. This arrangement became necessary from the number of deaths returned as *consumption* and *decline,* under 1 year of age, and from 1 to 10. The total deaths registered under *marasmus* amount to 68,650, or about one half the number returned under the head of phthisis pulmonalis; the sexes being 100 males to 105·81 females, as in the case of *phthisis,* the latter sex preponderating.

From what we observe in England, where genuine phthisis is by no means infrequent in mere infancy, and of very considerable frequency indeed under 10 years of age, we cannot help believing that a very considerable proportion of the mortality classed under the head of *marasmus* ought to have found a place under *phthisis*. We admit that from the mode in which the materials for the tables of mortality were collected, it would be impossible to define the precise proportion in which cases should have been referred to the head of phthisis; but we have a conviction that to have been accurate, it must have been considerable; and this conviction adds to our impression of the great prevalence of consumption in Ireland.

The Irish name of phthisis is very significant and expressive, *seilgean as*—a shrinking of one's self. *Cnai*, or *cnasidh,* (we wish Mr. Wilde could help us to the pronunciation of these names,) a wasting or decay with or without disease of the chest, is the Irish term for marasmus. When the disease presents itself in the form of *tabes mesenterica* or general atrophy, many superstitious notions are entertained by the peasantry respecting its probable cause, such as the child being under the influence of spirits or fairies, the "gentry," or the "good people;" and hence the term in such common use, "back-gone," or "fairy-stricken," more than one of the A. forms gave these returns, and one, "the fairies tuck him away." These superstitions, though not unpoetical, yet naturally suggest an inquiry into the amount of education-grants to Ireland.

Under the 87th head, or that of intemperance, we find historical information of much interest, and in one respect not unpleasing, for it shows that this disgusting and destructive vice is on the decline. The deaths in the register ascribed to it immediately or remotely amounts to 1239 or 1043 males, and 196 females. The evidence of the existence of this evil during at least two centuries abounds. Stanihurst and Dymmok allude to the practice during the 15th and 16th centuries. In 1724 the pernicious use of ardent spirits became so manifest in the city of London, that the College of Physicians there put forward a public manifesto detailing its calamitous consequences. "It is also to be observed," says the worthy Rutty, in his Natural History of the County of Dublin, "that with us in Dublin the use of spirituous liquors, nearly from the same fatal era (1724), began to prevail, and from thence to the present time hath gradually prevailed to an enormous degree." After expressing what appears to us rather a whimsical speculation than a sound conclusion,—though he asserts that it is founded on the baptisms in Dublin, for 41 years,—that the drinking of ardent spirits unfits men for the propagation of male progeny, though leaving them the power of producing females, the same authority informs us, that Sir William Petty (in the 17th century) estimated the number of alehouses in Ireland at one third of houses generally. Dr. Rutty gives the following enumeration of houses of this description in Dublin in 1749: of alehouses, 2000; taverns 300; brandy-shops, 1200;—total, 3500, being nearly Sir W. Petty's proportion for the whole country. The following are Mr. Wilde's sensible, and on the whole encouraging remarks on this subject:

> "This baneful thirst for intoxicating drinks appears to have gone on increasing to an extent that can hardly be credited (and will, it is to be hoped, be scarcely believed in future ages), from the days of Rutty to almost the present time; for when we sum together the deaths caused directly by the excessive use of spirits—the 77 deaths enumerated under the head of *delirium tremens*—the number of cases of lunacy that were induced by the same evil—and the vast multitude of accidents that have been the result of drunkenness,—then, I think, the loss of life caused by the use of ardent spirits, within the 10 years, cannot fall short of 2000, independent of the number of diseases induced, or rendered fatal, by its previous injurious effects upon the constitution, and the train of want, sickness, misery, and destitution, entailed upon the wretched families of those persons who were its unhappy victims. From the records furnished by the census inquiry, it appears that this description of death increased rapidly from the year 1831 to 1839. Happily, a new and better state of things has commenced, the effects of which had already begun to tell, in the two last years over which this inquiry extends; and, compared with the days of Rutty (1772), our improvement is evident, for, by the Dublin Police Report for 1841, it appears that there were only 880 public-houses and taverns, and 112 grocers who retail spirits by permission of the customs, which, with 106 unlicensed houses and 'grocers' shops, that 'retail spirits without a license,' amounting in all to but 1098, presents, in proportion to the population of the metropolis at the present day, an astonishing decrease in places of this description, since 1749. Including the cases of *delirium tremens* with the deaths

from intemperance, during the 10 years ending 6th June, 1841, it affords a proportion of 1 in 33·77 of all the violent and sudden deaths, and 1 in 902·25 of the general mortality of the kingdom from all causes." (Report on the Tables of Deaths, p. xli.)

In connexion with this subject, we are surprised not to see the immortal name of FATHER MATTHEW: but we suppose the dignity of an official report did not permit this.

On the subject of the comparative mortality of England and Ireland, Mr. Wilde presents some general remarks, which appear, to say the least, plausible, though founded rather on speculation than sound statistical grounds. Considering the larger proportion of the rural compared with the civic district in Ireland than in England, and the much greater comparative mortality in the population of the latter than the former, ranging, for instance, from 1 to 64 in the agricultural districts and the open country, to 1 in 28 in densely-populated manufacturing towns, such as Manchester, &c.,—he infers that the rate of mortality in Ireland must be less than in England. The annual proportion of deaths to the living in the latter country is, on the average of the period during which the registration act has been in force, 1 in 45; whilst, from an examination of the entire records of disease, death, and longevity, in Ireland, Mr. Wilde is led to believe that were an accurate registration effected, the *proportionate* annual mortality would not be found to exceed 1 in 52, or 1 in 53. All this sort of reasoning sounds, we admit, very plausibly; but we cannot help being impressed with the very different conclusion reached by the commissioners, who, in words which we have already quoted, state that there can be " no reason for supposing the mortality less than 1 to 45, on the contrary, many circumstances would appear to place it higher."

Founded on a table of proportionate mortality in each county and province of Ireland, he classes the counties in the following order: In the first class, which includes those of Tyrone, Down, Fermanagh, Londonderry, Donegal, Armagh, Antrim, Carlow, Kilkenny, and Sligo, the average proportionate mortality, or ratio of deaths to the living, in one year, is 1 in 62. In the second class, including the counties of Limerick, Mayo, Westmeath, Kildare, Kelly, Cork, Longford, Queen's, King's, Wexford, Wicklow, Cavan, Monaghan, Waterford, and Leitrim, the average is 1 in 57·5. And in the third class, including the counties of Clare, Dublin, Louth, Meath, Galway, Tipperary, and Roscommon, 1 in 51·7. In these calculations, the specified cities are not included; their mortality, with the exception of Galway, offers a remarkable similarity, varying only from 1 to 32·12 in Dublin, to 1 in 35·05 in Belfast.

There is a table of deaths in towns, (exclusive of the cities of Dublin, Kilkenny, Cork, Limerick, and Waterford, and the towns of Drogheda, Belfast, Carrickfergus, and Galway,) which, could the *data* on which it is founded be implicitly relied upon, shows admirably the effect of density of population on sickness and mortality. In towns of 2000 population, and not exceeding 5000, the mortality from all diseases amounted to 1 in 50·85; in towns above 5000, and not exceeding 10,000 to 1 in 45·24; and in towns above 10,000 to 1 in 43·08; whilst in the open country the deaths were 1 in 76·93. With the exception of a singular deviation in the case of smallpox, in the case of which disease the mortality in towns of the first class, or those of the smallest population, was less than in the open country, the deaths from special diseases observed the same ratio as the total deaths, the mortality in towns increasing in proportion to the population, and being very much less in the open country. We give fever as an example. The deaths from this cause in towns of the first class, were 1 in 532·4; in the second, 1 in 439·2; in the third, 1 in 366·2; and in the open country, 1 in 864·3. Nothing can manifest more strikingly the effect of increased density of population in increasing the rate of mortality; and even admitting, as the commissioners do, that there are errors in the numeration; yet, as it is presumable that these errors would be pretty nearly equal in the different localities, the table still retains its value.

Mr. Wilde furnishes a very lucid commentary on the "general table of deaths by years and ages," which is found at page 163 of the "Summary of Ireland." There took place, however, within the ten years 36·120 deaths of which the ages were not specified. But of 1,151,254 deaths, of which the ages were described, 269,199 took place during the first year of life; 435,117 during the first five years; and one half up to the 20th year or between it and the 21st. This he would regard as fixing the average age, which he arrives at by finding the mean period of a given number of deaths. For example, if, in 11,480 deaths in the county of Carlow, it be found that one half of the entire die under, or at the 29th year, then that becomes the average age or mean point at which death took place, and not, as in other statistical returns, by summing the ages and dividing by the number of deaths. Certain of the results thus obtained are certainly very extraordinary; for instance, the average age ranges from 6 in Drogheda, and 6 to 7 in Galway, two of the most unhealthy towns in Ireland, to 31 in Wexford county, the rural district of which, it has long been a matter of history, is one of the most healthy. We would remark that a table of the ages at which the total deaths (1,187,374) took place, which will be found at page xlvii of his Report, furnishes evidence of great longevity. We find, for instance, 25,877 deaths, male and female, at the age of 80; 3958 at 90; and 2445 at 100. It should be observed, certainly, that these are marked at the decennial period, when, as Mr. Wilde remarks, there is a predominance of deaths returned, as being of that age beyond the reality.

We now return to a subject of great moment, as well in this country as in Ireland—coroners' inquests. After much delay Mr. Wilde succeeded in obtaining a return of 20,334 deaths upon which coroners' inquests had been held. These deaths he arranges into six classes. Of these, the first comprises deaths caused by violence, neglect, evil intent, or design, of some second or third party, in short, homicides; the second, suicides; the third "accidental deaths, caused without intent or design;" the fourth, "deaths from natural causes," consisting chiefly of those taking place in gaols and prisons, or those occurring so suddenly, or under such suspicious circumstances as elicited judicial investigation; the fifth, deaths from the immoderate use of ardent spirits; and the sixth, those cases in which the cause of death was uncertain.

Of the eight specified varieties of death, included under the first class, those by murder amounted to 890, the majority of which took place in the rural districts. It is pleasing to observe that the returns show a progressive diminution of this horrible crime. During the first five years, from June 1831 to June 1836, the annual average number of deaths from this cause was 1008, and in the last five years 722, while in the last quarter period the two years and a half, ending the 6th of June, 1841, the number had decreased to 576 (annually). The proportion of males to females, for the whole period, was 47 to 1; but this number was seriously augmented by the marked preponderance of females in 1837, over all the other years; and this remark holds good with regard to each of the provinces. The months in which most murders occurred were December, January, March, and June, while April, July, and September were the least. Compared with the existing population the proportion of murders throughout the provinces was, in Ulster, 1 in 24,858; Connaught, 1 in 8496; Leinster, 1 in 8223; and Munster, 1 in 6191. This comparison is in the highest degree favorable to the north of Ireland. Mr. Wilde mentions the very curious fact, that of 119 homicides in the county and town of Galway, but 5 were returned as deaths by manslaughter; out of 434 homicides in the county of Tipperary, 333 were pronounced by coroners' juries to be manslaughter. Was the difference in the nature of the offences, or in the minds of the juries?

The inquest returns have specified six particular modes of death under the head of suicides; viz , those by hanging, drowning, gun-shot wounds, stabbing and cutting, poisons, and suffocation, which, with those where the means of destruction have not been specified, amount to 755 in all; 474 males, and 281 females. Nearly a third of these deaths were caused by hanging; about a fifth by drowning; less than one tenth by stabbing and cutting; somewhat more than a twelfth by poison; more than a seventeenth by gun-shot wounds; and scarcely one thirty-seventh by suffocation. The proportion of the sexes varies according to the means of destruction employed; thus hanging, stabbing, and cutting, and suffocation, were the means chiefly employed by the males, while drowning and poison were those principally made use of by females; fire-arms were in no instance resorted to by females.

Inquests were held upon 8072 cases (6083 males and 1989 females) of accidental deaths, caused without intent or design. The causes of death returned were: drowning, 2410; killed by wheel-carriages, 603; burns and scalds, 532; cold and exposure, 495; suffocation, 369; killed by animals, 214; falls from horses, 200; killed by the falling of masonry, 118; gun-shot wounds, 79; poison, 68; suffocation, from the injurious effects of lime-kilns, 43; killed by machinery, 40; by the explosion of gunpowder, 33; by lightning, 28; *starvation*, 25; and from eating improper food, 4. The deaths from unspecified causes amount to 2811.

In the fourth class, or deaths from natural causes, there are 5417 cases (3774 males, and 1643 females), in 4156 of which (a very large, and, we would say, censurable portion), the immediate cause of death was not specified, the verdict generally recorded by the jury being, "Died by the visitation of God." The specified causes amount to 14, viz. apoplexy and paralysis, 421; still-born, 315; hemorrhage, 112; epilepsy, 108; diseases of the chest, 75; age and debility, 72; fever, 46; insanity, 39; diseases of the cavity of the abdomen, 24; childbed, 17; mortification, 16; cholera, 9; concussion of the brain, 6; and cancer, 1.

The deaths from the immediate use of ardent spirits amount to 1129;

that is, 950 males, and 179 females. The inquests upon these deaths divide themselves into, intoxication, 902, in which death took place either immediately, either upon drinking immediately of whiskey, or while persons yet laboured under intoxication; and 225 deaths caused by the united influence of intoxication, cold, exposure, or suffocation.

In the sixth class, "cause of death uncertain," including all those cases in which a verdict of "found drowned," "found dead," or, "the cause of death not known," has been returned by the juries, the whole amount to 1407; that is, 991 males and 476 females.

At the conclusion of this portion of Mr. Wilde's report, we have what we regard as very important information conveyed to us in the following statement:

> "In 12,762 instances in the total number of inquests, medical evidence was had recourse to in the different classes, according to the following proportions: 1st class, 10 with medical evidence to 1 without; 2d class, 10 to 6; 3d class, 10 to 8; 4th class, 10 to 5; 5th class, 10 to 5; 6th class, 10 to 9. In 315 inquests held upon new-born infants the coroners' juries determined, *without the examination of medical evidence, that* 99 *were still-born;* while in 79 instances of accidental gun-shot wounds, medical evidence was had recourse to in 60 cases!" (Report on the Tables of Death, p. 41.)

Let us, on this side of the Channel, beware how we exclaim, "See, how prettily matters are managed in Ireland!" till we have set our own house in order. We feel a conviction, from what is passing under our own eyes, that were a return obtained, from the provinces of England, of coroners' inquests, specifying those in which medical evidence was resorted to, and those in which it was omitted, the proportion of cases in the latter predicament would be found larger in this country than in the "Green Isle." Such a return could readily be obtained by the home secretary, for his own information, and that of the registrar-general. Were the return so framed as to comprise in every case the verdict, whether medical evidence had been resorted to, and whether this was founded on anatomical examination, the latter authority would speedily discover what a mockery and delusion constituted a portion of his annual reports. It would be generally discovered, too, that verdicts, however they may have the "outward and visible signs" of precision, however scrupulously they may eschew the old and hackneyed phrase, "Died by the visitation of God," are yet mere guesses, in which the chances are at least twenty to one against the guessers, for want of that medical evidence which is so carefully and habitually excluded; John Bull would discover, too, that there was "a solemn farce" daily performed all over the country, which ended in a tragedy, in that very sensitive part of his system—the purse.

It would be out of place to burthen our pages with facts in support of our allegations. The return we have suggested would at once show whether they are well founded. Besides, facts have been already published, in an appropriate quarter,* showing that, in certain parts of the country, matters are conducted as we have represented them to be.

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* See on this subject, in the Provincial Medical and Surgical Journal, No. 175, a letter from Mr. Lane of Grosmont.

How, it may be asked, has such a state of things been brought about? Why is it that that medical evidence, which the interests of medical science, and the community, and the efficient working of the registration act equally demand, is thus scrupulously excluded? We shall endeavour to explain this. Not as a consequence of any new legislative enactment; but mainly as a difference in the practical working of the existing law, the number of inquests holden throughout the country has very much increased. This is in part owing certainly to increased population and the growth of machinery; but is principally attributable to the circumstance that parish-constables and coroners give a more extended signification to the law than it previously possessed. An institution, manifestly framed solely for the detection of crime, as is evident, from the terms of the law on the subject, which expresses that the coroner is to inquire into cases of *unnatural* and *violent* death, is strained to embrace all cases of sudden death, and all cases regarding which any obscurity exists as to the cause, however exempt may be the deaths of both classes from all suspicion of homicide or suicide. If we add to this that the law is stretched so as to include, under the head of violent deaths, many cases not resulting immediately, but mediately or remotely, from violence, such as deaths from *tetanus*, and deaths, which only by a forced construction of the word can be considered violent at all, such as those from *delirium tremens*, one need not feel surprised at the increased bustle in coroners' courts. But if the bustle be great, the cost is great too, and great is the complaint and remonstrance of magistrates, in quarter-sessions assembled, at the inflammation of the county rates by coroners' charges. But these latter gentry are not absolutely indifferent to the frowning aspect of that great abstraction—"Quarter sessions;" nay, so far from being indifferent, they quail before it; and, being unwilling to propitiate this awful "Cerberus" by a diminution of the number of inquests and their own fees, they endeavour to curtail the expenses of each inquest by the exclusion of the only evidence which can give their verdicts any value, medical evidence. The "Medical Witnesses Act," which gave a guinea, and in case of anatomical examination of the body, two guineas to each medical witness, was devised, we are convinced, in a friendly spirit to the profession, and with the intention of securing evidence deserving of credit, for the detection of crime, and for the illustration of obscure deaths; but the practical operation of this act, in the provinces of England at least, has been diametrically opposite to that which Mr. Wakley intended.

The question will naturally arise, how is this state of matters to be corrected? We candidly admit that we have no hope from any modification of coroners' courts. We cannot conceive the possibility of modifying an institution, framed in a barbarous age, when homicides were abundant, purely for the detection of homicide, and for the assessment of the damages due from the hundred where the crime was committed, so as to make it a fit instrument for the discovery of the cause of obscure death in the present day. A Bill is at present (March 1844) in progress through Parliament, and will certainly pass, which invests magistrates, in quarter sessions, with the power of increasing the number of coroners' districts, and thus, by augmenting the number of these functionaries, and bringing each one nearer the work he has to perform, of diminishing one item of cost, the coroners' travelling expenses, (technically called mileage ;) but lest this should, by possibility, be attended with any beneficial result, and diminish the murmurs of quarter sessions, by this very bill the allowance of mileage is, with unaccountable absurdity, increased from ninepence to one shilling per mile. We need not say that no good can possibly arise from such legislation as this.

But we believe that even skilful legislation would be wasted on machines so ill adapted for the purpose now required as are coroners' courts. No Coroners' Amendment Act can answer any good object. Thirteen persons, often humble in station and education (in one case, as Mr. Lane* informs us, a stable-boy) are brought from their daily vocations, at much inconvenience to themselves, and are anxious to get the business over as speedily as possible; and as soon as they discern that there is no reason to suspect homicide, they promptly agree to any verdict, to which they may be led by the coroner (generally an attorney), anxious to avoid any cost beyond the necessary expenses of the court, which are always abundant. Jurors will ever take the view, that, provided there is no homicide, their vocation is at an end; and this limited view we have known them persist in taking, in spite of the remonstrances of medical men, when such have been present. Cases of sudden and obscure death, if duly investigated by a medical man, surely can receive no additional illustration from a coroner and his jury. Let such cases then, be decided by medical officers appointed in different districts of the empire, to discharge functions similar to those performed by the medical officer of the morgue, at Paris, and to these let sudden and obscure deaths be referred. Where suspicion of homicide exists, or where it may arise in the course of the medical officers' investigation, then let the case be examined by the (now) well-organized police and magistracy of the country, who have shown themselves, as we ourselves have witnessed, more skilful in the detection of crime than a parish constable and a coroner's court. Some exclamation may be heard touching that "Palladium" of English liberty,—a jury; but let it be remembered that there are still two juries between one accused of homicide and conviction, a grand jury and a petty jury of assize. Let, then, an institution be abolished,—totally unsuitable to the requirements of the present day; and which has become an obstacle almost insurmountable to the due examination of a class of deaths, which the interests of medical science, and those of the public require to be fully investigated.

We quit this digression, if that can be called a digression which has the most direct relevancy to what must be the foundation of all vital statistics, correct registration, to express the high opinion we entertain of this truly complete Report, *complete*, at least, so far as attainable sources of information rendered fulness and accuracy possible; and to bestow well-merited commendation on the learning and professional knowledge displayed in Surgeon Wilde's Commentary on the Tables of Death.

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* See his letter, already referred to, in the Provincial Medical and Surgical Journal.

ART. III.

Anatomisch Pathologische Opmerkingen over de Ontsteking van eenige Inwendige Deelen van het Oog, en bijzonder over Choroiditis als Oorzaak van Glaucoma. Door J. L. C. SCHROEDER VAN DER KOLK, Hoogleeraar in de Geneeskunde te Utrecht. (*No Date.*)

Anatomico-pathological Remarks on the Inflammation of some internal parts of the Eye, and especially on Choroiditis, as the Cause of Glaucoma. By J. L. C. SCHROEDER VAN DER KOLK, Professor of Medicine at Utrecht. 4to, pp. 26. One Coloured Plate.

THIS memoir was published in the first number of the Transactions (1841) of the Society at Amsterdam for the Promotion of Medicine and Surgery. Independently of the high character borne by the author as a pathologist, the paper itself would justly claim our attention. It is divided into two parts, the first anatomical, and the second pathological.

The author commences by some remarks on the importance of pathological anatomy. If this science, he observes, is indeed to be fruitful, and to afford any explanation of the origin, cause, and course of different diseases, it must by no means be confined to recitals merely of the morbid changes presented by single parts of the body, nor to delineations and descriptions of the form and colours of certain morbid degenerations, but it must go hand in hand with an accurate knowledge of the structure and functions of the various organs in the healthy state. However much has been done by descriptions and engravings to elucidate the diseases of the eye, he thinks that for want of sufficiently exact anatomical and pathological observations, there is yet much uncertainty, and even confusion, in the knowledge hitherto acquired of the origin and nature of many of the diseases of that important organ. To judge of its diseases, and distinguish them with accuracy, it would be necessary to possess an exact knowledge of the system of the eye in general, and especially of the course and distribution of its nerves and finer vessels; upon which latter point there reigns, according to the Professor, yet great uncertainty.

In respect to inflammations especially, it is of great importance to distinguish those which are external from those which are internal; a matter not always easy, because the internal inflammations of the eye, above all if they follow a chronic course, are not always even visible during life. In order to understand this subject fully, it is necessary to be aware that the vessels which are distributed externally to the conjunctiva, are by no means directly connected with the internal vessels of the eye, so that a severe external inflammation or degeneration may exist for a long time without communicating any diseases to the internal parts of the organ. The internal nerves, *nervi ciliares*, seem still less connected with the external ones, which give sensibility to the conjunctiva. The latter arise principally from the lacrymal nerve, which holds no immediate connexion with the branches of the ophthalmic ganglion; a wise precaution, our author remarks, of nature. He thinks it doubtful whether the branches of the ciliary nerves, which, according to Schlemm and Valentin, pierce the edge of the cornea, are connected with the nerves of the conjunctiva. At all events, the connexion can be of very trifling influence, as appears from the frequent and severe external affections of the eye, which occur without any morbid change of the internal parts. In illustration of this, Dr. Schroeder van der Kolk mentions a case of melanosis, in which the eye was extirpated, and in which he succeeded, notwithstanding the necessary division of many of the vessels, in making a very successful injection, even of the finest branches. All the internal parts of the eye appeared quite healthy, although the fungous growth from the sclerotica and cornea was half an inch, and in some places, a full inch in thickness. The same fact derives additional confirmation from the results of the operation for strabismus; the division of the conjunctiva, and of one or several of the muscles of the eyeball, never causing any internal ophthalmia.

Contemplated in a pathological view, there is, among the internal parts of the eye, none, according to Dr. Van der Kolk, except the retina, more important than the choroid. He says, it is that part of the eye in which by far the greatest number of blood-vessels and nerves of the vegetative life are united, and which possesses in a high degree all the conditions fitting it for inflammation. The choroid, besides, gives vessels to almost all the internal parts of the eye, so that an attack originating there may give rise to the most diversified effects. As the state of the choroid cannot be fully investigated except in the dead body, its inflammation is in many manuals not noticed at all; and although some observations and separate treatises have appeared on choroiditis, Dr. Van der Kolk considers the importance of its attacks, and its manifold existence in internal disorders of the eye, to have met with by far too little attention. If the disease becomes chronic, which often happens, it is still more easily mistaken, so that it is almost always the acute form which is described. Our author proceeds to explain the fine connexions of the vessels of the choroid with those of the other parts of the eye, before taking up the anatomical consideration of some of the diseases of this organ.

The choroid, he remarks, is nothing else than a continuation of the pia mater of the brain. Externally, it is covered by the vorticose veins, while on its internal surface, otherwise called the *membrana ruyschiana*, the fine arterial ramifications are found which secrete the pigment. These vessels, however, do not end in the choroid, but pass over into the iris. By means of a beautiful distribution, they form the *corona ciliaris* or ciliary processes. The arched folds of which these processes consist are received between the folds of the *zonula zinnii*, which surrounds the lens, and forms the anterior part of the hyaloid membrane. The connexion between the two zones is rendered much more intimate, Dr. Van der Kolk observes, from very many of the finest capillaries passing out of the fringes of the *corona ciliaris* into the *zonula zinnii*, whence arise ramifications of the highest importance, which have not been described with sufficient exactness. This transition of vessels from the choroid to the hyaloid zone has been represented by several anatomists as seen in the eye of the fœtus born before the full time, but it has been doubted whether it could be seen in the full-grown subject. Dr. Van der Kolk has, by a lucky injection, filled the vessels in question in the healthy adult eye, and has observed extremely fine vessels passing from the exterior circumference of the zonula zinnii into the hyaloid membrane.

The vitreous humour, according to our author, is surrounded by a very thin serous membrane, through which run some extremely minute vessels, which are rarely injected. If we take off this membrane from the vitreous humour, which once happened to our author in an excellently injected eye, it is then evident that the very delicate vessels in question spread along the membrane. On account of its perfect transparency and extreme tenuity, it is indeed scarcely possible to recognize it as a membrane, unless its vessels have been previously filled, which very seldom happens.

These vessels, which probably serve for the secretion of the vitreous humour, arise from two sources. Some shorter ones, which, for distinction's sake, Dr. Van der Kolk calls the *vasa brevia membranæ hyaloideæ*, arise from the exterior margin of the *zonula zinnii*, whence they send their ramifications over the hyaloid membrane, and especially over its anterior part. Other vessels arise from the *arteria centralis retinæ*, just where it commences to spread itself into the retina. Of these vessels, distinguished, by their extreme fineness, and their straighter course, from those of the retina, four seem to be demonstrable. They are called by Dr. Van der Kolk the *vasa longa membranæ hyaloideæ*, and anastomose with the *vasa brevia*. The vessels, then, of the vitreous humour are connected with those of the ciliary processes and the choroid, through the zonula zinnii. The vessels of the retina, which are very abundant, and much firmer and more winding than those of the hyaloid membrane, terminate chiefly in the retina itself, but partly, as appears from a preparation in the possession of Dr. Van der Kolk, they unite with the fine vascular band of the zonula zinnii and ciliary processes.

From all this it appears, that the vessels of the choroid are connected with the iris, corona ciliaris, zonula zinnii, retina, and corpus hyaloideum. From this general union arise some still finer vessels. In the very successfully injected eye already referred to, some extremely fine ramifications pass from the interior margin of the zonula zinnii and corona ciliaris to the anterior surface of the capsule of the lens. The branch of the arteria centralis retinæ, which is demonstrable in the fœtus, piercing the vitreous humour, to reach the posterior surface of the capsule of the lens, vanishes completely in later life. Dr. Van der Kolk suspects that in the adult some extremely fine branches pass from the zonula zinnii, and especially from the vasa longa membranæ hyaloideæ, towards the posterior surface of the lens. This he gives only as a conjecture, as he has not been able to confirm it by injections.

Besides the vessels above mentioned, there appear to be others, too fine to be artificially filled in a sound eye, which spread from the exterior margin of the iris along the posterior surface of the cornea. In chronic inflammation of the eye, these vessels become so enlarged, that they may be filled with coloured material.

The point, then, whence the greatest number of vessels spread into the internal parts of the eye, and which thus comes to be of the highest importance, is that where the vessels of the choroid and ciliary processes unite with those of the zonula zinnii and retina. It is from this point that branches pass towards the capsule of the lens and hyaloid membrane.

Dr. Van der Kolk goes on to observe that, in all inflammations of the choroid, the ciliary nerves are affected, whereby the motions of the iris become impeded. According to the greater or less affection of some of these nerves, some part of the iris, he says, will be more contracted than another, an observation with which our readers are, no doubt, already familiar. He also points out the effect which a morbid state of the ciliary nerves must have on the nutrition of the eye. In an atrophic eye he found all the ciliary nerves much more slender than natural, of a transparent bluish colour, and seemingly deprived of their medulla, while the ophthalmic ganglion was much reduced in size. According to Walther and others, affections of the ciliary system are capable of causing blindness. In a remarkable case of ischias which had proceeded to suppuration, and where the ischiatic nerve was imbedded in pus, Dr. Van der Kolk found on dissection the principal veins of the upper part of the head, as the venæ frontales and temporales, and even those of the diploe of the skull filled with pus; also in the cavernous sinus pus was present; the ophthalmic ganglion was red and surrounded by pus. The patient had been phrenitic, and had been affected with violent pain in the head, and the eye had been red and strongly congested; but there was no blindness.

The ciliary system is connected, moreover, Dr. Van der Kolk remarks, with the fifth nerve, whence may be explained the severe pain of the forehead which attends inflammation of the choroid, and which may arise from a reflex effect upon the origin of the frontal nerve.

Our author next proceeds to consider some of the morbid affections of the eye. He begins by observing, that inflammation of the choroid is described, by some authors, under the more general and very indefinite name of *ophthalmitis interna*. The disease, he says, is most distinctly recognized in the acute form. The symptoms are a pressing, distending, dull, but constantly increasing, pain of the whole eye, soon spreading by sympathy to the head; irritation of the retina, constant photopsia, diminution of sight, dimness and contraction of the pupil. There follows discoloration of the iris, which is also pressed towards the cornea; the headach becomes extreme; the conjunctiva and sclerotica redden; the cornea begins to lose its lustre, and at length matter shows itself in the anterior chamber. This description is taken from Weller, who has merely followed Beer. We must confess, we have rarely met with an idiopathic inflammation of the eye, answering to this description. Neither do we see sufficient proof, that the symptoms in question are those of acute choroiditis. They are fully as likely to belong to an acute inflammation of the retina. We consider this part of Dr. Van der Kolk's paper to be defective. He regards the symptoms set down by Beer and Weller as those of ophthalmitis interna to belong to acute inflammation of the choroid, but he cannot surely expect his *ipse dixit* on this point to be acceded to without evidence. Had it been at all manifest that the symptoms in question indicated peculiarly an inflammation of the choroid, Beer would undoubtedly have preferred the name *choroiditis* to *ophthalmitis interna*. Dissections of such cases are wanted, to clear up their real nature.

Dr. Van der Kolk observes, that the chronic form of choroiditis has long been described under an entirely different name. He holds *glaucoma*, in fact, to be the result of chronic inflammation of the choroid. In support of this notion, he insists much that the greenish reflection in glaucoma is of a concave form, whereas did it depend, as the generality of authors have supposed, on an affection of the vitreous homour, it should appear, he thinks, as a sphere. Of the value of this observation, we shall have occasion to say something hereafter. Our author seems altogether unacquainted with the striking proof of the greenish appearance in glaucoma depending entirely on the state of the lens, afforded by the fact, that if the lens is extracted, the greenness and opacity disappear completely, let their apparent form be what it may.

The pupil is dilated and immoveable in glaucoma. This shows, says Dr. Van der Kolk, an affection of the ciliary system, and thus of the choroid, and cannot be explained by an affection of the retina; a remark to which we cannot assent. In many cases of gutta serena, we have a dilated immoveable pupil, without the smallest suspicion of any disease of the choroid. An insensible state of the retina is perfectly sufficient to cause a want of activity in the ciliary nerves. The motion of the pupil is almost entirely a reflex action, depending on the influence of light on the retina. If the retina is rendered by disease incapable of being acted on by light, the reflex effect on the iris is also at an end.

The pupil is not only motionless in glaucoma, says Dr. Van der Kolk, but its shape is changed, being elongated towards the two angles of the eye, as in the ruminantia, showing an unequal action of the ciliary nerves, whereof two are comites of the long ciliary arteries and veins. These two nerves are more affected, he thinks, by the inflammation of the choroid than the others, whence the oval form of the pupil. This form of the pupil has often been described and figured by the German oculists; but it is remarkable, that, although we see the pupil irregularly dilated in glaucoma, we rarely in this country observe the transversely oval pupil.

The loss of sight in glaucoma is not at all proportionate to the change in the colour of the pupil. Hence we may conclude, Dr. Van der Kolk says, that an opacity of the vitreous humour is not the cause of glaucoma. In fact, the deterioration of vision is so great, that all objects appear as if in a cloud, before any change in the pupil is observable, whence some have believed the true cause of glaucoma to be inflammation of the retina. The symptoms of retinitis, however, are very different from those of glaucoma.

The cause of glaucoma, according to Dr. Van der Kolk, resides neither in the vitreous humour nor in the retina, but consists in a chronic inflammation of the choroid, and an effusion of coagulable lymph between that membrane and the retina. This opinion, he tells us, he has adopted, not from any superficial agreement merely between the phenomena of acute or chronic choroiditis, and those of glaucoma, but from anatomical inquiry.

As a continuation of the pia mater, says our author, the choroid belongs to the serous membranes. Now, it is known, that in certain inflammations, acute as well as chronic, an exudation of serum or of coagulable lymph proceeds from such membranes, and this happens, he adds, in chronic choroiditis. He first observed this in the eye of an old woman, which had been ineffectually operated on for cataract by keratonyxis, while the other eye suffered from amaurosis. In the cataractous eye, he found a thick layer of coagulable lymph between the choroid and the retina. Such an effusion, in consequence of choroiditis, completely explains, in his opinion, all the phenomena of glaucoma, which in this woman was combined with cataract. Of the eye in question, Dr. Van der Kolk gives a representation, and we have no doubt a correct one; but the accuracy of his conclusion, that such an effusion is the cause of glaucoma, we can by no means admit.

The case, upon which so important a conclusion is grounded, is not related with sufficient minuteness. If we understand our author aright, the eye presented the symptoms of cataract combined with glaucoma; it was operated on with the needle through the cornea; the operation was fruitless, and, we presume, followed by inflammation. Is it not much more likely that, in consequence of the inflammation excited by the operation, an effusion took place between the choroid and the retina, than that this effusion, as assumed by Dr. Van der Kolk, preceded the operation, and was the cause of the previous glaucomatous appearance of the eye?

As the matter at present stands, we have little better than a simple averment on the part of Dr. Van der Kolk, that an effusion of coagulable lymph between the choroid and the retina is the cause of glaucoma Glaucomatous eyes have been dissected by Walther, Rosas, Eble, and Mackenzie, and no such effusion of lymph has been noticed by any of these observers.

On the supposition that an effusion of lymph by the choroid is the cause of glaucoma, Dr. Van der Kolk proceeds to explain some of the other symptoms of the disease.

As the sclerotica, he says, possesses a great firmness of texture, not easily admitting of dilatation, it follows, that, by an effusion of lymph between the choroid and retina, the eye must be rendered preternaturally full; whence a feeling of distension, and a gradually increasing hardness of the eye. By the effused substance, the retina will be pressed towards the vitreous humour, whence amaurosis must ultimately ensue. For a time, he thinks the retina will be pressed forwards, so that the focus of the rays of light will no longer fall on it, but behind it, by which means objects will lose their sharp outline, and appear as if in a cloud. At length, not only the retina, but the vitreous humour, the lens, and the iris will be pressed forwards, and the anterior chamber be diminished.

If the lymph, as is always the case at first, is yet clear, the pupil remains black, and a deterioration of vision occurs before any change of colour is to be observed. When the lymph becomes thickened, albuminous and opaque, a change of colour shows itself deep in the bottom of the eye, of a concave form.

An altered or diminished secretion of pigment is also an attendant on chronic inflammation of the choroid; whence the iris becomes pale, and the greenish appearance is augmented. The greenish appearance, however, cannot result from the loss of pigment alone, but depends essentially on the thickened lymph, which assumes, by means of the light entering the eye, a greenish tint.

That a change of the vitreous humour is not the cause of glaucoma, Dr. Van der Kolk concludes, because, first, the greenish tint exhibits a concave and not a convex form, and secondly, the vitreous humour has never shown on dissection a greenish colour, nor a colour which could give rise to such a tint.

We quite agree with our author that the vitreous humour is not the seat of glaucoma; but we cannot say we put much faith in his assertion of the greenish tint exhibiting a concave form. Walther says it is convex. The concave appearance is probably an optical illusion, similar to that by which a cameo sometimes appears as if it were an intaglio. We believe the conclusion, as to the concavity or convexity of the greenish tint in glaucoma, will depend a good deal on the opinion held by the observer regarding its seat. If he regards it as seated at the bottom of the eye, with Dr. Van der Kolk, he will be very apt to see it concave; if, with Mackenzie, he considers it as seated in the lens, it will seem to him convex.

Dr. Van der Kolk refers to certain occasional changes which the vitreous humour undergoes in colour in glaucoma. It has been found yellowish, blackish gray, reddish or filled with reddish points. These changes of colour he considers as consequences of altered secretion, arising from inflammation communicated to the vessels, already described, of the hyaloid membrane.

Dr. Van der Kolk next criticises the notion of some authors, that the green colour in glaucoma is produced from want of the choroid pigment, and from the action of the amber-coloured lens on the light reflected from the bluish choroidal blood-vessels. He refutes this explanation by the fact, that in old people the choroid is nearly destitute of pigment, and that it is quite so in the albino at all ages, yet there is no green appearance visible in the eye, except in the disease called glaucoma.

Though he is convinced that the greenish colour of the thickened albumen, or coagulable lymph, lying between the retina and the choroid, is the real cause of the glaucomatous appearance, he confesses that a loss of pigment must contribute somewhat to change the blackness of the pupil, and aid in the production of the greenish hue. He refers to the state of two eyes, in which he found the pigment defective: in the one, the choroid, over all the bottom of the eye, was of a light brown colour, but was rather darker towards the anterior part of the eye; in the other, which had been glaucomatous for six years, the degeneration was much farther advanced, and the effused substance was already more or less organized; the vitreous humour was partially atrophied; and the choroid had entirely lost its pigment. In neither of these eyes was there any varicose dilatation of the vessels visible, but he confesses they might have shrunk after death.

That a dilatation of the vessels should occur, is indeed a necessary consequence of inflammation of the choroid, and that such dilatation is a constant phenomenon of glaucoma, Dr. Van der Kolk admits. He thinks that many ophthalmologists have described, under the name of ophthalmitis interna, merely acute choroiditis, while the chronic variety they have designated by the names of glaucoma and cirsophthalmos. His conviction is that the more or less striking signs of glaucoma depend solely on the degree of the exudation between the retina and choroid, which is the essence of the disease, and a consequence of choroiditis.

The effusion varies in quantity in different cases. In some it is small, and retaining its original transparency, it produces no green colour, although all the other symptoms of glaucoma are present. The bottom of the eye, in these cases, presents merely a light cloudy or reddish appearance.

It is an important remark, that, on dissection, if the eye be opened under water, the effusion is not easily detected, unless it be thick and opaque, which may be the cause why it has not been oftener observed.

Dr. Van der Kolk believes that a small degree of effusion frequently occurs, especially in advanced age, between the choroid and retina, and that Jacob's membrane is nothing else than a precipitation of the effused fluid, after death, from the influence of cold, or of the fluid in which the eye is placed, a notion which we consider altogether inadmissible.

Sometimes, he remarks, inflammation of the choroid with effusion has another issue, namely, ossification. Formerly, it was thought that the ossification took place in the retina itself, but later observations have shown that it occurs between the choroid and retina, in consequence of chronic inflammation.

As the vessels of the choroid connect themselves with those of the zonula zinnii, crystalline lens, and hyaloid membrane, the remaining consequences of glaucoma are easily explained. The capsule of the lens receives vessels from the zonula zinnii, and perhaps from the ciliary processes. It is well known, that by long-continued glaucoma, cataract is at last produced. The inflammation of the choroid spreads, according to Dr. Van der Kolk, to the zonuli zinnii, and hence to the capsule; the nutrition of the lens is disturbed, and its transparency destroyed. He seems quite unacquainted with the change in the colour and transparency of the nucleus of the lens, which forms an essential part of the disease called glaucoma, in its earliest stage.

Dr. Van der Kolk gives two figures illustrating the spread of inflammation to the capsule of the lens and lining membrane of the cornea, but as the eye whence these figures were taken was one in which the morbid state of the parts was excited by external injury, they cannot be received as strictly illustrative of glaucoma, although, he says, the eye was glaucomatous and cataractous. In this eye, there was an effusion of fluid between the choroid and retina, vessels were seen spreading from the zonula zinnii to the anterior surface of the capsule, the centre of the capsule was thickened and opaque, and at the same time vessels were found extending from the orbiculus ciliaris and great circumference of the iris to the lining membrane of the cornea.

Our author concludes by repeating, that all the phenomena of glaucoma can be explained without difficulty, from an effusion in consequence of choroiditis, in some cases acute, and in others chronic; and that in the course of the disease, from the connexion of the vessels of the choroid with those of the other parts of the eye, all the affections which are consequences of glaucoma are clearly elucidated. The same original disease, namely, a greater or less degree of choroiditis, has been described, he says, by most authors as different diseases, under the names of ophthalmitis interna, cirsophthalmos, glaucoma, and hydrops oculi. These are only separate issues of the same original affection, namely, choroiditis; the specific character of the inflammation causing in the eye, as in other parts of the body, remarkable differences in the issues.

The plate accompanying Dr. Van der Kolk's paper contains four magnified figures, illustrative of the arrangement of the blood-vessels of the hyaloid membrane, capsule of the lens, and lining membrane of the cornea; and of the effusion of lymph, which he describes between the choroid and retina.

Although we can by no means subscribe to the pathology of glaucoma adopted by the professor, we have perused his paper with much interest, and regard the anatomical part of it as deserving the special attention of our readers.

Art. IV.

On the Nature and Treatment of Tic Douloureux, Sciatica, and other Neuralgic Disorders. By H. Hunt, m.d.—*London*, 1844. 8vo, pp. 192.

We have been much gratified by the perusal of this volume. Frequently, and strenuously, have we, in this journal, urged those who really have had experience to communicate its results;—to tell what they really do know; to enter minutely into points of treatment, and into the details of their own management of disease; to communicate freely for the common benefit what they have found most useful themselves. How excellent a book has Dr. Graves produced by doing this. He calls it a 'System of Clinical Medicine,' but happily it is no system at all. It is not a complete work on a science which is as yet only fragmentary and incomplete. It does not treat every disease as if it were thoroughly known, ab ovo usque ad mala. He does not seem to fear, lest he should by any possibility leave anything unsaid. But he gives the result of his own experience; the formulæ he employs, and the reasoning which he pursues whilst prescribing. Hence the practical usefulness of his book; the frequency it is recurred to in doubt and difficulties; the advantage so often found from his hints and excellent formulæ. A similar spirit pervades the writings of a physician, once so distinguished in the same city, which is especially remarkable for the abilities of its medical practitioners, the late Dr. Cheyne of Dublin. The few papers which he has left in the 'Cyclopædia of Practical Medicine' are full of the best practical matter. We especially remember those on gastric fever, wakefulness, and croup. We have read them often, referred to them still oftener, and always with benefit. No directions for the sick man's comfort ever seemed trivial to a man who was worked to death in consultation practice. The arrangement of the beds, pillows, coverlid, and the minutiæ of diet are not disregarded by a mind capable of the largest views of disease. The only drawback is that he wrote so little. To be sure, Dr. Cheyne and Dr. Graves are not ordinary men. But there are many excellent practitioners scattered over Great Britain who have made good use of ample opportunities of studying disease under the best circumstances for that study, those of undivided responsibility. And such might be of much use in their generation beyond their own sphere.

Dr. Hunt has done this with tic douloureux and neuralgic diseases. He informs us that he practised for some years in the south of Devon, where a warm and humid climate favorable to the development of the neuralgic habit gave him an opportunity of investigating this class of maladies. Finding that tic douloureux existed in very opposite states of the system; that it was cured by very opposite plans of treatment; that remedies at one time much vaunted, have fallen into disuse, and that those which cured one case were positively injurious in others,—he endeavoured to discover the causes of this difference. He examined every case on its own merits, scrutinising closely the various organs of the body as well as its general condition, and if he found any well-marked symptoms of disorder or disease of any important organ, regarded that as the exciting cause, and directed his remedies to its relief. This view of neuralgia is not a new one. Whytt viewed nervous diseases in the same way; but his list of causes is much too narrow, and his treatment, in consequence, is limited and defective. Dr. Hunt's view of tic douloureux is consequently comprehensive and thoroughly practical; and although he brings forward no specific, no new drug, he endeavours to lay down clearer indications for the uses of the old ones than has been yet attempted.

It may be said that there is some want of method and arrangement in the book, but the compensating advantage is that the author has not tried to systematise; that he has neither docked nor elongated his patient according to the length of his bed. But we proceed to our analysis.

Description. Dr. Hunt describes the character and peculiarities of tic douloureux clearly and forcibly. The pain, at first dull, then plunging, like electric shocks, with inconceivable rapidity through the different branches of the nerve, (usually the 5th and 7th, but occasionally others,) ceasing for a few minutes, to be renewed, until the patient is driven into a kind of frenzy, and gradually or abruptly subsiding. These paroxysms may be irregular or regular. "Those that come on suddenly and violently from the first are frequently the most severe, irregular, and obstinate." In proportion as the irregular form is prolonged, the attacks become more acute and frequent, and are brought on by trifling causes of irritation, as a touch, a movement, or sudden exposure to heat and cold. If depending on visceral disorder, the attacks vary with the primary disorder. The regular form recurs, with the regularity of an ague, at the same hour; at first trifling, increasing in severity to its acme, and gradually subsiding, several times a day, or daily, or with days' or weeks' intervals, but still so regularly, that the day or hour of its return can be foretold. There is a marked tendency in the disease to return annually at the same period in both forms. "Some are quite free from pain except during one, two, or three months in the year; others have their sufferings *much increased* in particular months, some asserting that they suffer most in October and November, and others in April and May."

It is instructive to trace the analogies in the different members of the same family of disease. As in epilepsy, an attack of tic is sometimes preceded by a feeling somewhat like the epileptic aura. In one case depending on disordered stomach and liver, the attack was invariably preceded by a sensation as if a fine thread passed from about the region of the pylorus through the chest and neck to the cheek when the pain began. In others, a sensation, as if a stream of cool air was gently passing over the part, preceded the pain: one gentleman compared it to the electric aura; others have remarked that for a few hours before the attack they have had that restlessness and irritability and incapacity to settle themselves to any occupation which is so well expressed by "fidgets." Dr. Hunt has found this state to prevail in cases depending on a disordered stomach. In one instance, the painful side of the face wasted, but in twelve months after the pain had been relieved, the wasted cheek had recovered its natural size. In Dr. Hunt's experience, the intensity of the pain has not depended, in the least degree, on the particular cause; there has been as much suffering when the cause was in disordered functions as from local organic irritation.

Dr. Hunt's division of this disease is entirely a practical one, according to the causes from which it originates, and these he arranges under nine heads :

1. Tic Douloureux arising from the neuralgic habit.
2. „ „ dyspepsia.
3. „ „ dyspepsia, complicated with congestion of the liver and other viscera.
4. „ „ anemia.
5. „ „ morbid action in the spine.
6. „ „ disorder of the uterus.
7. „ „ disease of the brain.
8. „ „ local mechanical causes.
9. „ „ malaria, recession of eruptions, and other causes.

1. *Tic douloureux arising from some peculiarity of constitution, or neuralgic habit.* Some individuals, especially women, are, it is well known, morbidly sensible to every impression, moral and physical; pleasure and grief, joy and anxiety, as well as more material causes which directly act upon the nervous system, as mechanical injuries, change of weather, unwholesome or improper food, stimulants and medicines, produce upon their delicate organization much stronger impressions than on ordinary persons. Dr. Hunt seems to assume that this delicate state of the nervous system is the cause of tic, but, as this peculiar constitution is very common and tic douloureux rare, it need not be said that this is but a predisposing cause. Practically, however, and Dr. Hunt's is eminently a practical book, this condition may be assumed as the cause; for the treatment very much consists in improving this state, and giving strength to the nervous system.

Such individuals are, says Dr. Hunt, greatly benefited immediately by a removal to a high, dry, and open situation, care being taken that it is not too bleak. A few months' residence in a mountainous region, such as North Wales, gives tone and strength rapidly, but the caution is requisite, lest, under the immediate feeling of improvement, the invalid is tempted to take an imprudent amount of exercise, which is often followed by exhaustion. From Dr. Hunt's experience, a humid, relaxing climate is very injurious to all this class of delicate females, " few things operating more detrimentally on their general health, or having greater power of producing that peculiar kind of constitutional disorder of which neuralgia is the frequent consequence."

As a general proposition, we are inclined to agree with Dr. Hunt, but here a rule which he himself so wisely lays down, must be attended to, that each case must be studied and treated according to its own nature and peculiarity. We have several patients now, in our eye, with delicate nervous organizations, who are more comfortable and in better health, in a warm and somewhat humid climate, than in a cold and dryer one. They are better also when the weather is damper and warmer, that is, when the peculiarities of the climate are more marked. These are exceptions, but not very uncommon ones. Celsus makes a good practical observation, that change to a worse air is often useful. Where neuralgia has been brought on in a warm and relaxing climate, the change to a dry and colder one should be tried. But, on the other hand, those with a neuralgic habit of body, who are living in a dry, cold, and inland situation, may find benefit by a change to a warmer and more humid residence near the coast.

The injurious effects of anxiety, worry, and other moral causes, need hardly be pointed out. We recollect hearing of a physician of celebrity, who would not continue to attend any patient who had any weight upon his mind: probably from feeling that his visits were useless. In this state of the nervous system, trivial causes, as Dr. Hunt says, are "the greatest obstacles which we have to encounter in the treatment." In the treatment of these patients it should be remembered, says Dr. Hunt, that they "cannot bear harsh or violent remedies with impunity, but that they require a steady and patient perseverance with mild ones, until a more healthy action is induced; afterwards, by a cautious use of tonics, combined with sedatives, and attention to a proper plan of living, the general strength may be so increased, that the neuralgic habit will at length, to a certain extent, be overcome." Caution is necessary, adds Dr. Hunt, lest the violence of the pain may lead to the exhibition of powerful tonics and narcotics by which the neuralgic habit may be confirmed. In addition to the common rules, to use exercise to produce such an amount of fatigue as moderate rest will recover, and to rest during digestion, Dr. Hunt insists on the necessity of feeble persons avoiding exercise for some time previous to their meals, for if they expend their nervous energy just before eating, digestion will be impeded. He gives a case of a gentleman subject to tic, who was in the habit of taking violent exercise in shooting, hunting, &c., who could never dine before he had rested quietly nearly two hours, for if he did, a violent paroxysm of tic was the certain consequence. And what is curious was, that the tic depended on an organic cause. These rules are particularly to be observed by those in whom fatigue after exertion is increased by some or other stimulants. It is "a feverish fatigue," and it is removed by a saline draught with ammonia, or a few grains of nitrate of potash and sal volatile.

These feeble persons are often much fatigued by dressing, lose their appetite for breakfast, and beginning the day badly, go through it tired and weary. A little nourishment, such as a cup of coffee, with a little bread, before leaving bed, enables them to dress with comfort, to sponge with cold water, or, what is better, to use the shower-bath and friction. Most fully do we agree with Dr. Hunt as to the importance of these seemingly trivial hints.

2. *Tic douloureux from dyspepsia.* The neuralgic habit of body may be the effect of previous disease. In investigating the history of cases of tic, Dr. Hunt has found that the disorders which have preceded the pain, and which may be regarded as its cause, are such as debilitate the body, either suddenly or by slowly undermining its powers. Of the former, influenza, as Dr. Holland pointed out, was a frequent cause. Of the latter, disorders of the digestive organs, of an atonic or nervous character, rather than of an inflammatory, are the most common. Indeed, however various the causes, the stomach is usually the first organ that suffers, although the patient may be unaware of it, and expect that he is quite well, excepting the pain in his face: a remark of Dr. Hunt's which is applicable to many diseases.

Two cases are detailed of tic arising from the simplest form of stomach disorder.

A middle-aged dean, a generous liver, finding himself growing stout, adopted a spare diet, with excessive walking exercise, which brought on general debility, indigestion with acidity, and, in a few weeks, constant pain in the cheek, aggravated in paroxysms. An emetic, followed by a warm purgative, and a course of liq. arsenicalis, combined with a sedative, relieved him.

The second case was that of a strong, athletic man, of forty, whose diet for many years had been a beefsteak for breakfast, with coffee, a full allowance of meat with two thirds of a bottle of port for dinner, and the remaining third, with meat, for supper. He had intense neuralgia of the left cheek, the paroxysms returning at all hours both of the day and night. Tongue foul, appetite good, but uneasy feelings owing to digestion. An emetic relieved the pain at once. It was followed by warm aperients, and a more rational diet, and he was soon cured.—The rapid amendment in both these cases Dr. Hunt judiciously attributes to the simplicity of the cause. But such simple cases are rare. For many years he has made it a regular practice in these cases to begin with an emetic: much glairy and tenacious mucus is evacuated, which, he has not the slightest doubt, facilitates the action of the medicines, especially of arsenic. If the paroxysm is regularly intermittent, the emetic should be given an hour before the fit; and after the emetic, a warm aperient draught, of twenty grains of rhubarb and sulphate of potash, with thirty drops of sal volatile, in some aromatic water. After this, a course of arsenic, combining it with a sedative, or with a few grains of the bicarbonate of potash if there is an acid stomach, he finds the best remedy: beginning with four minims three times every day, with double the quantity of compound tinct. of camphor, and gradually increasing the dose of liq. arsenicalis, until there is some decided symptom of its action, which is commonly evinced when the dose has amounted to ten. When the pain has considerably decreased, he discontinues the medicine for a few days, and recommences with the original small doses, and finds it rarely necessary to increase them, but continues them for several weeks, if not months, after the pain is removed; for patients should be strictly cautioned against the error of thinking themselves cured as soon as the pain is relieved. They must persist in the medicine and diet until the tone of their stomach is quite restored, or the pain will return. The susceptibility of the stomach towards this remedy is various. In some cases it must be given on a full stomach. If the pain at any time increases, the aperient should be repeated, and the quantity of arsenic increased. During the whole treatment an occasional aperient is useful. When the pain is relieved and the stomach improved, the substitution of a grain of quinine, three times a day, is useful, although it would have disagreed at first. Large doses of iron, quinine, bark, &c. are injurious in these cases. The arsenic may disagree *immediately*, producing an indescribable sensation of distress in the stomach, dryness of the fauces, white tongue, and other symptoms of gastritis. It should be at once discontinued; and as this morbid sensibility is probably accompanied with slight inflammation of the mucous membrane, a rigidly farinaceous diet, in small quantities and lukewarm, should be ordered; with small doses of nitrate of potash, combined with two or three minims of Scheele's prussic acid, three or four times a day, and three or four grains of James's powder, at bedtime. If aperients are needed, a common lavement, a little castor-oil, or two or three ounces of Pulna water; a sinapism to the stomach, or rubefacient liniment, and an occasional blister. After a few weeks of this treatment, arsenic may be borne, and at first should be given with or just after meals; and if it again disagrees it should be abandoned altogether. It is more beneficial to do little than much in such cases; by being content with a proper diet, and by avoiding anything that can possibly irritate the over-sensitive nerves. The attention to diet in all cases of neuralgia from dyspepsia is most important; and Dr. Hunt gives the result of his experience.

In cases of extreme sensitiveness of the stomach, the mildest food is sometimes necessary. Such patients should live entirely on farinaceous food, until the nerves of the stomach become less sensitive; which is known by the tongue becoming cleaner, and the general feelings (better known to the patient than described,) returning to those of health. When the stomach has been thus improved, some animal food should be added, beef-tea, a chicken prepared thus: "a chicken is to be wrapt in muslin, and stewed for twelve or fourteen hours, with half an ounce of vermicelli and a few whole peppercorns, until the whole has become a jelly; some of this, diluted, if necessary, with a little toast, forms a very nutritious and easily-digested meal." As the appetite improves, a slice of chicken or game, with stale bread or toast, &c. In such weak stomachs every kind of food has a tendency to become acid, which is in some measure prevented by some slight stimulus with or after meals; a little weak brandy and water, or a teaspoonful of sal volatile in a glass of water, or a cup of coffee.

For those less sensitive, or whose morbid sensibility has been quieted, a plain nutritious diet, of animal food, (chicken, game, mutton, and venison are the best kinds,) with stale bread or toast, and with a few well-boiled vegetables, *if they can be taken with impunity;* and there is seldom any objection to plain puddings of rice, bread, and tapioca. All fish, pastry, rich puddings, fruit, new vegetables, pickles, cheese, and various sauces must be strictly forbidden.

Errors in quantity may be prevented by two rules: 1st, to live simply and to avoid a variety of dishes; and, 2d, to eat slowly, that the first indications that sufficient food has been taken may be felt and obeyed.

Water is the best drink. Some, especially those who have indulged in wine, require the stimulus of a moderate quantity in order to digest at all. But the smallest quantity for this purpose should be taken, and as strength is gained, this allowance should be gradually diminished and discontinued altogether. A little brandy and water or a glass or two of good sherry or sound port are the best. For breakfast, bread or toast, weak café au lait, or scalded milk, prepared by placing the can of milk on a stove moderately heated, until all the cream has risen to the surface, which is to be removed when cold. This is easily digested. Those who can digest bacon may eat it. It should be toasted after being boiled. The other hygienic rules laid down in the previous chapter should be followed.

As good rules for the treatment of atonic dyspepsia are useful in so many chronic diseases, and disorders of the general health with which it is associated, we need not apologize for quoting so largely; for on its right treatment much of the success of a physician depends. The chief point on which we are disposed to differ with Dr. Hunt is in the use of vegetables and of farinaceous puddings, in addition to meat. Except in those comparatively few cases where the mucous membrane is so sensitive that it will not bear meat at all, we find that on the whole it is better to limit our patients to a dinner of meat, stale bread or biscuit, and rice. Vegetables, especially potatoes, are often injurious; and a patient who likes them is not the best judge if he is the exception; and if farinaceous puddings are allowed in addition to meat there is danger of error in quantity. The difficulty, we well know, in limiting the diet is to get our directions attended to.

3. *Tic douloureux from dyspepsia, with congestion of the liver and other viscera.* The symptoms, in addition to the local pain, are "a sallow, muddy complexion, with eyes half jaundiced; foul tongue; hard and tense abdomen; sluggish, irregular bowels; and urine scanty, high coloured, or turbid."

Large doses of opium, taken to mitigate the pain, often act injuriously, by weakening the stomach and producing this congestion.

Long-continued anxiety Dr. Hunt regards as one of the most frequent causes of it, as well as sedentary habits, too great exertion of mind or body, a residence in malarious districts. But he has somewhat strangely omitted what must be as material as any of these causes—errors in the quality and quantity of the food, of long continuance. Even long-continued anxiety and sedentary habits would have much less effect in producing this condition if the indvidual accommodated his diet to his diminished powers. From our own experience, we think very few persons are fully aware, who are in these circumstances, how much their bodily comfort is dependent on the strictest attention to the quantity and simplicity of their food, and especially, perhaps, when the injurious effects are produced some time after a meal.

The treatment Dr. Hunt adopts in these cases is to unload the congested organs by an emetic, followed by brisk mercurial purgatives, until the "system is unloaded, and the viscera are stimulated to a more healthy action." In old cases this requires perseverance and time; and, when the congestion is obstinate, a visit to Carlsbad, to ensure a course of saline aperients, change of air, habits, &c. Kissingen, however, is more conveniently got at. The great disadvantage, however, of German spas is their unwholesome table d'hôtes, especially in such cases as these, where diet must be most important. Fortunately we have two excellent saline springs at home, Leamington and Cheltenham.

It is in these sort of cases, Dr. Hunt observes, that purgatives of croton oil are beneficial; and he thinks this is especially the case when it acts as an emetic as well as a purgative. Mercury, given so as to affect the system, should, as a rule, be avoided in disorders of the nerves; but in these cases, when obstinate and of long standing, it may be necessary.

When this congestive condition is removed, tonics or sedatives may be employed. When the pain has been excessive and exhausting, Dr. Hunt gives sedatives, in doses of sufficient strength, repeated at short intervals. He has found belladonna the most efficacious, particularly when the pain is irregular. When regularly intermitting, he prefers a grain or two of solid opium, with camphor, an hour or two before the expected attack. This plan must be modified in those of feeble powers, in whom, with the congestion, there is much debility and irritability: milder purgatives, with as much nourishment as can be borne, and the free exhibition of cordials and perhaps sedatives. As the secretions improve, tonics, combined with aperients, gradually increasing the former and decreasing the latter. When the tongue becomes clean and the complexion clearer, if the pain continues, arsenic is a useful tonic, with a sedative (opium, belladonna, or conium,) at night, to ensure rest, and smaller doses during the day, to allay irritation. And even when the pain is overcome, attention is required to strengthen the system, to prevent a relapse, by quinine, steel, &c., continuing the plan for many months—avoiding the common error of attempting to cure quickly by large doses of the tonic.

4. *Tic douloureux from anemia.* Where there is no local cause of irritation to account for the pain, nor the neuralgic habit, but where persons, naturally of a strong constitution, have a pallid skin, loss of strength, and often symptoms indicating a deficiency both in the quantity and quality of the blood, Dr. Hunt arranges the cases under this division. It is this class of cases which are so much benefited by iron, continued unremittingly for months, until there is evidence of pure red blood in the system. In one case, which is detailed, of this kind, where the paroxysms were very intense, a grain of belladonna was given during the attack, and ordered to be repeated, if necessary, for three successive hours. Soon after the third pill was taken, the pain began to subside. The pain did not return for a week, and then one grain checked it. In these cases, belladonna is often very serviceable in allaying the general irritation of the system, as well as in checking the pain.

5. *Tic douloureux from morbid action in the spine.* Three or four cases of very obstinate tic douloureux of the face in females fell under Dr. Hunt's care, in which he could not discover any local cause, but attributed the disease to a deranged state of the general health. All plans of treatment and every kind of medicine failed to afford relief. Some symptoms, such as loss of power in the legs, led to an examination of the spine; much tenderness was discovered on pressing some of the vertebræ. Caustic issues were used, and the recumbent posture enjoined, with complete relief to the tic, and restoration or improvement of the general health. The duration of this treatment varied. The connexion between the state of the spine and the pain in the face could not be doubted. "Whether it originated in that part," adds Dr. Hunt, "may be doubtful." Whilst admitting fully that much injury was done ten or twenty years back by treating the tender spine of hysterical females by cupping, issues, and confinement to the horizontal posture, instead of strengthening the system, Dr. Hunt thinks that the opposite error is fallen into now, "with far more serious results;"—examples having occurred in his own practice and in that of others, "of protracted misery or death" produced by treating organic disease of the spine, its cord, or envelopes as a functional hysterical affection, requiring pure air and tonic remedies; and a careful attention to such cases has convinced him that the pain in the spine, which at first was purely hysterical, depending on some deranged state of the general health, degenerates gradually into real disease of structure. The commonly-admitted explanation of this, that nervous excitement leads to increased vascular action and a low kind of inflammation, he illustrates by referring to those cases of tic where the violent paroxysms of pain are, after some time, followed

by great and constant tenderness of the cheek, and constant swelling, as if from infiltration of serum. It is a point of great practical importance to discover when this transition takes place, and it is one of difficulty, as real disease may exist and be masked by hysterical symptoms. Dr. Hunt alludes to the difficulty, but attempts to lay down no rules for our guidance. There is a middle treatment, which we have not seen in books, but which we have employed, not indeed in tic, but in cases where there was long-continued tenderness of the spine, with disturbance of the general health, and various nervous symptoms, not yielding to the ordinary means. This is, a caustic issue or seton over the painful spot in the spine, combined with moderate exercise, and other general means calculated to improve the health. By this plan, the most injurious part of the old treatment—confinement to the recumbent posture—is avoided.

We rather demur to any direct comparison between the unfortunate results of treating hysteria as organic disease, and the opposite error to which the times are more prone. Death may not have been the consequence of the first mistake, but what a multitude of female constitutions has not this "nimia diligentia" seriously impaired, rendering them unfit for the proper discharge of the active duties of the remainder of their lives, and often entailing a similar condition of body upon their half-organized offspring! Still the error of treating organic mischief as mere hysteria is a very serious one. It brings us back to that truth, which we cannot too steadily remember, that "every case of disease is a separate study," and that no amount of experience will justify routine practice.

6. *Tic douloureux from disorder of the uterus.* Four cases are detailed. In all, the original cause of ill health appeared to be a bad labour attended with flooding, and followed by menorrhagia and leucorrhea. In two, the pain commenced soon after confinement, and always returned at the menstrual periods. In the other two the pain commenced several years after confinement, but this was evidently the cause of their broken health; in one of these as the disordered uterus was cured, so was the tic, in the other, the uterus was not relieved, neither was the neuralgia.

In all of these, carbonate of iron had been taken in considerable quantities, before Dr. Hunt saw them. The weakness and bloodless countenance seemed to indicate it, but it failed by increasing the menorrhagia, and, consequently, inducing greater debility. In these cases, Dr. Hunt says, arsenic is peculiarly adapted: he thinks it subdues morbid sensibility of the nerves, and restrains passive menorrhagia. He has also found belladonna and opium, previous to the menstrual periods, useful. Cold astringent injections to the uterus and cold hip-baths, sometimes saturated with bay-salt, are valuable adjuvants. The clothing over the loins should not be too warm, and lounging on warm relaxing couches should be avoided. The possibility of the menorrhagia depending on a loaded state of the bowels should not be overlooked. To show the connexion between the uterus and neuralgia of the face still further, one case is reported in which a lady was subject to neuralgia of her face in all her pregnancies, at about the same period. It lasted about ten days, and was cured by arsenic. In another case, a paroxysm like tic came on before delivery; it was very severe during delivery, and subsided as soon as the child was born. The placenta was retained, so that extraction was necessary; as soon as the fingers reached the uterus, the pain returned, but ceased as soon as the placenta was removed, and did not return. Allusion is made to the face-ache, which always precedes menstruation in some delicate young women; and a case is given, in which a lady had severe neuralgia of the ulnar nerve, coming on when she was about four months advanced in pregnancy, and continuing until her confinement, after which it immediately ceased.

When there is a periodical return of tic every month, Dr. Hunt has found that it was connected with some irregularity in menstruation.

7. *Tic douloureux from disease of the brain.* In these cases the pain may be at first intermittent, but by degrees the attacks become more constant and irregular, and confusion of thought, loss of memory, impairment of vision, difficulty of articulation, tottering gait, and palsy by slow degrees, indicate the cause. There is nothing new under this head.

8. *Tic douloureux from local mechanical causes.* The connexion between the pain and the teeth, to which the patient so often refers his sufferings, should be carefully examined. It is useless to make the examination during the paroxysm. But if, in its absence, any tooth on pressure, or on being slightly struck with a metallic instrument, is sensitive, or the paroxysm is brought on, the tooth should be removed. In all cases decayed teeth should be extracted or stopped, and stumps removed. Several cases are given of tic from diseased teeth, where either exostosis of the end of the fang, or a rough state, or the periosteal covering converted into a lamina of bone was discovered. Dr. Hunt has remarked that decay of the teeth was more frequently the consequence than the cause of tic; the teeth which were good and firm originally, becoming affected with caries at that point at the edge of the gums, where the enamel terminates. Under this head, unnatural growths of bone, local injuries of nerves, foreign substances pressing upon nerves, and the diseased state of nerves in the stump after amputation, are alluded to among the occasional causes of tic; and the propriety of keeping the general health as good as possible by giving tone and strength to the system, and by allaying irritation, insisted on even in those cases where the cause of irritation cannot be removed.

Dr. Hunt mentions malaria as a common cause of tic, but thinks that Dr. Macculloch has so ably treated that subject as to preclude the necessity of anything further than an allusion to it.

Periodical headach. Brow-ague, as it is commonly termed, only gradually assumes a periodic character, beginning as diffused headach, coming and going at uncertain times, at length returning with more severity at particular hours, and assuming the quotidian and more rarely the double quotidian or tertian type. The fully formed paroxysm commences with uneasiness in the temple, gradually increases for an hour or two, until it becomes excruciating pain which continues for an hour or more, and gradually subsides, leaving the individual perfectly easy in the interval. It often arises from malaria; sometimes from mere debility, or from anxiety, over-nursing, or any other cause of exhaustion. Two cases are given, in which it returned annually at the same time, one in August, the other in April. Dr. Hunt commences the treatment by an emetic, followed by a purgative, after which a course of quinine or arsenic, continuing the remedy for two or three days after the pain has ceased, and repeating it for a few days at the end of a week, otherwise, as he has found, the pain is very apt to return about the tenth day. In some cases, capsicum or black pepper, combined either with the quinine or arsenic, cures more quickly than either used separately. He prefers one or two grain doses of quinine three times a day, or every six hours, to larger doses.

Sciatica. Painful affections of this nerve, though classed under the same name are very various.

Acute inflammation of the nerve is most frequently the effect of exposure to wet and cold, standing or sitting in water, or wet ground, in boats or in coaches. The pain in the course of the nerve is very severe, aggravated by the slightest motion, and attended with high febrile symptoms. It requires strictly antiphlogistic treatment. Cupping along the course of the nerve, brisk purging with calomel, followed by salts and senna, each dose containing thirty drops of vin. colchici, or three to six grains of powdered colchicum, every five hours, until the febrile symptoms are diminished. Then ten to fifteen grains of Dover's powder will greatly relieve the pain; for the pain is seldom reduced in proportion to the reduction of the febrile symptoms, a point which should be remembered. At this stage, two grains of calomel, from four to six of powdered colchicum, and five of Dover's powder, should be given every six hours, with some diuretic, and an extra dose of opium at night if necessary; repeating the black draught every second morning. It is needless to add that the effect of the colchicum should be watched. Even after these means, the pain may be obstinate and severe, requiring blistering. It may be necessary to continue the calomel until the gums are sore, as it is important to prevent the disease from becoming chronic, which it is very prone to do.

This chronic inflammation of the nerve is obstinate, and sometimes followed by loss of power, wasting and shrinking of the limb. The pain does not wholly subside, but is aggravated in paroxysms, and particularly at night. In some cases there is nocturnal fever. To distinguish this state from one of pure neuralgia, is often difficult. Nocturnal fever, and scanty, high-coloured urine with sediment, sometimes assist the diagnosis. If the patient has this chronic form of the disease, when first seen, purgatives should be given, especially if his age is advanced. Calomel and colocynth, followed by castor oil, sometimes bring away hardened feces, and occasionally perseverance in the purgatives has relieved the disease, which seemed to depend on a loaded state of the bowels; but in general mercury will be necessary. The following formula Dr. Hunt employs—

℞ Hydrargyri phosphatis, gr. j.
Opii, gr, j.
Antim. potassæ tart. gr. ⅛.
Fiat pilula omni nocte sumenda.

If there is much nocturnal fever, in addition to this, moderate doses of nitrate of potash and colchicum should be given three times a day, with occasional aperients. If much exhaustion of strength and emaciation, the compound decoction of sarsaparilla with the liquid extract during the day, and the mercurial pill at night. Counter-irritation is especially useful. Open blisters, the tartar-emetic plaster, or even a caustic issue behind the trochanter. Dr. Hunt alludes to the hydriodate of potash as producing a powerful effect in some cases, but says he has not himself used it.

Sciatica, of which pain is the only symptom, returning in paroxysms like electric shocks, is occasionally met with, and should be treated like pure neuralgia, first by purgatives, and then by steel, quinine or arsenic with sedatives.

A case is given in which sciatica existed with piles and prolapsus ani, and the removal of the piles relieved the pain.

Persons subject to sciatica should wear flannel drawers next to the skin, and if they are very susceptible to weather, washed-leather drawers over the flannel, during the winter months. We have seen benefit in some cases of obstinate pain in the sciatic nerve from washed-leather drawers worn next to the skin.

It is well known that intermittents may become remittents. Dr. Macculloch alludes to the explanation which has been given, that this is owing, in intermittent fevers, to some intercurrent visceral derangement, but thinks there is no decided proof. Dr. Hunt has found that cases of intermittent neuralgia which have become remittent, have again resumed the intermittent type, after an emetic and brisk purging or after the system has been brought under the action of mercury; and he is inclined to believe that when an intermittent neuralgia becomes remittent, it is to be attributed to some disorder of some of the viscera. Practically this is important,—the change indicating the necessity of evacuants and deobstruents, after which quinine may again be given.

Dr. Hunt gives another hint worth attending to; that when tic occurs annually, it depends on some deviation from health, to which many persons are annually subject, and that by attending to the first symptom of this general disorder, you anticipate the local disease.

The volume concludes by some observations on the use of sedatives and tonics, in neuralgia.

Sedatives. There are three modes of giving sedatives to allay violent pain.

1st. By a very large dose at once. This is hazardous.

2d. By a smaller one, gradually increased. This is uncertain.

3d. By giving the largest ordinary dose, and repeating it every hour, or every second hour. This is the plan Dr. Hunt adopts. He is most partial to belladonna, and the largest quantity he has given has not exceeded one grain for three successive hours. Before the third dose is given the patient should be visited. When a decided check has thus been given to the pain, smaller doses, Dr. Hunt finds, keep both the pain and morbid irritability under control,—a third, a half, a grain once, twice, or three times every day, gradually diminishing the dose as the case improves. The patient should be instructed to take a dose at any time when the pain threatens to return. As a general rule, sedatives should not be given on the first visit, as by obscuring the symptoms, they render the detection of the cause more difficult. When there is uterine irritation, they are beneficial during menstruation, and when menorrhagia also exists, if given a few days before the commencement of menstruation, they often both moderate the discharge and allay irritation and pain. In these cases a mild aperient, such as castor oil, compound decoction of aloes, with or without gray powder, or blue pill, should precede the sedative. In cases where there is pain in the spine arising simply from weakness and irritability, sedatives are useful with quinine and steel; but when complicated with visceral obstructions, or of long standing, and when it has become a fixed disease, little relief can be expected.

Tonics. Arsenic is clearly Dr. Hunt's favorite remedy, and he devotes a chapter to the rules which guide him in its administration. He finds it act most favorably on those of lax fibre, languid circulation, cold and moist skin, and whose urine is pale and plentiful. In such it not only relieves the pain, but gives general strength. Where the urine is high coloured and scanty, with lithate of ammonia sediment, the tongue loaded, and especially its tip and edges red, it disagrees and aggravates the pain; but it often is useful when the visceral disorders on which these symptoms depend are removed.

It is peculiarly appropriate when the disease arises from malaria and is strictly intermittent, when it depends on a neuralgic habit, disorder of the uterus; but when tic is associated with morbid action in the spine, or with anemia, or is complicated with visceral congestion, it is usually injurious.

Dr. Hunt does not doubt that many will think he has laid too much stress on the necessity of unloading the bowels and producing a more healthy secretion before tonics or sedatives are given. We are not of this number, as we are convinced that it is a matter of primary importance, and that it cannot be too strenuously insisted on. The combination of tonics with aperients, (as quinine with sulphate of magnesia and sulphuric acid,) which is often good practice, Dr. Hunt has not dwelt on as we think it deserves.

Arsenic should not be given for many months successively, but should be discontinued from time to time, as soon as any of its peculiar effects on the system are discovered, and not resumed until all symptoms of its action have subsided. These are, less plentiful secretion of urine, more acid and high coloured, with at last deposition of lithate of ammonia, disinclination to food, rather hot skin, sensation of general warmth, with a tingling in the fingers and toes; but all these symptoms seldom occur together. The change in the urine alone is a sufficient indication that the arsenic should be discontinued. Arsenic has been given in the solid form. But we refer our readers on this point to the book itself, for we should not feel justified in giving less than every word of caution with which the recommendation under any circumstances of such a dangerous remedy, was guarded. Dr. Hunt evidently puts but little faith in local remedies of a sedative kind. He has found aconitine ointment (one grain to a drachm) of temporary benefit, as well as belladonna, and he has often used with advantage a poultice made of equal parts of linseed meal and bruised stramonium seeds.

The length and completeness of this analysis afford sufficient proof of the estimation in which we hold the work itself. No man could have written it unless he had seen much disease and carefully studied it. The author's judgment and his principles of investigation are sound. He studies every case on its own merits, and makes his facts available as guides in future practice by examining, comparing, and arranging them. We can confidently recommend this volume to every student of disease; a large class; for what practitioner does his duty who is not a student all his days?

Art. V.

The Physiology of Inflammation and the Healing Process. By Benjamin Travers, f.r.s., Surgeon extraordinary to the Queen, &c.—*London*, 1844. 8vo, pp. 226.

Mr. Travers's object, in the unpretending little treatise now before us, " has been to extend our acquaintance with the phenomena of inflammation, as far as the present state of science permits, by the joint aid of physiological and pathological observation, each deriving force from the other."

In proceeding to lay a brief account of it before our readers, we must not fail to express the great pleasure which it gives us, to see a practical surgeon of Mr. Travers's eminence—at a period of life when novelties usually cease to be attractive, and are often regarded with distaste or with positive repugnance and contempt—boldly avowing his conviction, that the pathological doctrines which have been handed down, with but little question, from the time of Hunter, must now be reexamined by the improved light which modern science affords, and tested especially by the facts which the microscope reveals. Even if his volume did not in any way add to our stock of information, we should feel that he well deserved the thanks of the medical world for his good intentions. As a Treatise on Inflammation, we must certainly consider it defective and, in some respects, erroneous; yet, as containing the only account yet published, of the valuable series of preparations made by the late Dr. Todd of Brighton, to illustrate the reparative processes in the frog's web, and as giving to the world the highly interesting results of a series of experiments and observations made by the author himself (with the assistance of Prof. Owen and Mr. John Quekett,) on the same subject,—we believe that it may rank amongst the most important contributions, which the literature of this department has received in later times.

In the Introduction is given a general view of the present state of our knowledge as to the physiology of nutrition, as a proper preliminary to the discussion of the nature of inflammation. In the justice of the following statement we fully concur: "The alterations of which the composition of the blood is susceptible, and those of the relative and reciprocal actions of the blood and the vessels, [or rather, we would say, of the blood and the *tissues*,] which may be fairly presumed, if not ascertained to follow thereupon, are data abundantly sufficient to furnish forth all the phenomena of inflammation, and to supply the rationale of their occurrence." (p. 13.) But we find this statement afterwards complicated by the introduction of the nervous system, as an element in the production of the phenomena of inflammation, to an extent that is not warranted by facts; in this we trace the persistence of certain physiological notions of former years, which, sanctioned by the authority of Hunter, still continue to pervade, more or less, the minds of those who regard themselves as his particular disciples, though inconsistent with the results of wider observation and more correct reasoning:

> "It is as indispensable to the understanding of any part of the doctrine of disease, as of the economy of health, to appreciate the influence of this system; all morbid phenomena are traceable to one or other of its properties; and it may be

received as an axiom, that the disturbance of consent and relation, as their uniformity turns upon nervous organization, constitutes the most prominent feature, if it be not the exciting cause, of morbid action. Inflammation, therefore, stands in the same relation with all morbid affections, whether functional or organic, to the powers and uses of the nervous organism; nor can we stir one step towards an explanation of its causes, phenomena, and laws, its symptoms or their indications, without appealing to its guidance." (p. 18.)

We are not sure that we understand this extract very clearly; and we regret to say, that a certain degree of obscurity pervades other parts of this introduction. Thus, we are told in the succeeding paragraph, that "Inflammation is susceptible of a purely functional as well as an organic form"—a distinction which, with reference to this department of pathology, we must confess ourselves unable to understand. We observe, too, in p. 11, the following statement, with which we cannot accord:

> "The arteries possess a capability of dilatation and a power of recovery; whether muscular, or in virtue of a proper contractility of tissue, or compounded of both, physiologists are divided; Mr. Hunter's experiment showing the difference between the living and the dead artery, notwithstanding. In the larger arteries the muscular, and in the small the contractile property is said to predominate; but whether any order of vessels possesses more than the elasticity which accurately preserves the vessel under its varying dimensions in a state of fulness,is an open question." (p. 11.)

Now, Hunter's statement was, that all the arteries possess *elasticity*, a physical property dependent upon the presence of yellow fibrous tissue in their walls; and also a peculiar vital *contractility*, analogous to that of muscles, and dependent upon a tissue resembling muscular fibre; and, further, that the former predominated in the large arteries, and the latter in the small. Now, as this statement has been fully confirmed by other anatomists and physiologists, who have proved that, in all its essential conditions, the contractility of the arteries resembles that of the lowest forms of muscular fibre,—and as the microscopical inquiries of Henle and others have shown that fibrous structure analogous to the non-striated muscular coat of the alimentary canal exists in arteries, we cannot see any ground for doubting Hunter's conclusions. And we think that in separating *muscularity* and *contractility*—assigning one to the large and the other to the small arteries—Mr. Travers shows a vagueness of conception as to those properties, which we should not have expected from him. He proceeds to say, "that the proof of *elasticity* is furnished by the varied states and rapid changes of the capillary circulation in the temporary phenomena of blushing;" but these we should assign rather to the *muscularity* of the arterial walls, seeing that the changes in question are undoubtedly under the influence of the nervous system, which, so far as we know, has no power over the property of simple elasticity.

In the first chapter are stated the preliminaries or postulates, on which Mr. Travers's view of the character of the inflammatory process is based. These, however, do not so far differ from the ordinary statements on the subject as to require any remark from us. He repeats the assertion, that the nervous system is essentially connected with the process, in the following form:

> "Inflammation is not seated exclusively in the blood, the blood-vessels, ab-

sorbents, or nerves of the part, nor confined to the capillary system of vessels, nor the cellular tissue which connects and supports them. These several parts forming a homogeneous whole, cannot be disjunctively affected, in morbid more than in healthy action; each being an integral element in the composition of the other, reciprocating organization and function, acting and acted upon respectively, their peculiar functions are merged in the production of general phenomena, whether healthy or morbid, to which each is indipensable. Thus neither heat, pain, swelling, nor redness could be demonstrated in inflammation, more than temperature, sensation, figure, and colour could be maintained in health, if the blood or the vessel, the nerve or the absorbent were taken away. We might as well expect nutrition to go on in the absence of the stomach." (p. 26.)

Now we think that, in this passage, Mr. Travers confounds the external signs of inflammation, with the essential nature of the process. If it be, as physiologists now seem inclined to regard it, a perverted form of the ordinary action of nutrition, it may be as independent of the nervous system as is the normal process which it represents; and the fact that inflammation may take place in paralysed parts, and even in parts whose nerves are uninjured, without pain, seems a proof that the nervous system is not a necessary and essential participator in the phenomena of inflammation, though usually concerned in the production of the symptoms or manifestations of this process, both local and general.

Mr. Travers takes exception to the doctrine propounded by Dr. Macartney, that the healing process may be effected without inflammation; and that the latter action, so far from being favorable, is adverse to healing. On this point we shall presently offer a few observations, when noticing another part of the treatise.

The two succeeding chapters, on the direct effects of stimuli and of wound, and on the local symptoms of inflammation, do not offer us anything peculiarly calling for remark. The constitutional symptoms, or effects of inflammation on the system, are next discussed; and this in a manner, which shows all that power of observing phenomena, and of drawing from them sound practical deductions, which we should anticipate from a surgeon of Mr. Travers's experience. The general state of the system suffering under acute inflammation, and the *irritative* form of the fever accompanying certain injuries, or occurring in bad constitutions, are most graphically described; and are rightly attributed, we think, partly to the altered condition of the blood, and partly to the affection of the nervous system. That the increased quantity of fibrin in the blood, and other alterations in its quality, must operate as disturbing causes on the general functions of the system, can scarcely, we think, be doubted; but on the other hand, as Dr. Williams justly remarks, the constitutional disturbances does not bear a relation by any means constant to the alteration in the condition of the blood; and it often seems as if it preceded the local affection. Moreover, we find that the varying excitability of the nervous system in different individuals, has a great influence on the degree of constitutional disturbance produced by a local inflammation; and it seems to be from the low degree of this in most of the inferior animals, as compared with man, that their system is generally affected much less than his, by severe injuries or local diseases.

The observations on bloodletting as a remedy for inflammation are so valuable, from their being at the same time scientifically true, and practically important, that we shall transfer them almost entire to our pages.

"The choice of measures, *i. e.*, local or general bloodletting, is determined, partly by the relation of the parts affected to the centre of the circulation, and in part by the more or less urgent necessity that exists for disembarrassing the general functions, and arresting destructive inflammation. In visceral inflammation, venesection is [often] indicated and warranted to the utmost extent that the powers of life will bear; for here the mass of blood is so altered and spoiled for its proper and healthy purpose, by the direct implication of the blood-making and blood-preparing organs in the disease, that relieving the system of its presence to the extent that can be borne, is the main resource we possess for its preservation. As we would remove a poison, a *materies morbi*, in such cases we take away blood..... A freer circulation through the small vessels, and those of the excretory glands especially, ensues almost immediately upon a full bloodletting; the sense of overwhelming oppression is relieved, and the inflammation, if not abridged by its effects, is disposed to a kindlier termination.

"There are two false doctrines concerning bloodletting for inflammation, which cannot be too strongly condemned; the first, anticipatory bloodletting, by which I mean, the large and repeated detraction of blood, before inflammation, being considered inevitable, has actually manifested itself, on the hypothesis of starving the action, and thus rendering it tractable, which is a direct attack on the vitality and fatally perverts the action, if it do not destroy the resisting powers of the system. The second, continuing the employment of the lancet, so long as the last-drawn blood exhibits the signs of inflammation, which if drained to the last drop it would do; or, in other words, not reflecting that there is a line beyond which the practice becomes destructive instead of remedial; and that there are many inflammations which do not admit of arrest by depletion, and upon which other modes of treatment are efficient for this end, even though not an ounce of blood be drawn. Many lives have been sacrificed to the prevalence of these irrational and absurd notions; and many preserved in their extremity by being fortunately placed beyond the reach of the surgeon; especially, I am induced to believe, in military practice.

"When the change of the mass of blood ensues upon local inflammation, where-ever seated, some of the internal organs especially, but all more or less, indicate the effect produced by it, and fever is established..... It may be asked why, if this reasoning be correct, we do not combat the general inflammatory fever of the system, when arising from a complicated injury, or fracture, or ulcer of the extremity, as when affecting the visceral organs? the answer is clearly this, that in the former case the inflammatory fever is secondary, and not idiopathic; that the local injury or inflammation attending it has already produced its effect upon the nervous system, to the extent of materially deranging and depressing its powers; and that we dare not reduce them directly, as by the lancet, lest we destroy the vitality of the inflamed part, if not sink the general power too low for sustaining life under its burthen. But even in this case, if from a seeming metastasis, or from any cause, the visceral organs indicate inflammation, we do not hesitate to employ general bloodletting, and in all such cases instantly and freely employ topical bleeding." (pp. 58-60.)

The various results of the inflammatory process are next discussed. The effusion of the serum of the blood, which is the first of these, seems to be, in Mr. Travers's opinion, as in ours, a simple physical result of the over-distention of the blood-vessels, and of stagnation of blood in them; taking place, as it does, in so great a variety of conditions, in which inflammation does not at all participate. The exudation of fibrin, however, dissolved in the serous fluid, in other words, of *liquor sanguinis*, is considered by Mr. Travers, as a pathognomonic sign of the presence of inflammation; being effected by a vital action analogous to secretion. "I cannot view this phenomenon," he says, (p. 74,) "as some have done, as a mere mechanical effect produced by a gorged state of the vessels; if it were, there would be no such uniformity as exists in the results of inflammation; we should oftener have blood poured out *en masse,* and not its separable ingredients; the change commencing within the vessels, and perfected externally. The process is peculiar to life, and the operation of physical and chemical forces is in subservience to vital laws." We are not sure, however, that it is necessary to have recourse to this explanation; and if we *can* fairly explain the phenomenon on physical principles, it is obviously better to do so. The increased tendency to separation between the two elements of the circulating blood, the augmentation in the quantity of the fibrin,—the distention of the vessels, and consequent weakening and thinning of their walls,—the stagnation in the movement, and consequently increased pressure,—would all seem to favour the effusion of the liquor sanguinis; and as it has been clearly proved by the experiments of Mr. Robinson, that this *may* escape from the vessels without inflammation, but under physical conditions which resemble those produced by the inflammatory process, we are much inclined to refer the effusion of fibrin in an inflamed part, to the same causes. That it should take place so constantly, is only to be expected from the uniformity of the preliminary changes; and there could be no effusion of blood *en masse,* without an absolute rupture of the vessels. We would not wish to assert dogmatically, however, that no process resembling that of secretion, is concerned in the effusion of fibrin from inflamed vessels; but merely desire to point out that physical causes, known to be in operation, will go a great way to account for it.

The process by which the organizable fibrin, poured out into a wound with loss of substance for the purpose of reparation, forms a vascular communication with the surrounding parts, is extremely well described; and evidently from the author's own observations. We should gladly have transferred the whole to our own pages, did our limits permit; but we must be content with quoting the following summary:

> "Every wound presents the following phenomena: the occlusion of all divided vessels, not of large caliber, by clot or coagulum of blood; the actual stasis of the circulation in the part, the first local phenomenon of inflammation, with or without lesion; the gradual deposit of fibrin devoid of colour, and the subsequent more gradual vascularization of that fibrin, by the opening of transparent channels for the admission of at first isolated globules, then a train or file of globules, and ultimately a column conveying colour; their inter-communication, anastomosis and reflection towards the part whence they are derived; and lastly, the offset of stagnant meshes, like advanced posts from the parent blood-vessels, slowly acquiring motion and dispersing. All tend to demonstrate that the laying down of the organizable fibrin, the intrusion of the blood-corpuscle, and thence the formation of the new vessels, are the work of active coloured capillaries nearest to the point of stases, aided and completed by the return of circulation, also gradual to the vessels, trunk and branch, which had been rendered motionless by congestion." (p. 80.)

Mr. Travers does not admit that there is such a thing as isolated or independent vascularization,—i. e., the production of vessels in the organizing lymph, not originating in the subjacent vascular surface; and he gives a different explanation of the appearances, which have induced some pathologists to admit such an inference. He states, however, that the vessels are visible in fine striæ, before circulation can be detected in them; and that isolated globules often enter the capillary tube and perform an oscillatory motion for many hours, before any series of them passes into it. Hence we cannot regard the new channel as burrowed out by a string or file of red corpuscles, pushed out from the nearest capillary, by a *vis a tergo*, as some have maintained that it is. Mr. Travers denies that blood effused from wounded surface ever becomes the medium of organized adhesion; and specially refers to Mr. Dalrymple's case, (Medico-Chirurgical Transactions, vol. xxiii,) of the supposed vascularization of a clot of blood effused between the periosteum and bone, as misunderstood by the describer. From our own inspection, however, of a portion of this clot, the vessels in which had been injected from the subjacent surface, we feel disposed to affirm almost positively, that these vessels *could not* have been, as Mr. Travers supposes, those of the connecting cellular tissue of the bone and periosteum, being entirely different in their aspect and distribution. The question, however, is not one of much practical importance; since all are agreed, that blood is much less readily penetrated by vessels, than organizable lymph. We fully accord also with Mr. Travers, in assigning to the process of *rapid* organization, which is seen especially in the ill-elaborated lymph of cachectic subjects, the character of want of permanency;—"slow and sure being a pretty constant couple throughout nature;" and we shall presently apply the same view to some other forms of the healing process. We should have liked to see, in this chapter, the result of Mr. Travers's observations upon the *organization* of lymph or liquor sanguinis; by which we understand the development of cells, fibres, &c., in its substance,—a process which is anterior to its vascularization, and independent of it. The importance of the adhesive process in the economy of nature, and the results of its deficiency in disordered states of the system, are well displayed.

In chapter vii, is an account of the preparations made by the late Dr. Todd of Brighton, to illustrate the successive stages of the healing process, in daily stages, from the infliction of injury to the completion of repair.

"To accomplish this purpose, a corresponding injury was inflicted upon the webs of a large collection of fresh frogs at the same time; and a wounded web being removed at successive intervals of twenty-four hours, was dried, and then placed between two squares of glass, and hermetically closed by a balsamic composition, which preserves appearances even to the shade of colour with remarkable fidelity; the date of each preparation being registered, and carefully engraven on the glass. This interesting collection was purchased of Dr. Todd's executors, by the Royal College of Surgeons, and is now arranged in their museum. The instant appearance of the part, especially of the blood-vessels and their colour, can only be preserved by such a mode of death, as is instantaneous and without struggle, for which purpose Dr. Todd employed nicotine. It is deeply to be regretted that the specimens are unaccompanied with any manuscript record or notes by way of illustration, although subjected to careful examination by the doctor, from time to time, with the design of working out the subject, and ultimately presenting it in a finished state to the profession. Neither the eye nor the mind of a survivor, however accomplished, can supply the place of those of an original inquirer. The time occupied for the completion of the process varies according to the nature of the injury; and allowances must be made for the incidental varieties of vigour or languor in individuals, and perhaps the influence of season, to reconcile the seeming discrepancies in the rate of advance; wherefore an interval of three days furnishes a safer average index of changes, than a shorter period. With such an admission, it is remarkable how striking a uniformity prevails throughout this interesting collection, and how additionally corroborative of general results are these natural and necessary inequalities of progress." (pp. 93-4)

For the details of these appearances, as presented at successive intervals, after the following injuries,—incised wound traversing an artery, incised wound traversing a vein, incised wound in the direction of the toes, excision of a circular portion of the web by a punch, contusion, and burn,—as well as for a brief summary of the appearances exhibited by each series,—we must refer our readers to Mr. Travers's work. We may state, however, that the reparative processes appeared to be of the same character, in all instances, with those which are seen in the first sprouting of the feet of these animals; as soon, at least, as the immediate effect of the injury was recovered from.

The formation of granulations, and the process of suppuration, treated of in chapter VIII, are regarded by Mr. Travers, as constituting "the second or advanced stage of adhesive inflammation, providing repair not only for solution of continuity, but for loss of substance." We formerly expressed our concurrence in the opinion of Dr. Macartney, that granulation and suppuration are not by any means essential to the closure of a wound or ulcer, in which there is great loss of substance; and that they are to be avoided if possible, both on account of the constitutional disturbance they engender, and the imperfect nature of the local restoration,—the endeavours of the surgeon being rather directed towards the establishment of a process resembling that of natural growth, which, though *slower*, is much *surer*. For in the latter, the new tissues resemble those which they have replaced; whilst granulations consist of an entirely different structure, very rapidly organized, but of low vitality, and not permanent in their character; so that they are absorbed at a subsequent time, and produce a contracted cicatrix. We are further inclined to regard the effusion of liquor sanguinis in the degenerated or degraded form of pus, as the consequence of the low vitality of the part from which it is exuded; a view which corresponds very well with the known action of the influences which produce suppuration. We know that these views will not spread rapidly amongst our older brethren, who have been accustomed to regard suppuration as a *healthy* process, when the wound is in a healing state; but we would ask whether any reparation can be more complete than that which takes place in the frog tribe,—new legs, with perfect bones, muscles, nerves, &c., being often produced after the severe loss or injury of the original ones,—whilst no pus-globule, nor any fluid resembling pus, can ever be detected during the process?

We find, in a later part of the work, some further observations upon the constitution and origin of pus; from which it appears that Mr. Travers regards this fluid as consisting of the "superfluous or waste lymph, which has been separated during the adhesive stage, from the mass of the blood, held in solution by the serum, being thus a chemical modification of the constituents of the liquor sanguinis; in short, the latter fluid deprived of its original characters and power of spontaneous coagulation." (p. 172.) We do not see that the mere dilution of the fibrin with serum, can destroy its coagulability; since we know that a very minute quantity of fibrin diffused though a serous fluid, will occasion the formation of a coagulum, however slight. We are much more inclined to regard the aplastic character of pus as the result of a change in the *vital* endowments of the fibrin of the liquor sanguinis at the time of its exudation. The effusion of pus upon the surface of granulations is considered by Mr. Travers as necessary to their maintenance, affording them a bland and homogeneous protecting coat; this may be very true,—but the question still is, whether it is desirable to encourage the process, and whether the closure of a wound may not be often effected, under seclusion from air, and a regulated temperature, by a process of a much more satisfactory kind than granulation and suppuration. We think it can; and that the "slow and sure" operation to which we have adverted, is worth more attention than Mr. Travers has given to it.

In order to complete the history of the healing process, which the cabinet of Dr. Todd partially displayed, Mr. Travers carried on, with the assistance of Mr. John Quekett, a series of observations on the effects of injuries of the frog's web,—adopting the more useful plan of permitting the animals to live, so that the stages of the healing process might be observed and recorded, until it was perfected in the same individuals. The ninth chapter of Mr. Travers's work, which contains his account of these observations, we regard as the most valuable in the whole work; and we only regret that it was not expanded into a separate memoir, and accompanied with such delineations as, without being too elaborate in their execution, might have conveyed a better idea of the successive stages of the process than any words can do. We are sure that our readers will thank us for placing before them the following recapitulation or digest of the phenomena observed during the early period of reparation. The return of the circulation has been already noticed:

"1. Stasis, or actual arrest of the circulation, is a direct effect of local irritation, more or less persistent, according to the degree or shock. Thus, if unattended by injury to structure, recovery from it restores the previous condition, whether inflammation be set up or not. Its local extent, like its duration, will be according to the amount of the irritation. The circulation is oscillatory at the verge of the stasis; beyond this it is preternaturally slow; and yet further from the stationary centre it is, or appears to be, somewhat brisker than natural. The suddenness and completeness of the stasis determine the acuteness of the inflammation; it is slowly formed and imperfect in what are termed congestive inflammations.

"2. Contingent upon the stasis is the effusion of serum or of the liquor sanguinis, the one or the other, according to the nature of the injury inflicted, whether of function, simply, or of structure. If serum only be effused, the inflammation, if any be present, admits of perfect resolution; not so, if the effusion be of the liquor sanguinis holding the fibrin of the blood in solution. This is the distinction between the effusions of shock and of lesion, the nervous and vascular, the inflammatory and non-inflammatory œdema. The second results from a more considerable and prolonged action than the first, if both be the effects of inflammation; but whereas the aqueous effusion is often unattended with inflammation, the fibrinous effusion is characteristic and proper to it, whether with or without primary breach of texture.

"3. The fibrin effused in a state of solution in the liquor sanguinis only becomes susceptible of organization; i. e., capable of permanent incorporation with the living solids, when separated from the other constituents of the blood......

> "4. The effusion of the liquor sanguinis from the arterial capillaries is the first change consequent upon inflammation, either with or without breach; it is, in either case, limited to the extreme verge of vascular action, or the boundary-line of its arrest (complete stasis). In the act of coagulation upon the face and sides of the wound, the contained fibrin separates from the serous portion of the liquor sanguinis, and becomes a crust or membranaceous stratum, covering in the wound at all points, the sections of blood-vessels, nerves, absorbents, &c. This, then, forms the intermedium of vascular communication by anastamosis, in cases of union by adhesion..........
>
> "5. The second act in the healing process is the separation of the lymph-particle from the blood within the vessel. This is not seen until after some time has elapsed, for it is not an immediate consequence of the stasis, which may and does continually happen for short and definite periods, without this separation or any effusion of lymph, as in simple irritation or inflammation of a sound texture without deposit. The necessity created by extensive breach, or injury determining such breach, establishes the adhesive action on a scale of extent and duration proportioned to the loss incurred by the injury; and the effusion—always marginal only—during the period of stasis, and maintained so much in advance of the active circulation, as almost to convey the idea of its being derived from some other source, is now in the form of lymph-globule, which has become separated within the arterial coloured capillary, for the special purpose, as it would seem, of supplying the permanent plasma or new solid. Its appearance in the veins is not recognized until the stasis is passed and the circulation restored. Neither is it seen in the newly-formed vessels, but only in the vessels which have undergone the stasis. These are preternaturally dilated; the venous side of the capillary circulation appears to be that most loaded; it is at least most conspicuous. The restored circulation of the part is slow and laboured; the deposit is not only limited to the verge of the wound, but its progress and the rate of healing bear a steady reciprocal relation to each other.
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> "The elaboration of organizable lymph appears to become perfected as it advances, confirming the practical observation, that wounds which heal slow heal sound. It is quite a mistake to suppose that, in wounds attended with loss of substance, the effusion of organizable fibrin constitutes the permanent material of the new solid. The first deposit effused with the liquor sanguinis is an amorphous exudation, and presents no such regular figure and arrangement as the lymph-particle which has been separated within the vessel before deposit. They are, in truth, different in this respect; the first, or that which forms immediately on the receipt of injury, and serves for the intermedium of organization in a close apposition of surfaces, (being separated from the liquor sanguinis, with which it is effused,) would not serve as a base for the new solid; it is soon absorbed, being only a temporary bond or adhesive layer, in harmony with the parts, though serving the important purpose of consolidation by anastamosis of the contiguous vessels of the opposite sides, or union by the first intention; the second is not called for in mere divisions of substance, and is not ready if it were; it requires a higher and long-continued inflammatory action; it is a permanent, not a provisional mean of reparation—a substantial addition of structure, not a mere conjunction of parts." (pp. 160-6.)

There is a little obscurity in the above statement, as regards the separation of the lymph-corpuscles; but, on looking into the detailed accounts of the observations, we find the following passage, descriptive of the appearance of the web on the twentieth day after the injury, which fully explains the author's meaning:

> "Active circulation in new vessels, to the outer margin of the lymph-layer bordering the wound; a very great number of the isolated round particles of lymph in the circulating vessels, not forming part of the current, nor blended with the blood, but stationary, either single or in groups, then carried on from time to time in vessels which have the appearance of veins; again, these or similar particles

accumulated at the outer edge of the wound permanently at rest, and disposed in rows, forming the new lymph-bed of organization." (p. 145.)

From these observations, Mr. Travers has been led to an inference respecting the function of the colourless or lymph corpuscles, which closely corresponds with that upheld by Dr. Carpenter in his Report on the Physiology of Cells,* and which affords a striking confirmation of it; viz., that they are concerned in the elaboration of the organizable lymph or plasma, at the expense of which the new tissues are produced. It would seem, from Mr. Travers's observations, as if the material first effused were the ordinary liquor sanguinis of the blood, and as if this were not sufficiently organizable to become an entirely *new and permanent* tissue, though adequate both to afford nutrition to the old, and to form a new tissue of temporary character. It is not, according to him, until this increased development of lymph-corpuscles manifests itself in the vessels of the part, that the permanent new tissue is generated.

Now it is a very interesting question, whether the healing process, as thus described, is to be regarded as of an inflammatory character or not. We think not; and for these reasons: The process is exactly analogous to that which is witnessed in the first development of many new parts during embryonic life; as, for example, the crystalline lens, and the extremities when they begin to bud out from the trunk. In both of these cases there is a retardation of blood in the neighbouring vessels, which are much dilated, and a great accumulation and stagnation of the colourless corpuscles, which appear to elaborate the organizable matter required for the new growth. All these, therefore, are merely acts of formation taking place with unusual activity. A similar condition is seen in the vessels of the Fallopian tube during the passage of the ovum, around which the chorion is formed by the exudation from the lining of the tube; and the same would probably be found in the spots at which the rapid growth of the stag's horns takes place, or any other act of new formation, to which no one has ever thought of applying the term inflammation. In all these cases, there is no change in the general character of the blood; because the action is one of a purely local kind; and as fast as the new fibrin is generated, it is used up in the formative process. But if this production were *too* rapid, there would then be an accumulation of fibrin in the blood, and a part of it would probably pass off in the unorganizable or aplastic form of pus. We reserve, however, the detailed discussion of this subject for another article; and shall now conclude with a few words on the closing chapters of the work before us, which treat of ulceration, cicatrization, and gangrene.

Mr. Travers is a staunch advocate of the Hunterian doctrine, that the absorbents are the vessels chiefly, if not solely, concerned in the removal of the tissues by ulceration; and he combats at some length the objections of those, who have maintained that the veins are the agents of this operation. His arguments, however, are chiefly based on the assumption, (for such we must term it,) that the debris left by the ordinary waste of the system are taken up by the lymphatic vessels; a doctrine which has been lately called in question, and of which there does not seem any sufficient proof. The question seems to us, however, of very little practical importance; that of the condition of the parts disposing to ulceration is of much greater consequence. It is not, in our apprehension, an increased action of the absorbent vessels (whether veins or lymphatics,) which is the primary cause; but an increased waste or disintegration of the tissues, resulting from previous changes in their state. There is, in fact, as it seems to us, a molecular death of a certain amount of tissue, whose debris are removed by the absorbent vessels; the difference between ulceration and sloughing being simply this,—that in the latter, the whole substance dies at once, and separates from the living part, so as to be thrown off *en masse;* whilst in the former, the death is more gradual, and the process of interstitial absorption still continues active, so that the products of disintegration are removed as fast as they are set free. The latter is consistent only with a state of parts, in which the vitality has been less completely destroyed than in the former process; and, consequently, we find ulceration taking place at the edge of a slough.

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* Brit. and For. Med. Rev., Jan. 1843..

We shall only further say, that these closing chapters, concise as they are, abound with important practical observations and deductions; and that the whole work, though certainly far from being a complete treatise on the subject, may be strongly recommended to the student and practitioner, as well as to the professed pathologist. On the latter, Mr. Travers will confer a great boon if he will follow out his series of observations on a more extensive scale, and give their results in a more extended and elaborate form.

Art. VI.

Recherches Anatomiques Physiologiques et Pathologiques sur les Cavités closes, naturelles ou accidentelles, de l'Economie Animale. Par A. Velpeau, &c. &c.—*Paris,* 1843.

Researches, Anatomical, Physiological, and Pathological, on the close Cavities of the Animal Economy, natural or accidental. By A. Velpeau, &c.—*Paris,* 1843. 8vo, pp. 208.

The materials of this work are derived entirely from the personal observation of its author, who has therefore professedly avoided all reference to preceding inquiries, and contented himself with regarding observations analogous to his own, but of anterior date, as so many corroborations of his statements. This method holds out many advantages where the subject of investigation is intricate; and no one is better entitled to adopt such an unfettered course than M. Velpeau, whose well-known professional erudition will at once exempt him from the charge of bringing out old things as new, through ignorance of the labours of his predecessors. We shall make our notice of his work conformable to the spirit in which it is written, and confine ourselves to an examination of the reality of the facts adduced, and the validity of the conclusions derived from them, with as little reference as possible to the history of the subject. The contents of M. Velpeau's book are rather startling, for they amount to no less than an attempt to subvert certain positions which are generally reckoned among the best established in anatomy and physiology.

The First Chapter is on the "anatomy of the close cavities," and its scope is to demonstrate the apparently singular proposition that serous and synovial membranes, as distinct tissues, have no existence, and consequently that the notion of close cavities formed by such membranes, is entirely devoid of foundation.

In support of his doctrine, M. Velpeau appeals to the development of the parts in question in the fetus, as well as to a careful investigation of them in the adult. From the examination of ten embryos, aged from fifteen to thirty days, he concludes that, at the end of the third, and often even of the fourth week of intra-uterine life, the free surfaces present no appearance of membrane. The whole body seems to consist of a homogeneous, gelatiniform, and fragile substance. The cavities present everywhere simple surfaces, and are nowhere lined with membranes consisting of a distinct tissue. There are no laminæ, or membranes capable of isolation. The whole reduces itself to *surfaces*, and *parenchymata*. We see a cutaneous surface, a mucous surface, and serous surfaces, but nothing which can justify the expressions "cutaneous membrane," "mucous membrane," or "serous membrane," whether in the head, chest, or abdomen. During the subsequent periods of fetal life, the parietes of the close cavities assume, in the course of their development, some of the characters of membranes, in certain regions of the body, or certain portions of their own extent; but these characters, which become more and more distinctly marked, even to adult age, and some of which endure even to the last term of organic evolution, are wanting completely in many of the cavities in question, and are not to be found in all parts of any one of those cavities, at whatsoever period of life they may be sought for. (pp. 2-3.)

M. Velpeau then enters minutely into the anatomy, in the adult, of the internal surfaces of the cranial, spinal, thoracic, and abdominal cavities, of those of the joints, of the tendinous sheaths, and of the bursæ mucosæ. As the peritoneal sac seems, on the whole, to afford the most unequivocal instance of a distinct membrane forming a shut cavity, we extract our author's account of it as an example of his manner of treating the whole subject.

"There is no part of the body where it is possible to detach such extensive layers of serous membrane as in the interior of the abdomen. Nevertheless, even there, the peritoneum does not exist indiscriminately at every point of the cavity called 'peritoneal.' The anterior and posterior surfaces of the stomach, the convex part of the small intestine, the cells of the large intestine, the posterior surface of the bladder, the two surfaces of the uterus, and the exterior part of the liver—none of them admit of the actual separation of a purely serous membrane. On the moveable viscera having a fleshy fibre, the serous surface being doubled, (united,) with a pretty thick fibro-cellular layer, may decidedly be detached as a membrane, but it is then clearly a cellular layer become serous on its free surface, and not a serous membrane properly so called. On the liver, an organ of which the tissue is at once dense and fragile, the serous surface evidently forms part of the subjacent layer which is itself continuous with the parenchymatous texture of the biliary organ. The same remarks apply, at all points, to the serous surface of the uterus. The ovary, the size of which does not vary continually like that of the intestines, presents, in like manner, a serous surface merely, instead of a real membrane. Behind the linea alba, in the vicinity of the umbilicus, the serous surface is so completely confounded with the other tissues of the abdominal parietes, that it is impossible to separate them otherwise than by the artificial formation of a *traumatic* peritoneum. It is evidently the uniform aspect and *unquestionable con-*

tinuity of the whole serous surface of the parietes of the serous cavities that has caused them to be described as so many distinct membranes. As this surface belongs to real membranes at certain points, it has thence been concluded that the membrane must exist also in parts where it could not be at all detached. To render manifest the error of such a supposition, we have only to recollect that the serous membranes do not exist prior to the organs invested by them; that the most evident of them become distinct only at a very advanced period of the embryonic state; and that all these surfaces become gradually established in the place where they are to remain, instead of applying themselves to it like an expanded veil." (pp. 9-11.)

It would be occupying our space unnecessarily, to follow M. Velpeau in his description of the serous surfaces in other cavities of the body, since his mode of demonstration and reasoning is precisely analogous to that contained in the foregoing passage; for further anatomical details, we therefore refer the reader to the work itself.

With respect to the synovial cavities, in like manner, M. Velpeau maintains that, instead of the supposed continuity of membrane throughout their inner surface, there are few vestiges of distinct membrane at any point of it. Such membrane is nowhere to be found on the free surface of the cartilages, and it is equally absent on the internal surface of most of the ligaments. In the knee, for example, if we seek for synovial membrane behind the patellar ligament, or the termination of the muscles of the thigh, we shall be unable to isolate it as a distinct membrane. "The same holds true opposite to (on the articular surface of) the internal lateral ligament and the other fibrous layers of the circumference of the joint. Outwardly, on the contour of the cartilaginous surfaces, in the grooves which separate the fibrous envelopes of the articular heads, we scarcely meet with a real synovial membrane; again, it appears to form part of the folds, and fringes, and cellulo-adipose fasciculi, called synovial glands, on several of these points." (pp. 11-2.)

This description is anything but lucid; we have therefore rendered our author's words into English as closely as possible, leaving the reader to determine their precise signification for himself. Indeed we cannot help remarking that the general style of this work falls far short of the perspicuity and neatness which usually characterize French writings on medicine, and form so agreeable a contrast to the clumsy and confused diction of too many of our own works on that science.

In the fifth section of this chapter, M. Velpeau treats of the membranes supposed to line "accidental close cavities." Such cavities he divides into "functional" and "pathological." The functional close cavities consist of, 1, *Serous cavities;* 2, *Articulations;* 3, *Cellular cavities.*

1. *Accidental serous cavities.* These are formed, according to M. Velpeau, when an ovary, a loop of intestine, a knot of epiploön, or any other viscus, passes through a fissure of the peritoneum and abdominal muscles, so as to fix itself for some months or years under the skin. When this takes place, we may be certain that a serous cavity will establish itself around the displaced organ, and the author states that he has frequently observed such an occurrence in cases of hernia, and some varieties of hydrocele. (pp. 33-4.) We should have been glad if M. Velpeau had adverted more particularly to the nature of these cases, which must, of necessity, be rare, and of which, as here alluded to, it is not easy to form any distinct conception.

These accidental serous cavities present the same characters as the normal, that is to say, they do not exist as independent sacs, but merely represent the surface of the neighbouring tissue, which has become smooth and polished from accidental causes.

2. *Accidental articular cavities.* Close cavities connected with the articulations are accidentally formed, as a consequence either of a luxation or a fracture. These we need not dwell upon, as they are perfectly familiar to surgeons; the only point to be noticed is the alleged absence of any distinct lining membrane, the inner surface of these cavities being, according to M. Velpeau, nothing more than the polished surface of the textures which enter into their composition.

3. *Accidental cellular cavities.* Accidental close cavities are nowhere so frequent, says our author, as under the skin. Wherever the skeleton forms a permanent projection at any point, if any part of the body endowed with resistance has to support long-continued or often-repeated pressure or friction, we may be sure that a mucous (synovial) cavity will there be formed. It is thus that such cavities occur on the back of porters, on the acromion of persons whose shoulders have often to sustain burdens, on the angle of the scapula in those who make use of bricoles, scuttles, &c., on the anterior part of the sternum in joiners and others, on the malleoli of tailors, on the hump of hunchbacks, on the salient points of club-feet. M. Velpeau has known such cavities to form on the body of the clavicle, on the posterior surface of the forearm, on the internal surface of the tibia, and on the crest of the ilium, in persons who have had these parts subjected to frequently repeated friction and pressure. These cavities, like normal ones of the same character, are destitute of investing membrane.

The pathological close cavities are very numerous, comprehending every species of abscess, cyst, and morbid deposit, but it is to morbid close cavities analogous to the cavities of the joints and tendons, and those which are subcutaneous, that M. Velpeau especially directs our attention. These cavities are observed in the cellular tissue, in certain glandular bodies, and in the lymphatic ganglia.

a. In the cellular tissue. Morbid close cavities appertaining to this tissue, have been described particularly under the name of serous cysts. There are few parts in which they have not been found, and their dimensions are extremely various. When they are situated amidst a lax and abundant cellular tissue, they may indeed be detached in the form of distinct sacs, but this simply as any layer of cellular membrane may be separated from the adjacent layers. But where these cavities are surrounded by dense tissues, they are represented, through a greater or less portion of their extent, by the substance of the tissues which they appear to line. They are, in effect, enlarged cells, or natural interstices, the parietes of which have been expended by the accumulation of the liquid with which they are filled.

b. The glandular bodies. The close cavities occasionally formed in the glandular bodies are adduced by M. Velpeau as illustrating his doctrine much better than those of the cellular tissue. In the thyroid body, the breast, the testicle, in which these cavities are often met with, "it is sufficient," he says, "to open one's eyes, in order to perceive that they have never existed in the capacity of membrane; that their surface makes part of the tissue of the gland itself; and that the pellicle which may occasionally be isolated from it, has been formed there in a manner similar to that which forms part of the cavity of an aneurismal sac, in which the blood has continued to circulate." (p. 37.) This fact, our author states, he has often verified in both lobes of the thyroid body; and at various points in the substance of the breast. Close cavities occurring in the ovary seem to M. Velpeau to afford unanswerable evidence of the truth of his ideas with respect to close cavities in general, for we here find real cysts, like hydatids, which are readily detached from the tissue of the organ, contrasted with close cavities, the surface of which is inseparable from it, and forms part of it. (p. 38.)

c. Ganglial cavities. Close cavities of this kind have been observed by M. Velpeau under the jaw, in the region of the parotid gland, in the carotid canals, in front of the larynx, in the supra-sternal fossa, in the axilla, at the bend of the arm, in the groin, in the ham, and in the interior of the pelvis. These cavities, M. Velpeau observes, present two modifications.

> "1. Some of them remaining imbedded in the substance of the ganglion, resemble the close cavities of the thyroid gland; no portion of their circumference, or of their extent, can convey the idea of an ampulla (pouch) or of a membrane. They may be exactly compared to cavities hollowed out in plaster, paste, or any other inert substance. 2. The close cavity, originating near some particular point of the circumference of the ganglion, hastens to escape from the inclosure in which it was formed, to augment itself at the expense of the neighbouring layers: in this case, the ganglion, which is always to be found at some point of the cavity, prevents it from being confounded, on the one hand, with a cavity purely cellular, and, on the other, with the central cavities of parenchymatous organs." (p. 39.)

In neither of these modifications, according to M. Velpeau, can the close cavity be separated from the neighbouring tissues, of which it is, in effect, a portion.

Chapter II is on the "evolution and functions of the close cavities of the animal economy." M. Velpeau here states that the observations now brought forward form part of a general work on ovology, founded directly on observation; and of which another part, namely, that relating to the envelopes of the fetus, the placenta, the vesicles, the umbilical cord, and the external surface of the embryo, has already been published in an isolated form. If our review of the work before us were intended to be strictly analytical, this chapter would demand our special attention, inasmuch as it involves some anatomical details into which our author affirms, and we believe justly, that he has himself been the first to enter. On the whole, however, the tendency of these details being merely to illustrate those statements respecting the early condition of the internal surfaces of close cavities, which were introduced at the commencement of the present article, we are content to recommend them to our physiological readers as an original and careful investigation of the state of the surfaces in question, at various successive periods of intra-uterine life: they do not admit of condensation, and to insert them in full would swell this article to inordinate dimensions. We have the less hesitation in omitting a notice of them, because, as above stated, the inferences derived from them have already been distinctly announced, while the accuracy of the details themselves will be better tested by anatomical research than by any remarks of ours.

Chapter III is on the "diseases of the close cavities." It opens with the following proposition: "No disease originates, properly speaking, at the surface of close cavities; it is always the tissue of which these surfaces form part that is the primary seat of the malady."

"Such a proposition," says M. Velpeau, "which in the eyes of some persons might appear a paradox, if not a mere play upon words, is worthy, in my opinion, of serious discussion." (p. 96.),

Now, notwithstanding the ingenuity of our author's illustrations, we must confess that this same question of surfaces appears to us to partake largely of the nature of a logomachy, and the reason of this we apprehend to be that M. Velpeau has not sufficiently distinguished between a mathematical surface and a surface in the lax and common acceptation of the term. Viewing the thing mathematically, it is quite evident that no morbid action, and no action of any kind, can commence in a surface; for a superficies being, "that which has only length and breadth," is an idea, not a material substance, inasmuch as every portion of matter is extended in the three dimensions of length, breadth, and thickness. But when we speak, in ordinary language, of a "membranous surface," we mean the visible and tangible expanse of a thing which has thickness, however small that thickness may be.

This being premised, we would state the question as follows. It cannot be denied that the serous or synovial surface of a cavity exercises a function that would not be exercised in its absence; it must therefore have a peculiar organization adapting it to that function. Does this peculiarity of organization belong to a distinct and isolable membrane, or is it the result of a change which takes place in the structure of a subjacent tissue as it approaches its free surface? This is a point for the anatomist to decide. Without professing to have entered into any recent investigation of it with reference to M. Velpeau's doctrine, we are strongly inclined to believe that he is, to a considerable extent, correct in his anatomical views; and that the lining membranes of close cavities cannot be fairly separated from the tissues which they invest at so many points, as anatomists generally imagine. But, admitting the truth of our author's observations in their fullest extent, and taking it for granted that the so-called membranous surfaces of close cavities are not membranes at all, but that the peculiarity of organization which fits them for their function, resides in the tissue subjacent to the surface to a small portion of its depth; these things, we say, being admitted, ought we to consider the surfaces in question as distinct tissues, or as part merely of the various tissues which form the parietes of the cavity? In a word, is the recognition of serous and synovial membrane as distinct tissues to be made contingent on their separability from the parts which they invest? It seems to us that the claims of these tissues to independence is in no way affected by their mechanical connexions, nor by the fact of their imperceptible transition into other tissues. We understand, by a distinct tissue, an organized structure, which presents similar or nearly similar characters, both physical and vital, in whatsoever part of the body it may be found, or howsoever it may be distributed in relation to the structural anatomy of parts. Who can separate the tendinous from the fleshy fibre of a muscle? Yet who would maintain that tendon and fleshy fibre are not distinct tissues? The real upshot, therefore, of

M. Velpeau's observations is merely that the internal surfaces of close cavities are not generally separable in the form of continuous membranes as anatomists have imagined; but this fact though admitted, would leave their established relations to general anatomy and physiology nearly untouched.

But we proceed now to examine more particularly the pathological views of M. Velpeau.

"In purely serous cavities," he says, "in the abdomen, and especially in the chest, the tissues of which the serous surface forms the free boundary, are so vascular, and penetrated with liquids, that their inflammations seem to invade, by a similar mode of attack, the surfaces called 'peritoneum' and 'pleuræ,' and appear actually to have their starting point in the parietes (internal surface) of the cavity rather than in the layers situated beneath those parietes (that surface); but it is sufficient to reflect for a moment on the fact, that the vessels and nerves which always proceed from the deep towards the superficial parts, have their course necessarily arrested under the serous surface, whither they come to have their extremities covered; for, as every inflammation requires a vascular and nervous system for its establishment, it follows that inflammations cannot originate in the surface of serous cavities. At the same time, as the inflammatory action quickly modifies (the state of) the sanguiferous capillaries and the cellular texture of the organic layers in which it takes place, it reacts with sufficient rapidity on the serous surface to destroy almost immediately its smooth and humid aspect, and to render it impossible to recognize." (pp. 96-7.)

But we submit that M. Velpeau here falls into the same ambiguity in the use of the term "surface" which we just now pointed out, and all that he says amounts simply to this—a mathematical surface forms no part of that which it is supposed to terminate—a position which no one will dispute.

M. Velpeau seems indeed to have a certain misgiving on this head himself, for he adds,—

"If this doctrine appear to involve only a modification of language when applied to the serous cavities properly so called, it will suffice, in order to give a different idea of the matter, to put it in relation with the tendinous synovial cavities, and above all with the cavities of the joints." (p. 97.)

"On the supposition," says M. Velpeau, "that the synovial cavities are of the nature of sacs, pouches, membranes, sheaths without apertures, comparable by way of illustration to a man's night-cap, and thence concluding that every articulation contains a capsule which lines the free surface of the cartilages and the circumference of the osseous heads, and the interior of the adjacent fibro-cellular envelopes, surgeons have quietly settled it that inflammations, ulcerations, transformations, and degenerations of all kinds occur in the tissue of the synovial membranes, even on the articular cartilages. Accordingly, we find, even in the most recent works on surgery, chapters entitled 'cartilaginous synovitis,' 'ulcers, thickening, fungosities, transformation, degeneration of the synovial membrane of the cartilages.' It is, nevertheless, a fact that not one of these maladies ever commences on the free surface of a cartilage. The synovial cavity not appertaining to a proper membrane, and only existing between the osseous extremities at the expense of the cartilages which form its principal walls, could not be the seat of inflammation or of any other disease, unless the cartilages were themselves susceptible of inflammation or ulceration. Believing that I have elsewhere demonstrated that the articular cartilages are destitute of arterial and venous circulation, having convinced myself by numberless observations of every kind, that they are not the primary seat of the malady, I believe I am stating nothing but what is conformable to facts, in affirming that neither inflammation, nor ulcers, nor fungous diseases, nor transformations, nor degenerations of any kind exist as a primary malady on the free surface of the articular cartilages. That this assertion may not receive an interpretation different from that which I myself give to it, I will add that inflammations, ulcerations, &c. have been seen by myself as by others on the free surface of certain cartilages, so as apparently to confirm the opinions promulgated according to the doctrines of Bichat; but these lesions had not taken their starting point in the place where they were observed, and had already lost their original character. Thus, if an inflammation or any other alteration arise without the circumference of the cartilage, so as to cause the exhalation of a thin layer of plastic matter, this layer may deposit itself between two articular cartilages, become organized and vascular, and even confound itself with the surface of one of them. In this way we have a real membrane, more or less vascular, and free and mobile, in a greater or less degree, over a variable extent of the articular cavity. Nor is there any reason why such a layer should not sometimes attain a considerable thickness, and give the idea of fungosities, of vegetations belonging to the synovial membrane or to the cartilage. If the malady, once established in the tissues which form the contour of the articular extremities, be intense or long continued, it is possible also that it may vascularize them by degrees, so that the substance of the cartilage shall begin to undergo a real organization, and the whole shall at last become gradually organized from the circumference to the centre; but this production of vascularity, this morbid organization, by no means proves that there is a synovial membrane on the diarthrodial cartilages, or that inflammation ever originated on an isolated layer of those cartilages." (pp. 97-9.)

Here, fortunately, we get rid of the question of "surfaces," which has hitherto cast its unsubstantial film over our reasoning, and come to a point of morbid anatomy, which, however, will require further and more exact observation for its solution. But we cannot here suppress our wonder at M. Velpeau's conclusion, that cartilage is not an organized substance, because it has no visible vessels or nerves; a conclusion opposed to the whole scope of recent physiological research. A much more singular position, however, is that a plastic layer situated between two articular cartilages, and which has become organized and vascular, may confound itself with the surface of one of these cartilages, which is, by hypothesis, an inorganic substance. But even this is surpassed by the affirmation that intense or long-continued disease of the tissues surrounding the articular extremities may endow the latter (unorganized bodies according to our author) with vessels, and convert them into organized bodies. Suppose the articular extremities of the bones to be covered with some body that is really not endowed with organization—as caoutchouc for example—does M. Velpeau imagine that a lining membrane could confound its substance with the surface of the caoutchouc, or that vascular parts could penetrate it with their vessels, and communicate to it organization and vascularity, whether morbid or healthy? For our own part, we believe that articular cartilage does possess organization. We believe so, among other reasons, because it is tinged, like bone, by certain colouring matters introduced into the blood, and becomes yellow in jaundice, which would not be likely to happen with so dense a substance as cartilage if it were not organized, because it retains its shape and texture unimpaired while continually exposed to the action of liquids and to friction, which no unorganized body could do unless it were of a stony hardness, and the friction were against bodies much softer than itself; lastly, because we think we have repeatedly seen ulcerations penetrating into its substance, and granulations springing from it: we say we *think* we have seen these things, for other observers, like M. Velpeau, have derived a different impression from the exercise of their vision, which shows that there must be something in the conditions of the case that detracts from the usual certainty of ocular demonstration.

But though it were admitted that articular cartilage had no organization, it seems to us that the admission would be fatal to our author's view of the constitution of the synovial surface investing it. We find, eliminated from this surface a very peculiar secretion, which certainly differs from every other animal fluid. But, according to M. Velpeau, synovia is not secreted by the so-called synovial membrane, but by *cartilage*, of which the supposed membrane is merely the free surface. Again, articular cartilage has no organization; how then can it secrete? M. Velpeau is thus placed between the horns of a dilemma: either the synovial surface is a distinct tissue, or articular cartilage is an organized substance. We speak not here, be it remembered, of the separability or inseparability of the synovial surface as a distinct membrane, a point which appears to us to relate to structural rather than to physiological anatomy.

But it is now time to notice M. Velpeau's views with respect to the obliteration of close cavities by the artificial production of inflammation within them. After some general remarks on the effects of blood and pus effused in the close cavities, M. Velpeau proceeds as follows:

> "The organic process, known under the name of adhesive inflammation, is, in some sort, peculiar to the cellular tissue. Wherever it exists alone (apart from other morbid actions), it is followed by the confusion and permanent soldering together of the neighbouring layers which have been its seat. It precedes, and generally surrounds purulent inflammations, and marches, as it were, before them, making incessant efforts to restrain and circumscribe them, and to preserve the surrounding layers from their ravages. It is, in a word, a protective process, an effort to circumscribe, within the smallest possible space, the heterogeneous matters which are developed among the tissues. But this inflammation is developed the more readily in close cavities in proportion as their parietes are more smooth and completely serous. There, as in the cellular tissue, it causes the opposite walls, which are in contact with each other, to become glued together and united, returning, in some sort, to their embryonic condition, and thus effects a complete obliteration of the cavity. Adhesive inflammation, therefore, purely adhesive inflammation, is the thing most rationally to be wished for in the instance of dropsy or sanguineous effusion in the close cavities. Nevertheless, since this inflammation is in itself productive of great danger, when it occupies very large cavities, as the peritoneum or the pleuræ, it ought not to be rashly and indiscriminately excited in all cases. We know, moreover, that adhesive, becomes easily converted into purulent inflammation, under certain influences. The problem, then, which is now to be resolved, is the following: To excite in close cavities affected with effusion an irritation which shall be *always adhesive*, and which shall *never become purulent.*
>
> "The solution of this problem involves several data. The more the affected fluid resembles serum, the more easy it is to procure adhesive inflammation; but, on the contrary, the more it resembles pus, the less chance there is of escaping purulent inflammation. We hence perceive the propriety of endeavouring to reduce the effused fluid to the condition of serum, if its original character be not serous. In a certain number of cases, this result is obtained by emptying abscesses several times, at intervals, by a simple puncture. The same is the case with accumulations of blood. The cavity suddenly relieved from the presence of the purulent or sanguineous effusion, becomes filled anew, but exhales serum rather than a liquid analogous to that which it contained at first. This process, repeated a certain number of times under the influence of punctures, renders possible the

transformation of a collection of blood or pus, into one which is purely serous, sero-sanguineous, or sero-purulent. Observations, which are already numerous, have convinced me incontestably of this fact, in most regions of the body." (pp. 106-7.)

Arguing from the effect of irritating injections in hydrocele, M. Velpeau felt inclined to try an analogous practice in other cases of effusion within close cavities; but the various substances used by surgeons for such injections appeared to him neither sufficiently mild, nor sufficiently certain in their effects, to answer his purpose. He believes, however, that he has met with the desired agent in tincture of iodine diluted with water. Iodine, he observes, seemed eligible for another reason, namely, that serous effusions in close cavities are often connected with an infarcted state of some parenchymatous organ, in which case iodine might exert its resolvent effect beneficially. He pursued his experiments with a view to ascertain, 1st, if the tincture of iodine, like wine, will cause adhesive inflammation in close cavities; 2dly, if this tincture acts beneficially in the obstructions with which dropsies are sometimes complicated; 3dly, if like wine, it occasions mortification of tissues in which it becomes infiltrated. The first question has been for some time settled in the affirmative by the experiments of M. Velpeau, and various other practitioners. On the second point our author's experience has been equally satisfactory.

"Opportunities have not been wanting to me," he says, "of appreciating the influence of injections of this kind on the obstructions which complicate hydrocele. When the dropsy had been preceded, or was still complicated with a certain degree of infarction of the testicle, the greater part of surgeons recommended the use of general and local means to overcome the infarction, and not to resort to the vinous injection till a later period. Often, again, the infarction being considered the principal malady, the injection was regarded as useless, and, in the absence of other efficient means, the testicle was sometimes sacrificed. I have proceeded quite differently with the tincture of iodine. In hydrocele I commence always by the injection; and if the infarction, though considerable, consist not in scirrhus, encephaloid structure, melanosis, or tubercle, but is in effect merely an hypertrophy, it is almost discussed. In this manner I have obtained results altogether unexpected, in the cure of tumour which had been pronounced sarcoceles by skilful practitioners. On this point I possess numerous detailed observations." (pp. 110-11.)

With reference to the third question, namely, whether the effusion of the iodine injection into the cellular tissue is followed by gangrene, M. Velpeau observed, in seven or eight cases, that, during or after the operation for hydrocele, the injection became effused between the skin and the other tissues of the scrotum, without inducing any considerable inflammation, or at worst only occasioned a very limited suppuration. To remove all ambiguity, he injected the tincture of iodine, diluted with water, into the cellular tissue of a certain number of dogs, rabbits, and guinea-pigs, and in no instance did gangrene or suppuration, or any other bad consequence follow.

M. Velpeau recommends one third of the tincture of iodine to two thirds water as the best proportions for the injection, which he represents as rather more efficacious when used cold than warm.

He then records a few cases, and alludes to many illustrative of the success of the iodine injection in the following diseases:

1. Encysted collections of serum in the tunica vaginalis.
2. Collections of serum in the tunica vaginalis, which communicate with the cavity of the abdomen, forming what is called congenital hydrocele.
3. Serous collections within a hernial sac, whether the sac be continuous with the peritoneal cavity or otherwise.
4. Encysted serous collections of the spermatic cord.
5. Serous collections in the external genital organs of women, contained in close cavities, and resembling the last mentioned.
6. Serous collections in the lymphatic ganglia of the groin and iliac fossa.
7. Collections of purely liquid blood in the interior of the pelvis in women.
8. Old collections of blood, entirely liquid, in the tunica vaginalis.

We regret much that M. Velpeau has not thought it necessary to give any detail of the cases in which he has thrown the iodine injection into hernial sacs communicating with the cavity of the peritoneum. He has, in fact, not even mentioned their number. With reference, also, to the somewhat parallel case of congenital hydrocele, though he alludes to ten cases treated with the most perfect success, he gives no details of any one of them. We confess that nothing short of the most ample and particular evidence could convince us of the safety of M. Velpeau's practice in such cases.

At p. 125, the reader will find the case of a lady, in whom a collection of blood had formed behind the uterus, ascending towards the right iliac fossa. The diagnosis formed by MM. Velpeau, Andral, and Faivre did infinite credit to their sagacity, since the operation, performed by M. Velpeau, sufficiently evinced its accuracy; nevertheless, we think the operation was rashly undertaken, as the diagnosis could not possibly be certain, though it happened to turn out correct. The cure was completed by the use of the iodine injections.

In the same manner the author treats of the diseases of the subcutaneous and accidental close cavities. Under the head of "goitre," he gives the details of four cases, three of which were collections of fluid in the thyroid body;—the seat of the effusion in the fourth case is not distinctly indicated. One of these cases—a collection of serum in the thyroid—afforded the only instances in which M. Velpeau has known constitutional disturbance to result from the use of the iodine injection. This consisted in a febrile movement of the whole system, severe headach, indigestion, and jaundice. These symptoms lasted fifteen days. The operation was eventually successful.

We pass on to the treatment of effusions in the articular and visceral cavities. M. Velpeau, fearful of dangerous consequences, long hesitated to throw the iodine injection into the joints. He tried it first in two cases of bursal tumour in the ham, the communication of which with the cavity of the knee remained doubtful. Encouraged by the absence of any evil results in these cases, he practised the operation in one of distinct hydarthrosis with perfect success, the patient retaining the complete use of the joint. The details of this case are followed by those of six others, in which the operation was successful, and one in which it had every appearance of proving so, when the patient abruptly left the hospital. We may remark, by the way, that these cases, however interesting to the practical surgeon, are anything but illustrations of the principle with which M. Velpeau set out, namely, that the iodine injection induces adhesive inflammation between the opposite surfaces of close cavities: the only cases connected with the joints which could illustrate this operation, would be those in which anchylosis was intentionally induced by the use of the injection. M. Velpeau's cases afford, nevertheless, important instances of the power of the means in question of altering the morbid action of secreting surfaces. Connected with this subject our author introduces a controversial section relative to priority in the use of the iodine injection in diseases of the joints, and claims for himself the priority, while he accords equal originality to M. Bonnet, who was not aware of his own experiments. This is quite out of place in a work constructed on the principle announced at the commencement of the treatise before us. This principle is also left out of sight in the observations on the injection of the *visceral cavities;* for our author, having made no experiments on the human subject, merely discusses the matter theoretically, and alludes to the cases recorded by Warren, L'Homme, Jobert, and Van Roos Broock, none of which gentlemen have any business here—seeing that we are in the realms of simple observation. M. Velpeau gives us, however, the details of some recent experiments on dogs, in which the injection was thrown into the peritoneal cavity. The general results are as follows:

1. When the injection was made with a third, fourth, or fifth proportion of tincture of iodine, the animals always died; but when it was made with a seventh or less, they all recovered.

2. Death was, in all instances, preceded by symptoms of violent inflammation, either of the peritoneum or intestines. There was no reason to suppose that the animals were poisoned by the absorption of the iodine.

WE now draw towards the conclusion of M. Velpeau's book. In offering a general opinion of its merits, we would observe: 1. That the anatomical views it develops are probably true to a considerable extent, but that they are not borne out by sufficient exactness of detail, parts being asserted to be inseparable, without any reference to the anatomical means used to separate them. 2. That the physiological inferences appears to us to be of little importance, because, except in reference to the acceptation of terms, they leave matters very nearly where they were before. 3. That the pathological conclusions are by no means established, but should nevertheless lead to the careful revision of some points presumed, perhaps too hastily, to be ascertained. 4. That the therapeutical portion would have been much more valuable if backed by a more exact reference to cases. 5. That M. Velpeau has done injustice to his own ideas by giving them to the world in language which is too diffuse, and yet hard to understand from its abounding with words of vague signification; lastly, that, with all its faults, the book is well worthy of perusal —a fact for which the very name of the author is a sufficient guarantee.

Art. VI.

1. *Principles of Medicine: comprising General Pathology and Therapeutics, and a brief general view of Etiology, Nosology, Semeiology, and Prognosis.* By Charles J. B. Williams, m.d. f.r.s., Professor of the Principles and Practice of Medicine, and of Clinical Medicine; and First Physician to the Hospital, University College, London, &c.—*London*, 1843. 8vo, pp. 390. Article, *Inflammation.*
2. *The Actual Process of Nutrition in the Living Structure demonstrated by the Microscope; and the Renewal of the Tissues and Secretions, with the Phenomena and Products of Inflammation, illustrated and established.* (*Second Series of Experimental Researches.*) By William Addison, f.l.s, &c.—*London*, 1844. pp. 76. With two Plates.

During the interval which has elapsed since we last formally treated the subject of Inflammation, much addition has been made to our knowledge of the nature of this condition;—partly by means of the microscrope, which has furnished some new facts in regard to the condition of the corpuscles floating in the blood of the inflamed part, and in the products of the inflammatory action; and partly by means of analytical processes of a simple kind, which have made certain that which was before only suspected, in regard to the altered proportions of the fluid elements of the blood. We think it time, therefore, to bring the subject again under the notice of our readers; and the publication of the works named above affords us an advantageous opportunity for doing so. The general character of Dr. Williams's treatise, and most of the subjects embraced in it, have been noticed in our preceding Number; but we have reserved the portion relating to Inflammation for more detailed discussion,—as well for the sake of combining with it a notice of Mr. Addison's researches, as because we think we can connect with it some views of our own, which may help to elucidate this most perplexing subject.

Our general appreciation of the merits of Dr. Williams's work has already been expressed; and we need not, therefore, repeat it here. We shall only remark, that the portion now under consideration fully bears out the encomiums we passed upon the remainder; excepting that its value is impaired, in our estimation, by the adoption of the same notions, in regard to the forces which move the blood, as those we have already commented on in the sections on Congestion and Determination. The sketch of the whole subject, however, is drawn with the hand of a master; and on account, both of its scientific and practical value, is worthy of the most attentive perusal.

A tendency has manifested itself among many recent physiologists and pathologists (amongst whom Prof. Alison has taken the lead, as in so many other improvements), to regard the act of Inflammation as nothing else than an altered form of the ordinary Nutritive process. This, we are well convinced, is the true mode of viewing it. That it has not been so considered, and that attention has been so long and so exclusively fixed upon the state of the vessels, rather than upon that of the blood which they carry, and of the tissues which they penetrate, is, we are satisfied, the true explanation of the absence of profitable results, notwithstanding the vast amount of time and attention which has been given to the observation of the process. It is only by obtaining clear and positive ideas of the ordinary phenomena of Nutrition, that we can comprehend those alterations which, in a collective form, are spoken of as constituting Inflammation. Our knowledge of this department of physiology has received many accessions during the last few years; and the time seems to us to be arrived for its application to pathological inquiry. It will not be inappropriate, therefore, if we commence our exposition with a critical analysis of Mr. Addison's memoir; which we shall endeavour to treat with strict justice. We regret to find that our comments on his former paper were regarded by Mr. Addison and his friends as less favorable than its deserts required; but an attentive reconsideration of them, with all the aid derived from subsequent inquiry, and from the perusal of the present continuation, has only added increased force to our conviction, that the memoir to which we refer contained many statements and inferences, which will not stand the test of further investigation. And we cannot but think that our readers will be disposed to take a similar view, by the fact, that the present memoir is entirely devoted to the further elucidation of that subject, which we formerly noticed as the one in which we considered Mr. Addison's researches as most successful and satisfactory—the characters and functions of the colourless corpuscles of the blood. In bringing it under their notice, we shall offer no objection to the statements which Mr. Addison has recorded as *facts*—these being few, not inconsistent with generally-received doctrines, and easily verified. But we shall feel called on to inquire, how far the facts observed by him warrant the *inferences* which he has deduced from them. And if we do this with more strictness than the occasion may seem to warrant, we would say, in excuse, that the mode of reasoning adopted by Mr. Addison would, as we believe, if followed out in other directions, lead to the utter subversion of all philosophy.

The points which Mr. Addison considers himself to have established by his former researches on this subject, may be stated as follows:

1. That the colourless corpuscles exist in blood of man under all circumstances, and are constantly circulating through the capillary vessels, to the walls of which they have a tendency to adhere.

2. That they exist in great numbers in the blood of inflamed parts; and that they may be seen accumulating in the irritated vessels of a frog's foot, and showing an increased tendency to adhere to their walls.

3. That they exist in great numbers in the buffy coat of the blood.

4. That the liquor sanguinis—especially that of inflammatory blood—*fibrillates* in coagulating; so that a thin film of it presents all the structural characteristics and physical properties of fibrous or membranous tissue.

5. That lymph and pus-globules, exudation-cells, and epithelium, are altered forms of the colourless corpuscles.

With respect to the first four of these positions, we do not entertain the slightest doubt, since Mr. Addison's observations in regard to them coincide fully with those of other microscopists: and we fully agree with him in his estimate of their importance, believing that they will form the foundation of a novel physiological and pathological superstructure. But if we should venture to differ from Mr. Addison as to the plan on which this should be raised, we can only say that we desire our readers not to imagine us to affirm dogmatically that Mr. Addison is wrong, and that we are right; but merely to consider us as discharging our critical duty, in pointing out what we consider the weak points of Mr. Addison's argument. The *fifth* position taken by Mr. Addison, is the one to the "demonstration" of which this memoir is particularly devoted.

"By the application of several reagents," he tells us in his commencement, "and by comparing their effects upon the colourless blood-corpuscles, with those resulting from their application to pus-globules, I came to the conclusion that lymph and pus-globules, exudation-cells, and epithelium, originate from the colourless corpuscles. This conclusion was strengthened by the fact, that the fibrin of the liquor sanguinis was never seen, during the progress of fibrillation, to give origin to a corpuscle or globular particle of any kind.* The chief difficulty in establishing this conclusion arises from the doctrine, that all the blood-vessels have *permanent tubular parietes*. My object on the present occasion is to show the nature of these tubular parietes, the changes they undergo, and to point out how the colourless blood-corpuscles are included in their structure." (p. 3.)

The first point to which Mr. Addison directs our attention is the character of the normal fluid contents of the colourless corpuscles; and this he considers to be established by the following experiment, which we have ourselves repeated with a similar result:

"Provide six or eight slips of glass, such as are usually employed for mounting microscopical objects, and as many smaller pieces. Having drawn blood from a person with rheumatic fever, or any other inflammatory disease, place a drop of the colourless liquor sanguinis, before it fibrillates, on each of the slips of glass; cover one *immediately* with one of the smaller slips, and the others one after another, *at intervals of thirty or forty seconds*; then, on examining them by the microscope, the *first* will exhibit the colourless blood-corpuscles in various conditions, and numerous minute molecules distributed through a more or less copious fibrous network; and the *last* will be a tough, coherent, and very elastic membrane, which cannot be broken to pieces nor resolved into smaller fragments, however roughly or strongly the two pieces of glass be made to rub against each other. This is a 'glaring instance' of a compact, tough, elastic, colourless, and fibrous tissue, forming from the colourless elements of the blood: and the several stages of its formation may be actually seen and determined. Numerous corpuscles may be observed in all these preparations to have resolved themselves, or to have fallen down into a number of minute molecules, which are spread out over a somewhat larger area than that occupied by the entire corpuscles; and although still retaining a more or less perfectly circular outline, yet refracting the light at their edges in a manner very different from that in which the corpuscles themselves are seen to do. It is from these and various other larger and more irregular masses of molecules or disintegrated corpuscles, that the fibrinous filaments shoot out on all sides, as from so many centres; or frequently the filaments are more copious in two opposite directions. It is highly probable that the molecules and the plastic fibrillating liquid are both derived from the interior of the colourless blood-corpuscles." (pp. 4-5.)

From this and other corresponding observations, Mr. Addison draws the conclusion, that neither the fibrin or albumen of the circulating blood are diffused through its fluid portion, or liquor sanguinis; but that they are both contained in the colourless corpuscles; of these he imagines that a large proportion burst or become ruptured immediately that the blood is drawn from a vein, owing to the sudden change of temperature to which they are exposed, or from other causes; and that they set free the liquor sanguinis, which rises to the surface, drawing up with it the colourless corpuscles which have hitherto preserved their integrity:

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* In reference to this statement, we would remark, that Mr. Addison's observations seem to have been made upon fibrin withdrawn from the body, and coagulating upon a *dead* surface. Under such circumstances, it is not surprising that the cell-germs which the fluid may contain, should not be able to develope themselves into cells; but the history of the organization of fibrinous effusions poured out upon *living* surfaces seems to show that such a development *may* and *does* take place.

> "Accordingly, neither the fibrinous element nor the serum circulates in the blood, as part of the fluid in which the red corpuscles are suspended in the living vessels; they are both inclosed within, and form, incorporated together, an essential ingredient of the interior contents of the colourless corpuscle. Hence, therefore, the fibrin can no more be said to be elaborated from the albumen, than the stone or kernel of a peach can be said to be elaborated from the fruity pulp—they both grow together; so likewise the fibrinous element and the albuminous element grow or are elaborated from other sources: if they bear a due proportion to each other, the corpuscle or cell will be normal, and its subsequent function will be healthy; but if one element preponderate over the other, the reverse of this must happen. What, then, it may be asked, is the nature of the liquid in which the corpuscles move when in the living vessels? It is impossible to determine: for it cannot be procured for experimental examination without being mingled with corpuscles, nor, according to these views, without some of them bursting and mingling with it their own contents." (pp. 7-8.)

The startling doctrine here enunciated is somewhat qualified in the following note, attached to the end of the memoir: "It is not intended to deny that the fluid in which the blood-cells float, in their passage through the living vessels, is not, or may not be, an albuminous fluid; all that is meant by the affirmation is, that it is *not serum*, though clearly it must be mingled with, and form part of the bulk of this fluid." By this we presume that we are to understand, that *a part*, if not the whole, of the albuminous element of the serum is derived from the rupture of the colourless corpuscles; and that *all* the fibrin of the circulating blood is contained in them. Now, upon this we shall simply remark, that the doctrine seems to us entirely hypothetical; to be *not required* by any of the *facts* adduced by Mr. Addison; and to be inconsistent with many others, very well established. For the experiment we have quoted, and others of a similar character, only go to render it highly probable (for we scarcely think that they demonstrate) that the fluid contents of the colourless corpuscles, which they yield in bursting, are of a peculiarly *plastic* or organizable character; in other words, that they are of a *fibrinous*, rather than of an *albuminous* nature. They do *not* prove that *all* the plastic matter of the liquor sanguinis is contained by them, whilst the blood is circulating through the living vessels; still less that any considerable proportion of albumen can be shut up within them. And that such is the case, seems to us altogether disproved by a comparison of the relative amounts of the different elements of the blood. For, on watching the circulation in the living animal, the number of colourless corpuscles which traverse the field is by no means sufficient to hold the quantity of fibrin and albumen (probably not even that of the former alone), which we know to exist in the amount of blood that has flowed through the vessels during our observation. For it must be remembered, that the contents of these corpuscles are *fluid;* and that the numbers which are used to represent the proportion of fibrin in the blood, refer to its *dry* state, in which its bulk is of course far less. Mr. Addison further objects to the doctrine, that fibrin is elaborated from albumen; and speaks of them as both elaborated together, from other sources. We were not aware that albumen needed *any* elaboration; having always been accustomed to regard it as derived immediately from the food; and until Mr. Addison can show us that the albumen of the blood differs from that of the chyle, or that the fibrin is derived from any other source than the albuminous element, we shall take leave to consider it as an established position, that the fibrinous element of the blood is nothing else than albumen in process of preparation or being organized; that is, becoming endowed with plasticity and certain other peculiar properties which albumen does not possess. The doctrine, that the elaboration of the plastic element is performed by the agency of the colourless corpuscles, which was first propounded by Dr. Carpenter in this Journal, (vol. XV, p. 273,) seems to us very probable; and our readers can scarcely fail to perceive what striking confirmation it receives from the experiment we have just quoted. We believe that most physiologists, who are not prejudiced by the seductive simplicity of Schwann's generalization as to the derivation of all tissues from cells, are now arrived at the conclusion that, as regards the areolar and other simple fibrous tissues, no other explanation of their production need be looked for, than the known tendency of the particles of fibrin to arrange themselves in a linear manner, so as to form fibres; which tendency manifests itself much more decidedly, when the consolidation takes place upon a living surface, than upon a dead one. Mr. Addison's observation on the mode in which this takes place in buffy blood, seems to us to give a most satisfactory explanation of those appearances, which have led Henle and other microscopists, who did not coincide with Schwann in the idea that the fibres are derived from *cells*, to assert that they were produced from the *nuclei of undeveloped cells*. In the assistance which he has given towards making these important corrections, we regard Mr. Addison as having done good service to physiology.

Mr. Addison next proceeds to explain his views of the relation between the *liquor sanguinis* (or, according to him, the normal contents of the corpuscles), and *mucus* and *pus*. He states, that not only may we see in mucus numerous molecules and colourless globules, but that it frequently exhibits a copious and distinctly fibrous character or structure, and always does so when a little dilute *acid* is added to it—an addition which cannot *form* the fibres. On the other hand, if a few drops of the liquor sanguinis from buffy blood, before fibrillation, be treated with *alkali* (liquor potassæ), it becomes quite clear, transparent, and colourless, in consequence of the rupture of all the white corpuscles; and the mixed materials become semi-solid, and resemble a glairy mucus. Further, if a drop of blood be treated in a similar manner, the red corpuscles also rupture, and the resulting liquid has a transparent, brownish tint. On subsequently mixing a drop of dilute acetic acid with either of these mucus-like liquids, it is rendered opaque, and displays a more or less evident fibrous arrangement, according to circumstances. When a drop of white and opaque healthy pus is placed upon a slip of glass, and is well mingled with a drop of liquor potassæ, it entirely loses its opaque character, and becomes clear and transparent, resembling mucus;

with such a degree of tenacity and elasticity, that it may be drawn out into strings or filaments, six or eight inches long. On placing it under the microscope, it is seen to contain numerous molecules and granules, but no entire corpuscles, all having been ruptured by the alkali. But on mingling a drop of dilute acetic acid with this, it shrinks up, and resumes somewhat of its former opaque, white aspect; and on submitting it to an examination by the microscope, when pressed into a thin film, it exhibits as copious a fibrous structure or character as the fibrillated liquor sanguinis. Similar observations are added at the close of the memoir, in regard to the changes produced in animalcules, and in certain cells from the earthworm, by the agency of liquor potassæ and acetic acid. Mr. Addison adds some other observations, in regard to the effects of reagents upon the three kinds of corpuscles respectively; and from the similarity of these actions, he draws the conclusion, that pus- and mucus-globules are nothing else than altered forms of the colourless corpuscles of the blood. The following quotation fully enunciates his conclusions on this subject:

> "1st. That the plastic fibrillating liquid, denominated liquor sanguinis, exists as a fluid within the colourless blood-corpuscles, and that when it escapes from them it forms an elastic fibrous tissue, the serum being the residual liquid.
>
> "2d. That mucus- and pus-globules are altered colourless blood-corpuscles; and that the glairy fluid termed mucus is nothing more than an altered state of the fibrillating liquor sanguinis; the change from one to the other being coeval with the changes which characterize the microscopical aspect of the corpuscles. Hence, if we take the red portion of the buffy clot, and the red blood-corpuscle, to represent blood, then the colourless layer of liquor sanguinis, with the colourless blood-corpuscle will represent the first remove from blood, and mucus or pus, with the mucus- or pus-globule, will be the next. And it would appear generally, that the nearer the corpuscle is to, or the fewer the stages of its removal from, the circulating fluid, the more nearly it resembles the colourless blood-corpuscle, and the more decidedly and visibly its fluid contents, when they escape, fibrillate; whereas, the further the corpuscle is, or the greater the number of stages of its removal from the circulation, the larger it is, the more it is filled with molecules, and the less perfectly do the fluid contents fibrillate. Now, if it be admitted—of which I have myself no doubt—that the fibrillating liquor sanguinis is changed into mucus in the interior of living cells, then there can be no difficulty in admitting that similar living cells, by a different mode of elaboration, may not only form sundry kinds of fibrous or mucous tissue, but the tears, saliva, milk, or bile." (pp. 15-16.)

By these and other observations, Mr. Addison has been led to entertain the following new doctrine of Secretion:—that the cells by which (as it is now generally admitted) the secreted products are elaborated, are not developed in the secreting organs themselves, at the expense of materials supplied by the blood; but that they are neither more nor less than the colourless corpuscles of the blood, which elaborate these products while still floating in its current, and escape from the vessels in a manner to be presently explained. He appears to think that in this mode he gives a better account of the matter, than that afforded by the ordinary doctrine; which attributes the separation of particular products by particular glands, to "some peculiar transcendental and purely hypothetical selective process of exudation, through a structureless and transparent tissue." As we are of an entirely different opinion, we shall endeavour to make clear to our readers the grounds on which we think that the doctrine has no claims to acceptance.

It will be useful, as a preliminary, to arrange the products of secretion under two heads: *first*, those which result from the decompositions constantly taking place within the body, and which must be eliminated from the blood, in order to preserve its purity; and *second*, those which are intended to serve some particular purpose in the economy. The chemical characters of the former group—including the bile, urine, and the fetid secretions of the intestinal glandulæ—are entirely different from those of the blood; and it cannot be imagined that they can result from any simple metamorphosis of its elements. Moreover, if their elimination be checked, they accumulate in the blood, and then find their way out of the vessels, through other tissues and surfaces, by a simple physical process of exudation. But the fluids of the latter class, such as the lacrymal, salivary, pancreatic, and mucous secretions, do not differ from the blood in anything like the same degree; and may be easily conceived to be generated from it by a simple action, such as that by which Mr. Addison produces a kind of artificial mucus from the liquor sanguinis. They do not seem to accumulate in the blood, if their production be checked; and, consequently, there does not seem any occasion for supposing them to be pre-formed in it. All are agreed that the elimination of both kinds of secretions from the blood is a consequence of cell-growth: but the general opinion is, that cells developed in the substance of the liver have the power of selecting biliary matter; that cells developed in the tubuli uriniferi have the power of selecting urinary matter;—and so on of other glands. Now, on Mr. Addison's hypothesis, (for such we must term it, since there is not a shadow of *direct* proof offered by him—the only argument in its favour being the resemblance between certain characters of some of these secreting cells, and those of the colourless corpuscles of the blood,) we must regard the blood as containing not only the elements of all these secretions ready formed, but also the cells which eliminate them; and that, of the corpuscles which are commonly ranked under the appellation, "colourless," some are engaged in elaborating bile, others urinary matter, others milk, &c. Here, then, is a process of selection just as "transcendental" as that which is commonly supposed to take place in the glands themselves, and a great deal more hypothetical.—But further, it is necessary to suppose, on Mr. Addison's theory, that all the cells concerned in the separation of biliary matter find their way out through the liver, in virtue of some peculiar power which determines them to that organ;—that all the cells which secrete urine find their way out through the kidneys, for a similar reason; and so on of other glands. Hence, then, Mr. Addison's theory really requires *two* sets of "selective processes:" one by which the cells take up the product to be separated, and the other by which the gland takes up its particular class of cells;—whilst other physiologists are satisfied with but one. But he may affirm that we have misrepresented his opinions, in the assumptions to which we have shown that they lead; for that he would not assert that any of the colourless corpuscles floating in the blood hold the products of secretion, but rather that they become charged with them in passing out of the current through the glands. This *might* be admitted (if other circumstances rendered the hypothesis favorable) in regard to the secretions of the second class; as it might be supposed that the liquor sanguinis and colourless corpuscles became changed, in passing through a mucous membrane, into mucus and mucus-corpuscles, by some special influence there exerted; or into salivary matter, when passing through the salivary glands. But it *cannot* be admitted in regard to the bile and urine—of the pre-existence of whose elements in the blood, we have the most abundant evidence. Either, therefore, these matters must be contained in some of the corpuscles of the circulating blood, which is a necessary part of Mr. Addison's doctrine of secretion; or they must form part of its fluid ingredients, and must be separated from the liquor sanguinis by the cells developed within the several organs through which they are eliminated. In support of the latter doctrine, it appears to us that every analogy may be urged; besides the palpable objection which must occur to every one in regard to the former,—that we have no ground whatever to suppose that the secreting cells pass out *bodily* from the capillary vessels, and find their way through the basement membrane of the tubuli or follicles, impervious, as it would appear, to take their place as epithelium-cells on their lining.

We cannot find that any argument in favour of this doctrine is supplied by Mr. Addison's researches, save that which rests upon the similarity of the colourless corpuscles and the epithelium-cells, as to their aspect when viewed with the microscope, and their behaviour (as the chemist would say) when treated with certain reagents, especially the liquor potassæ. Mr. Addison objects to an observation in our critique on his former Researches, to the effect that there is something too specific in the *character*, if not in the *appearance*, of pus-globules, to admit of the conclusion that they are altered colourless corpuscles; and he remarks, "the only possible way that I am aware of, in which pus-globules can present a sensible character, is through the medium of the microscope," which renders it "impossible to separate, as regards these minute objects, *character* from an *appearance*." (p. xix.) Now, the distinction between the two seems to us obvious enough; most objects having a number of *characters*, of which their *appearance* to the eye presents to us but a small proportion. And we consequently maintain, that we have no right whatever to assert the *identity* of such objects as those we are now concerned with, by their *appearance* only; and that we must take into account their other *characters*. We think it would be quite possible to place before Mr. Addison a dozen cells, among which he should not be able to distinguish the slightest difference, either as to microscopic *appearance*, or as to behaviour with liquor potassæ or other reagents. And yet the *characters* of those cells might be most completely and specifically different; for one might be the germ of a zoophyte, another that of an oyster, another that of an insect, another that of a fish, and so on, up to man; each cell requiring a certain peculiar set of conditions for its development, and producing a certain definite structure when supplied with those conditions. On Mr. Addison's mode of reasoning, all these cells must be identical in *character*, because his microscope does not reveal to him any distinguishing appearances amongst them; the cell which forms the zoophyte, therefore, should be able to produce a man, if placed in the appropriate circumstances; and the germ of man might be degraded to the production of a zoophyte. We hope that this "glaring instance" may satisfy Mr. Addison that our distinction is *not* without a difference; and that he may now be prepared to understand our objection to considering the various kinds of epithelium-, pus-, and mucus-corpuscles as altered forms of the colourless corpuscles of the blood. It seems to us, that if we find one set of cells invariably elaborating a certain product, and another set constantly elaborating another product, a distinctive *character* is thus furnished to us between the two; which should outweigh all that the microscope can reveal to us in regard to their identity of appearance. For, to assert that the microscope is able to reveal to us the whole history of cell-life, and to enable us to determine, by simple inspection, what is the function which any given cell is destined to perform, is to claim for it, as it seems to us, a most undue degree of power as an instrument of research. We have lately been comparing, with some degree of attention, the various forms of epithelium- and cancer-cells; and we have come to the conclusion, that while some of these forms are sufficiently unlike one another to enable them to be separated by a definition, others are so nearly allied as to render it impossible to determine, from their appearances, to which group they belong. Yet every one knows that there are *characters* by which the two are distinguished—cancer-cells being remarkable for their extraordinary reproductive power, which gives the character of malignancy to the tissue they generate; whilst epithelium-cells have no such capability. Yet, upon Mr. Addison's system of reasoning, a tumour consisting entirely of accumulated epithelium-cells (and such we have seen) must be regarded as cancerous in its nature.

We trust that we have now sufficiently explained ourselves upon this point, and that we have shown the fundamental error of Mr. Addison's reasoning. As we have before remarked, we set a high value upon his *observations;* and we hope that it is something better than the influence of pre-formed opinions, which leads us to object to his inferences. We must now inquire into the evidence on which Mr. Addison rests his assertion, that the colourless corpuscles escape from the vessels, for the purpose either of nutrition or secretion. As far as we can gather from his observations, it is the following.—In the capillary vessels of the embryo, there is no definite lining membrane; but altered colourless corpuscles are seen incorporated with the fibres which form their walls. This statement is illustrated by a very beautiful drawing of a blood-vessel from a fœtal hare. In the transparent larva of an insect, a similar appearance was noticed by Dr. Carpenter, and communicated by him to Mr. Addison. In regard to these two facts, we must object to their being used to prove anything in regard to adult structure; since it is well known that, at the commencement of the formation of the vessels in embryonic structures, (those of the larva of an insect ranking under that category,) they are mere intercellular passages, without any definite walls whatever; and that these are gradually formed, during the same time that a process of metamorphosis is taking place in other structures. The next class of facts adduced by Mr. Addison, has reference to certain appearances seen in the adult structure. "Colourless corpuscles," he says, "and little masses of detached molecules, are found incorporated among fibres in all growing membranes, in the fibrous walls of the blood-vessels, in the buffy coat of the blood, and in mucus. There is a gradual transition between the colourless blood-corpuscles in the interior of the blood-vessels, and the lymph- and pus-globules, the exudation and epithelial cells on their exterior." (p. 28.) That the cells within the vessels, and the cells on their exterior, are identical, is, as we have already endeavoured to show, just the thing *to be* proved. The assumption that they *are* so, appears to us but a very poor foundation for the inference, that the walls of the capillary vessels have pores large enough to allow these bodies to pass out. The utmost extent to which Mr. Addison has witnessed any such process, appears to be that specified in the following quotation:

> "But it may be urged, as an objection to this theory of nutrition, that as we *can see* the colourless corpuscles in the irritated web of a frog's foot adhering to the tissue, why *do we not see* them passing through it, and forming fibres or epithelium? To this I answer, that the nutritive changes or processes are too slow in this example for us to follow, from beginning to end, all the actual stages of nutrition; the corpuscles go on congregating in the irritated tissue for an hour or two. Nevertheless, the epithelium of the web and the walls of the capillaries have their visible characters gradually changed during the observation; and numerous corpuscles, more or less altered in shape, may be seen mingled with or buried in the living tissue. This is as far, perhaps, as we are able to go in *seeing* the process of nutrition; and yet, if the part were patiently watched for a sufficient length of time, it is very possible that still more of the process might be seen. Moreover, that the great accumulation of the lymph-globules or colourless blood-corpuscles in the capillary channels of the web of the frog's foot, after immersion in warm water, their adhesion to, and incorporation with the tissue, is a 'glaring instance' of an acceleration of the normal process of nutrition, is in my opinion, substantiated by the fact, that, in the course of a day or two after the application of the warm water, the cuticle or epithelium of the web, composed of pentagonal or hexagonal scales, with their well-known nucleus in the centre, looking like a lymph-globule, peels off in large flakes—an event perfectly according with the theory, as it would not have happened so speedily, except for the application of the irritant." (p. 32.)

In like manner, the exfoliation of the skin in scarlatina is regarded by Mr. Addison as due to an accumulation of the colourless corpuscles in the blood, in consequence of the disorder of the processes of nutrition and secretion; and he thinks that these cells are thrown off from the surface, either by a desquamation of cuticle, or (in the less favorable forms of the disease) by muco-purulent discharges from the internal epithelial surfaces. A number of other well-known facts are adduced by him, in support of his doctrines; but we feel justified in affirming, after an attentive examination of them, that they are at least equally consistent with the doctrines commonly received, as to the presence of morbid matters in the blood. And we shall leave it to our readers to determine whether Mr. Addison can be said to have "demonstrated" the process of nutrition in the living structure, as he conceives himself to have done.

We shall allude to but one more topic before we dismiss Mr. Addison's memoir; namely, some observations on which he lays great stress, and which are certainly of much interest, relatively to the *motion* of the molecules contained in the colourless blood-corpuscles, in pus-cells, and in epithelium-cells. These motions have been frequently seen by him, when the cells have been made to discharge their contents by means of liquor potassæ; and he has also noticed them, in many instances, in the molecules still contained in the interior of the cell. Thus, in the mucous globules found in saliva, he has seen myriads of molecules in the most active state of motion, "almost reminding one of the busy scene of an ant-hill:"

> "The globules in which this remarkable appearance presents itself are always perfectly circular, and have a uniform molecular aspect, free from any conspicuous granules, nuclei, or discs. This observation has directed my attention more particularly to the cause of the innumerable and various appearances presented by pus-globules; and I now believe that the irregularity in shape and outline, and the appearance of conspicuous granules, nuclei, or discs, either in these globules, or in mucous globules, is connected with the cessation of the active movements of these minute molecules. The perfectly-circular and uniformly-molecular mucous globule is a living cell; while those globules presenting the characters which have hitherto been considered characteristic of pus-globules, are, in my opinion, dead ones." (p. xvii.)

Such motions are by no means uncommon, especially in the cells of vegetables. We have ourselves repeatedly witnessed them; and fully agree with Mr. Addison in regarding them as altogether different from those molecular motions, which most minute inanimate particles exhibit under the microscope. We are inclined to think that they might be found in *all* cells, at a certain period of their development; and we cannot therefore regard their existence as of any value in identifying cells, whose other characters are different, with each other.

We shall now express, in a concise form, our own views of the nature of the NUTRITIVE PROCESS, in such a form as to admit of comparison with the abnormal condition of that process in Inflammation.

I. *a.* The BLOOD supplies to the solid tissues the materials of the reconstruction, which is continually going on during their life; as well as the elements of their secretions, whether natural or morbid. *b.* The *fibrin* of the blood is the chief, if not the sole material for the formation or reconstruction of tissue; being, in fact, nothing else than *albumen in the act of becoming organized.* The change by which it is produced is one of a peculiarly *vital* nature. *c.* Fibrin, when duly elaborated, has the power of spontaneously assuming, by its coagulation, (which must be regarded as an act of vitality,) an organized form, of a low character—simple fibrous tissue. This *organizability* or *plasticity*, therefore, is the distinguishing property of fibrin. *d.* On the presence of this element in the blood, depends its capability of supplying the materials for the formation of new tissue; and the plasticity of the blood, taken as a whole, depends upon the proportion of well-elaborated fibrin which it contains. *e.* The elaboration of fibrin is an act performed by the *white* corpuscles of the blood, which bear a pretty constant relation to it, in their proportional amount, under different circumstances. *f.* The *red* corpuscles are to be regarded as chiefly, if not solely, ministering to the function of respiration; and as not essentially connected with the act of nutrition, otherwise than by supplying oxygen to the tissues and to the other constituents of the blood, and by conveying away their carbonic acid. *g.* The *albuminous* matter of the blood may supply the materials for certain secretions; but it does not immediately assist in the production of tissue, being in itself unorganizable or aplastic. *h.* The *liquor sanguinis* of the circulating blood, containing the fibrin as well as the albumen in solution, is the portion of the fluid essentially connected with the formative processes. Its fibrin is being continually withdrawn from it, in the healthy state of the body, by these actions; and is being resupplied as continually, by the elaboration of the albuminous material, through the agency of the white corpuscles.

II. *a.* The various solid TISSUES which are in continual process of change, more or less rapid, derive the materials of their reconstruction from the blood, especially from its fibrin; which they have the power, by their vital endowments, of causing to assume their own respective forms of organization. *b.* The vitality of the tissues in any part may vary in its degree; so that their formative power may be increased or diminished. When their formative power is increased, the process of nutrition is performed with unusual rapidity, and the fibrin of the blood is rapidly drawn from it; but when the formative power is diminished, the process of reconstruction is slowly and imperfectly performed, and the demand for fibrin is less. *c.* When their vitality is still further depressed, they will have an unusual tendency to disintegration, and but little disposition to re-formation; and when it is altogether lost, the process of renewal is completely checked, and destructive decomposition takes its place. *d.* Where *new* parts are being formed—as in the reparation of injuries—a portion of the fresh tissue is the product of the effused fibrin, which becomes fibrous in coagulating; but another portion is the result of cell-growth; and the germs of the cells, as well as the pabulum by which they are developed, are probably effused from the blood.

III. *a.* The MOTION OF BLOOD in the capillary vessels is dependent, not merely upon the propulsive power of the heart and arteries, but also upon forces generated in the capillaries themselves, by the actions to which the blood is subservient whilst passing through them. These forces appear to be of such a character, that the several particles of blood are *attracted* towards the points where they are destined to undergo a change, and *repelled* from them after they have been subjected to it. *b.* If these actions, whether of nutrition or secretion, be unusually energetic, the motion of the blood will be accelerated; and much more than the ordinary amount of blood will pass through the part in a given time, constituting *determination* of blood, or *active congestion*. *c.* But if these actions be retarded, there will be a diminished rate of motion in the blood in the capillaries, constituting *passive congestion;* and if they be altogether checked, there will be an entire *stagnation* of its flow through the part, notwithstanding the impulsive action of the heart. *d.* These actions may be interrupted by a want of their proper conditions, on the part either of the blood or of the tissues, or of both.

IV. *a.* The PRODUCTS of the *normal* actions that take place between the blood and the tissues through which it moves, are either *new tissues*, of a character similar to that of the respective parts through which the fluid moves; or the *secretions* peculiar to those parts respectively, which are themselves elaborated by cell-growth, and therefore by an operation that may be considered as part of the general formative processes. *b.* When these processes are unusually active, in the state of *determination* of blood to a part, the products are the same in kind, but larger in amount. *c.* When these processes are slowly and feebly performed, as in *congestion*, the products are still the same in kind, but are less in amount. The obstruction to the circulation of the blood, however, causes a mechanical transudation of its more fluid portions, through the walls of the vessels, into the surrounding tissues, or upon the neighbouring surfaces; in this manner effusions of *serum* may take place to a considerable extent; and it has been lately shown that, when the obstruction is complete, or nearly so, even *fibrin* may thus find its way out of the blood-vessels. *d.* In the formation of new parts, as in the reparation of injuries, an increased production of fibrin in the blood may take place by local action; but if this fibrin be drawn off as fast as it is formed, there will be no accumulation of it in the blood; and, consequently, if no morbid products are formed, such processes *may* be performed without inflammation.

We shall now put into the same form the positions which we think may be assumed in regard to INFLAMMATION; with the intention, however, of presently dwelling upon many of them at greater length. We wish to be understood as here referring to the *sthenic* form of this process: the peculiar characters of *asthenic* inflammation will be hereafter noticed.

I. *a.* The quantity of *fibrin* in the BLOOD undergoes a decided increase; the *plasticity* of the whole mass, therefore,—but especially that of the liquor sanguinis,—is greatly augmented. *b.* There is a corresponding increase in the proportion of *white* corpuscles; which are present in large amount in the vessels of the inflamed tissues, and have a great disposition to adhere to their walls; but which are also present, to an unusual amount, in the entire mass of the circulating blood. *c.* The increase in the proportion of fibrin is chiefly a local action, exerted on the blood during its passage through an inflamed part; and probably effected by the instrumentality of the *white* corpuscles. *d.* There is usually an increase, not only in the *quantity* of fibrin, but in its *plasticity* or tendency to become organized; as shown by the greater perfection of the fibrous structure into which it passes in coagulating. This may consist in an increased attraction between its particles; which continues to operate for some time, causing contraction of the fibrous network, subsequently to its first production. *e.* There is also an increased attraction between the red particles of the blood; causing them to adhere together in rolls, more firmly and for a longer period than they do in healthy blood. *f.* To these two causes, usually aided in their operation by the slowness of the coagulation—all concurring to produce an increased tendency to separation between the red corpuscles and the liquor sanguinis,—we may ascribe the production of the buffy coat of inflammatory blood. *g.* The increased *plasticity* of the blood is so constant a phenomenon of inflammation, that it may be regarded as essential to the presence of that state.

II. *a.* On the other hand, the *formative power* of the inflamed TISSUES appears to be diminished; their usual functions, whether of nutrition or secretion, being completely checked, or insufficiently performed, or perverted in their character. *b.* Whilst, therefore, an over-production of fibrin is taking place in the blood, there is diminished consumption or appropriation of it in the tissues. *c.* If the inflammation be severe in its character, the vitality of the tissues is so diminished, as to cause not only a cessation of their formative actions, but also an increased tendency to disintegration, as shown in *suppuration* and *ulceration;* or positive death of a large part, as in *gangrene. d.* The depression of the vitality of the tissues sometimes appears to result from a previous over-excitement of it; as when inflammation follows excessive use of a part, or the application of stimulants to it: but it is sometimes the consequence of some directly sedative action, as that of cold. *e.* Hence both *determination* of blood, and *congestion,* have a tendency to produce inflammation: the one being a state of over-excitement, which is very prone to occasion subsequent depression, whilst there is at the same time a tendency to increased production of fibrin in the blood; the other being itself a state of depression of formative power in the solids, but not passing into inflammation, unless there be at the same time an increased plasticity of the blood.

III. *a.* The MOTION OF THE BLOOD in the capillaries of the inflamed part is greatly retarded; as we might have anticipated from the impairment of the functional operations of the solids. There may even be a total stagnation of the blood in the capillaries of a considerable portion of the tissue; which will be followed by its death and disintegration. The degree of stagnation will depend upon the amount of the depression of the vitality of the surrounding parts. *b.* The motion of blood through the vessels in the neighbourhood, however, is more rapid than usual, and these vessels are themselves enlarged; so that the total quantity which passes through an inflamed member in a given time is greater than usual. *c.* The vessels are enlarged both *in* and *around* the inflamed part, in consequence of a diminution of the tonic contractility of their walls; which causes them to admit of abnormal distension, by the impulse which the blood receives from the heart. This diminution is another evidence of the depression of the vital properties of the solid tissues in an inflamed part.

IV. *a.* The PRODUCTS of inflammation differ from those of the ordinary processes of nutrition and secretion, not so much in their materials, as in the nature of the change which these have undergone. *b.* When the intensity of the inflammatory process is moderate, the *liquor sanguinis,* containing an unusual proportion of fibrin, and possessing a high degree of plasticity, is effused into the neighbouring tissues, or upon the neighbouring surfaces; being generated, by the local actions of the part, faster than it can be withdrawn by its formative processes. By the organization of which it is susceptible, when in contact with the living solids, it spontaneously assumes the form of simple fibrous tissue, constituting false membranes on the surface, or consolidating the substance into which it is effused. *c.* If the inflammatory process goes no further, there is no disintegration of the original tissue; but if its vitality be too far depressed, it dies; and the changes which it consequently undergoes, impress themselves upon the fibrinous effusion. The fibrin loses its vital power of coagulation, and in this aplastic state becomes the chief ingredient in the *liquor puris;* whilst the cells (*pus-corpuscles*), which are found floating in it, resemble the white corpuscles of the blood in a degenerated form. *d.* When the inflammation is very severe, and the stagnation of blood in the capillaries of the part is complete, an entire loss of vitality in the whole tissue at once, or *gangrene*, is the result. Gangrene does not originate, however, in inflammation alone; since any other cause, such as the long-continued action of cold, or pressure, interrupting the capillary circulation, or obstruction to the supply of blood through the arterial trunks, will equally produce it, by the suspension of the formative processes thus occasioned. But unless some degree of inflammatory action, that is, an increase in the plasticity of the blood, be set up at the same time, there is an indisposition to the formation of the line of demarcation between the sound and the dying parts, and the gangrene has a tendency to spread.

We shall now enlarge upon some of these doctrines; and advert to the evidence upon which they are founded.

The large increase of fibrin in the blood of persons labouring under acute inflammation, has been sufficiently proved by the experiments of Andral and Gavarret; a similar statement having been made, however, by previous observers; though upon insufficient grounds. The phenomenon is so constant, that it must be regarded as a pathognomonic sign of inflammation, distinguishing it from other conditions which simulate it; and enabling it in some instances to be discovered at an earlier stage, than that at which it could have been detected by either general or local signs. The degree of the increase bears a constant proportion to the extent of the inflamed part, and to the intensity of the morbid action. The proportion cf fibrin has been observed in some cases to amount to as much as 10 parts in 1000, the normal proportion being from 2½ to 3½; and even when it was previously lower than usual, as in continued fever, it began to rise as soon as ever a local inflammation developed itself. That the increase is not only in the quantity of fibrin, but in its plasticity (or tendency to become organized) also, appears from various phenomena of inflammation; such as the rapid production of false membranes from fibrinous effusions; as well as from the more complete fibrous arrangement seen in the buffy coat, than that which the ordinary coagulum of blood displays.

The increased proportion of the white or colourless corpuscles, in inflammatory blood, and their special accumulation in the vessels of the inflamed part, is the concurrent result of the independent observations of Gendrin, Gulliver, Addison, and Williams. The observations of the first two of these gentlemen are chiefly directed to the increase of these bodies in the *whole mass* of the blood; the two latter have also directed their attention to the part they play in the production of local changes; and although their observations were made in each case without the knowledge of the results obtained by the other, the harmony between them is most complete, and therefore gives us a strong impression of their accuracy, more especially as many of them have been confirmed by our own observations. As this is a point of great interest, we shall extract Dr. Williams's statement in full. We should premise that he accords with other observers, (Mr. Wharton Jones and Mr. Addison for example,) in the remark, that, in the ordinary state of the circulation of the frog, the white corpuscles have a tendency to adhere to the sides of the blood-vessels, remaining in the almost motionless layer of liquor sanguinis that surrounds the rapid current in which the red particles move.

> "I have never seen a solitary elliptical disc adhering to the sides of a vessel; and whenever one was arrested in its course, it was from its becoming hitched by one or more of the adherent round globules. But what appeared to me most remarkable in regard to these white globules, was the great difference in their number under different circumstances. In young frogs, and in those much subjected to experiment, they are always present: but in healthy adult frogs, placed under the microscope with as little handling of the web as possible, there were few or none to be seen. I have watched, for ten minutes at a time, without seeing one; the motionless layer was very thin, but clear, and all the blood particles in the larger vessels seemed to move at the same rate of speed. It is under these circumstances that the effect of irritation or mechanical injury was best seen. By pressure of the fingers on the web, partial stagnation was produced in many of the vessels; and when this yielded to the returning current, the walls of the vessels were seen studded with the white globules; whilst many others of the same kind rolled over them slowly in the direction of the current. A similar result ensued after the web had been stimulated by capsicum or an aromatic water. Even in the rapid flow of blood following these applications, minute globules could be seen creeping slowly along the transparent outline of the larger vessels; and as the arteries contracted, and the flow through the other vessels became less rapid, the number of these globules increased, their motion became slower, and many adhered to the sides of the vessels. If the stimulus used was rather strong or long applied, the number of sticking globules was so great as to prevent the red particles from passing; and these becoming impacted in increased numbers, gave to the obstructed vessels a uniform and deeper red colour. When the stimulation was moderate, and equally applied to the web, the stagnation usually took place first in some of those anastomosing veins, in which the current is naturally slow, and varying in direction; but when a stronger stimulus (as an essential oil) was used, the stagnation speedily ensued at the point of its application; in fact, unless very minute quantities were employed, the stagnation was almost immediate and extensive.
>
> "I have varied these observations in a great many ways, and I have always found considerable or continued irritation of the vessels (textures) of the frog's web to be attended with the appearance and adhesion of the colourless globules; and that, when the irritant used is at all strong, or frequently applied, many vessels become totally obstructed, appear larger and redder than before, by the accumulation of red particles in them, (the blood-liquors having passed on,) and exhibit to the naked eye all the appearance of inflammatory injection. The chief cause of obstruction seems to be comprised in the two circumstances, the increased production of the white globules, and their remarkable disposition to adhere to the walls of the vessels, and to one another." (pp. 209-11)

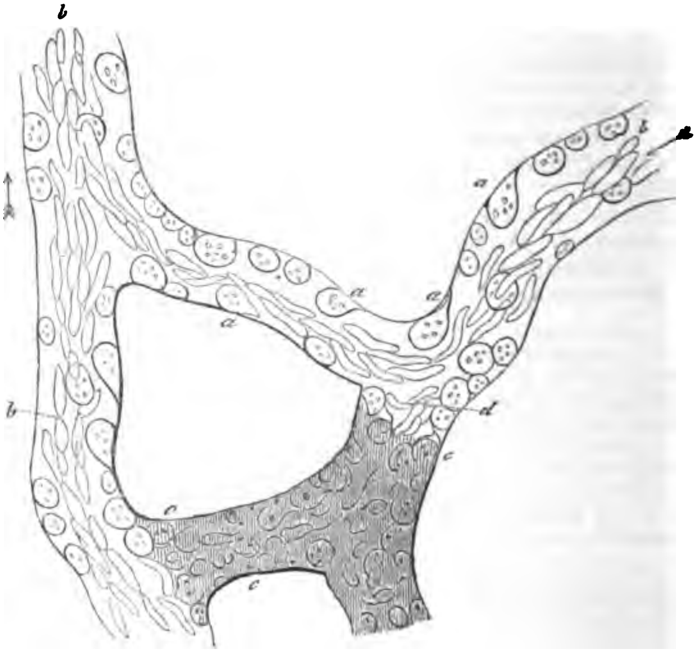
Dr. Williams is of opinion that the white corpuscles are *newly formed*, immediately upon the application of an irritant; and that, from the suddenness of their appearance, they cannot be regarded as *cells*. The researches of Mr. Addison, however, have clearly proved (to our minds at least) that they are true cells; and when it is considered how very small a proportion of the whole circulating system can be placed under observation at once, it does not appear to us difficult to account for their rapid accumulation in an irritated part, without imagining that they are suddenly generated. For there is no evidence that their quantity is increased in the whole mass of the blood, until the irritation has subsisted for some time, during which they are rapidly undergoing the process of cell-multiplication,—whilst at the same time, (according to the view we have adopted,) and by the very same act, increasing the quantity of fibrin in the blood.

Following out his opposition to the doctrine of "vital attraction" in all its forms, Dr. Williams objects to the idea that the adhesion of the white corpuscles to the walls of the vessels is anything else than a physical phenomenon.

> "The reason why the red particles are more readily carried in the stream, appears to be, that they expose a large surface to the current; and being covered by a perfectly smooth unadhesive membrane, they are not liable to stick to the walls. The white globules, on the other hand, are more compact; and although, when in the current, are readily carried by it, when more out of it, and in the motionless layer, are merely rolled by it, like pebbles in a rapid stream. Further, they manifest a distinctly adhesive property, which causes them to stick to the walls of the vessels. In this respect they contrast remarkably with the red blood-discs; and the newly-formed globules of irritated vessels seem to have this adhesive property in the highest degree; they are probably without a covering." (p. 212.)

Now we must take leave to remark that this idea of the possession, by the white corpuscles, of a "distinctly adhesive property," by which we presume that Dr. Williams means, in common parlance, that their surface is sticky, is a mere assumption, and is unsupported by any evidence whatever. For when inflamed blood is drawn from the vessels, the numerous white corpuscles which it contains do not show any tendency to adhere to each other, or to the red particles, which they would naturally do, if their surfaces were adhesive enough to cause them to stick to the walls of the vessels. On the contrary, they are distinguished by their isolation; whilst the *red* corpuscles *do* show a tendency to adhere to one another in rolls, especially in inflammatory blood; in spite of the smoothness of surface, which is dwelt on by Dr. Williams, as explaining their non-adhesion to the walls of the vessels. Now it is generally conceded that this attraction is a consequence of their peculiar vital endowments; and until the existence of some other cause shall be proved, we think that there is a strong argument from analogy in favour of the *vital* nature of the attraction between the white corpuscles and the walls of the blood-vessels. The last clause of the paragraph just quoted is not supported by any observations; and is opposed to all that we *know* of the nature of the white corpuscles.

As to the phenomenon itself, however, there is no difference of opinion between ourselves and Dr. Williams; and we avail ourselves of his figure and description to explain to our readers the condition of the blood and of the vessels in an inflamed part.



"The accompanying diagram exhibits the appearance of a small portion of the capillaries of a frog's web, after the application of a grain of capsicum. The elliptical blood-discs (*b*) are running in the axis of the vessel, which is much narrowed by white globules adhering to the walls, or only slowly rolling along them. These globules are speckled with nuclei or granules, refract the light strongly, and when rolled on by the current, some of them become pear-shaped from their sticking to the vessel, thus forming a kind of dragging tail, seen very well on those marked (*a*); on altering the focus, globules may be seen adhering to the other parts of the vessel. The shaded portion (*c*) is totally obstructed with lymph- and blood-particles, so impacted together, as to form a homogeneous red mass. In such a case I have often seen the particles at (*d*,) exhibit a pulsating or oscillatory motion, (corresponding with the action of the heart;) and this, after a time, succeeds in breaking down the obstructing mass, which passes away in clots, leaving the vessel (*c*) studded with lymph-globules like the other." (p. 243.) To the accuracy of this figure and description, we have the additional testimony of Mr. Addison. With the following induction, we fully accord:

> "It seems, then, to be well established, that an essential part of inflammation is the production of numerous white globules in the inflamed vessels; and that the obstruction of these vessels is mainly due to the adhesive quality of these globules. The production of these globules must probably be considered as an ultimate fact in the history of inflammation and nutrition; but it may be observed, that some-

times it seems to be the direct effect of an irritant acting on the blood-vessels and their contents; in other instances, it seems rather to result from determination of blood into previously congested capillaries. Any circumstance causing continued determination of blood where congestion is already present, will occasion the production of the white globules, and consequently inflammatory obstruction may ensue. The complete obstruction of some capillaries by coagulation takes place in all cases of severe inflammation of the frog's web; but there are slighter kinds of increased vascularity, in which there is no total obstruction, but a continued enlargement of the capillaries and veins, as well as of the arteries. This might be called simple determination of blood; but it differs from that of a transient character, in the motion of the capillaries and veins being slower, and in the vast number of white globules seen moving slowly in them. Very probably this kind of process takes place in the lowest forms of inflammation, and increased nutrition independent of inflammation. Something of this kind is generally seen in the capillary circulation of young frogs." (pp. 213-4.)

If Dr. Williams had connected with the development of a large amount of additional white corpuscles, the production of an increased quantity of fibrin, we believe that he would have come very near the truth, in regard to the participation of the blood in the phenomena of inflammation. We agree with him in thinking that both these phenomena may be present, to a moderate degree, without the existence of inflammation; since, whenever the formative processes are peculiarly active, as in young animals, and in the generation of new tissue for the reparation of injuries, they can only be supplied by such a modification in the character of the blood.

As to the *depression of the proper formative powers* of the solid tissues supplied by the blood, in the state of inflammation, we cannot conceive that much difference of opinion can exist, when the phenomena adverted to in our summary are duly considered; and we shall therefore proceed to the next head, the *altered motion* of the blood in the vessels of an inflamed part; in regard to which phenomenon we believe that there is little or no difference of opinion, although various causes have been assigned to it. Some, indeed, have regarded it as the essential character of inflammation; and even Dr. Williams falls into this error, for such we must term it. We cannot consider as the essential character of a morbid state that which is only *one of the results* of antecedent changes; on the united effects of which the various phenomena of that state depend, as we think may be shown in regard to the altered motion of the blood in inflammation. It is the tolerably uniform result of observation, that the application of a moderately stimulating substance to the web of the frog, *first* produces an accelerated motion of the blood in a part, with contraction of its vessels. Now we take these two results to be due to the temporary *exaltation* of its vitality; which will produce contraction of the vessels, by the excitation of their tonicity; and more rapid motion of the blood through them, by the more energetic demand for it. If, as Dr. Williams seems to suppose, diminution of the caliber of the arteries induces a more rapid rate of motion through them as a *physical* result, why does not this happen when cold is applied to a part? We know that it is quite otherwise, the arteries being contracted, in virtue of their excited tonicity, to which cold is a specific stimulus;* whilst the rate of motion through them is slow, because the general vitality of the surrounding tissues is lowered. On the other hand, moderate warmth induces enlargement of the arteries with accelerated motion; because, whilst it relaxes the tonic contraction, it renders the formative processes more energetic, and thus increases the demand for blood.

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* It may be objected to this view, that if the vitality of the tissues in general be lowered by cold, and increased by heat, that of the contractile coat of the arteries ought to fol-

But when the stimulating application has been applied for some time, or is more powerful in the first instance, an opposite result succeeds. The arteries dilate, and the blood moves more slowly; and this retardation may reach the point of complete stagnation. At the same time, there is increased motion of the blood in the vessels of the *surrounding* tissue, which, as well as the trunks leading to the inflamed part, are dilated; so that the whole quantity proceeding through the organ is increased. Now this dilatation is probably due in great part to the depressed vitality, or defective tonicity of the arteries, as Dr. Williams suggests; but we cannot (with Dr. Macartney and others) suppose this to be an essential character of inflammation; since it appears to us merely a consequence of the diminished vital power of the whole texture, which we believe to be one of its chief elements. The retardation of the blood in the central portion seems to us to manifest itself at an earlier period than could be accounted for on the principle of mechanical obstruction, caused by adhesion of the white corpuscles to the walls of the vessels, in the manner described by Dr. Williams; and we should explain it on the principle we have so frequently alluded to, that of diminished capillary power, (by which we mean, not mechanical force, but the force generated by the reactions taking place between the blood and the tissues,) resulting from the depressed state of the formative processes.* When the adhesion of the white corpuscles takes place to any extent, however, it undoubtedly becomes an additional cause of obstruction; and has probably a large share in producing the complete stagnation which is frequently to be observed. And this stagnation, when once produced, will of course act unfavorably upon the surrounding solids; occasioning a still further depression of their vitality.

We have lastly to consider the *results* of the inflammatory process; and these may, we think, be extremely well accounted for, upon the views we have been upholding. The mere accumulation of blood, and its stagnation in the vessels of the inflamed part, will give rise, as in the case of congestion, to the transudation of the *serous* portion of the circulating fluid; a physical action, to which an increased disposition is probably

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low the same rule; and that cold ought to produce dilatation, and heat to occasion contraction of their caliber. But it is to be recollected, that *cold* acts as a *specific stimulus* to the *tonicity* of the fibrous tissue, in the same manner as nervous influence or galvanism calls into action their contractility.

* By Dr. Alison it is believed that the retardation in the flow of blood is due to an *increased attraction* between its elements and the tissues. Such an increase would certainly appear to exist, especially in regard to the white corpuscles; but we doubt whether the supposition is necessary. In the normal process of nutrition, each particle seems to be attracted towards a certain point, where it is to undergo a change, and to be repelled from it after having undergone that change;—just as bodies are alternately attracted and repelled by another which is charged with electricity. Now on our view of the nature of Inflammation, the *attraction* of the particles of blood to the tissues still exists; but it does *not* give place to *repulsion*, in consequence of the absence of those changes to which it ought to be subservient; and therefore adhesion of its particles to the walls of the vessels manifests itself.

given, by the weakened state of the walls of the vessels. When the proportion of fibrin begins to increase, and the formative power of the part, together with the firmness of the walls of the vessels, diminish still further, there is a tendency to the effusion of the *fibrinous* portion of the blood also. As the experiments of Mr. Robinson* have proved that this may be poured forth, without the effusion of the red particles, under the influence of physical causes only, we do not think it necessary to invoke the aid of any other, under the various favouring conditions which we have specified. When the fibrin is thus effused, it will occasion a consolidation of the tissue, into the interstices of which it is poured; if it be sufficiently plastic to undergo spontaneous conversion into an organized structure. If poured out upon a membranous surface, it will form a false membrane; the structure of which is exactly the same as that of the buffy coat of the blood, only more perfect. The "exudation-cells" found in such tissues may be supposed to originate in the minute granules or cell-germs, which are seen floating in the liquor sanguinis of the circulating blood, and which may be considered as the offspring of the white corpuscles; but this is merely a probable hypothesis, chiefly supported by the fact that, as far as we *know*, all cells are developed from germs supplied by preexisting cells. If largely diluted with serum, the fibrin may be effused in a liquid form into a cavity, (as that of a serous sac,) and occasion the spontaneous separation of its contents into clot and serum. But if the fibrin be not duly elaborated, which may happen in consequence of a peculiar state of the system induced by various causes, it will be effused in various *cacoplastic* or *aplastic* forms, which we need not here stop to describe, particularly as they are well detailed in Dr. Williams's treatise.

The effusion of fibrinous matter, however, in the aplastic form of *pus*, must be ranged under a different category; being due to a *local* action more frequently than to a constitutional disorder; and into the causes of this we must next inquire. We cannot ascribe the suppuration of an ordinary *phlegmon*, to anything peculiar in the state of the blood; because we find no change in its condition, even when this process is actively taking place, the fibrin being still present in large amount, and of a high degree of plasticity. Moreover, at the very time at which the effusion of pus is taking place in one spot, we have (in subjects of a healthy character, at least,) an effusion of organizable fibrin into the surrounding tissue, producing its consolidation, and limiting the purulent effusion. Hence it appears to us next to certain, that the pus must be formed in the spot where it is effused; excepting in cases where there are diffused deposits of it, not preceded by inflammation in the parts so affected, which class of cases we shall presently consider. If our doctrines be correct, the formation of pus is due to an extremely depressed state of the vitality of the part most inflamed; which will exert an unfavorable influence upon the vital properties of the effused liquor sanguinis. Of such an operation there is no want of examples; the changes produced in the circulating blood by miasmata or infectious matters, in very small quantity, are "pregnant instances." The researches of Mr. Addison have rendered it probable, that the *liquor puris* is a degraded or degenerated form of the liquor sanguinis; and though we cannot agree with him in considering the pus-corpuscles as metamorphosed white corpuscles, (because we cannot think it probable that bodies so large as these can pass out of the capillaries, without an absolute rupture of their walls,) we are inclined to think that the two are developed from the same germs, the minute molecules circulating in the blood, and continually regenerated, (like the germs of the simple cellular plants,) by the bursting of the parent corpuscles. These germs, developed in the midst of a degenerated fluid, will have a degraded form; and thus from healthy liquor sanguinis, and from cell-germs, which would have otherwise produced exudation-corpuscles, will be produced, under the deteriorating influence of the contact of a dying and disintegrating tissue, liquor puris and pus-corpuscles. This view is borne out by the well-known fact that the admission of air to a surface, on which the formation of new tissue is taking place, will cause suppuration to occur, where no pus was previously effused; for the contact of cold air, depressing the vitality of the superficial tissue, causes a corresponding deterioration of the effused product; which thus, instead of becoming organized into new tissue, is converted into aplastic pus.

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* Medico-Chirurgical Transactions.

The circumscription of the deposit of pus in a solid tissue, by a fibrinous effusion, is well explained on the foregoing theory. For, as the inflammatory process is most intense in the centre, and gradually diminishes towards the periphery of the affected part, it will be in the *centre*, where the stagnation is most complete, and the vitality of the tissues the most depressed, that the production of pus will take place; whilst in the surrounding parts, the fibrinous effusion, not being subjected to this unfavorable influence in the same degree, will become more or less organized, and will prevent the extension of the purulent deposit. But when unfavorable states of the constitution, produced by general depressing causes of various characters, or by poisons directly introduced into the blood, prevent the formation of *perfect* fibrin, we may then have effusion of pus in an inflamed part without any such restriction; and when once generated, it will act unfavorably upon the surrounding parts, whose vitality is already depressed, causing their disintegration, and a consequent spread of the purulent effusion. This seems to us to be the true theory of *asthenic* inflammation; and the rationale of the success of a *general* tonic, or even stimulating plan of treatment, even whilst *local* depletion or sedatives are employed. Moreover, where pus is absorbed into the circulation,[*] there is good reason to believe, even from observation of its effects upon recently drawn blood, that it will occasion a most deleterious change in the character of the fluid; at once impairing its coagulability, altering the condition of its corpuscles, and giving rise (like a ferment) to an increased tendency to the production of purulent matters. When this is circulating with the blood, it may be separated by the two great depurating organs, the lungs and liver; or may be deposited in other parts, without the occurrence of inflammation in them; an increased tendency to such deposits in a part being perhaps induced by local congestion, which gives a predisposition to an effusion of some kind.

We quite agree with Dr. Williams in thinking that *ulceration* is only a modification of the ordinary suppurative process. It is generally accompanied with some degree of purulent effusion; but this commences rather on the surface, than in the substance, of an organ. The great feature in both is the depression of vitality, and gradual disintegration of tissue, depending upon the retardation or stagnation of the current of blood through it; and the chief difference seems to be, that there is not the same increase of plasticity in the blood, and tendency to effusion from it, in the early stage of the ulcerative process, as in the suppurative. It is not until a thick creamy pus is exuded from an ulcer, manifesting, not only an improved condition of the part itself, but an increase in the plasticity of the liquor sanguinis, of which it is a modified form,—that the healing process is fairly established; and, in order to accomplish this change, such constitutional means are required as shall favour the increase in quantity, or render the elaboration more perfect, of the fibrin of the blood; whilst local stimulating applications may have the same effect, by exciting the true inflammatory process, which was previously deficient.

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[*] There are cases, it is true, of the apparent absorption of pus, in which these results do not manifest themselves; but it is probable that in these it undergoes a change, before entering the circulating current.

When *gangrene* results from acute inflammation, it may be easily traced to the complete stagnation of blood in the vessels of a part; and the consequent loss of that vitality which was previously depressed. And the same process, taking place to a more moderate degree in the surrounding parts, will cause that deposition of organizable lymph, which limits the extension of the destructive process, and covers the surface from which the slough separates; and when, as in the case of diffused suppurative inflammation, there is not plasticity enough in the blood for this circumscribing effusion to be organized, the gangrene spreads, by the ordinary cause, i. e., the decomposing influence, upon a living part whose vitality is already depressed, of destructive changes going on in its immediate neighbourhood. And this want of limitation we see especially in cases where the gangrene does not result from local inflammation, but from some general depressing cause, which lowers the power of the whole system, whilst acting upon some one part especially. It is here necessary, as in diffusive suppuration, to take measures to *increase* the plasticity of the blood, instead of diminishing it; in fact, to excite a *sthenic* inflammatory condition, which did not exist previously. For if the views we are propounding be correct, the difference between the *sthenic* and *asthenic* forms of inflammation consists essentially in this, that, whilst in both there is a depressed vitality of the solid tissues affected, there is in the former a great increase in the plasticity of the blood, causing a tendency to effusion of organizable lymph, or of its modifications; which tendency is deficient or imperfect in the latter, in consequence of a want of the due elaboration of the fibrinous element of the blood. The distinction is most important in practice; because it leads us to see that, as the production of fibrin is necessary for reparation as well as for the original formation of tissue, we must carefully watch for the indications of its presence in sufficient or insufficient amount; and regulate our general treatment accordingly.

Our object in the preceding pages has been to show, that, starting from the two fundamental positions which we have assigned as the characteristic or essential conditions of inflammation, (at least in its sthenic form,) namely, a diminished vitality or formative power in the solid tissues on the one hand, and an increased plasticity of the blood on the other; we may account for all the leading phenomena of this process, upon simple physiological principles. The impaired tonicity of the *vessels* permits their enlargement. The depression of the ordinary vital actions of the part occasions *the diminution in the rate of the blood's movement;* which effect is aided by the adhesion of the white corpuscles to the sides of the vessels, producing mechanical obstruction. The stagnation, partial or complete, of the blood, produces in its turn *an increased depression* of the vitality of the tissues, and a tendency to the *effusion* of its fluid parts. The increase in the proportion of fibrin in the blood, together with the weakened state of the walls of the vessels, give to the *inflammatory effusion* the characters of the *liquor sanguinis*, rather than those of the *serum.* If the tissues into which this is poured sufficiently retain their vitality, the effusion becomes partially organized, filling up their interstices, and producing *consolidation.* Or if it be effused upon a surface, whose vital properties have not been too much lowered, it will form a *new membrane* of considerable tenacity. But if poured out into a tissue, or upon a surface, whose vitality is very much depressed, it is so far changed as to present the unorganizable form of *pus.* At the same time the tissue itself undergoes disintegration, in consequence of its impaired vitality; and its parts are removed by interstitial absorption (if this process still continues); or they are dissolved in the purulent effusion; or the whole mass is thrown off together in a slough.

In the *asthenic* form of inflammation, on the other hand, the same loss of vitality, suppuration, ulceration, or gangrene may occur; but there is little or no tendency to the restriction of these processes to one spot, or to the reparation of the injuries which they effect; the fibrinous material being either deficient in quantity or in plasticity. Hence we see that, in sthenic inflammation, as in the reparative processes, the increased production of fibrin in the blood is to be regarded as really a conservative operation. If it may be attributed (as we have endeavoured to prove) to the increased generation of white corpuscles, it seems to us that we make an important step in the analysis of this complex assemblage of phenomena. But the question still remains, What occasions this increased generation? To this we shall not at present attempt to make any reply; since we are in want of data, on which to found even a probable speculation. But we think it is something to be able to point to a fact so definite as this, as one of the essential components—if not *the* essential one—of the process under discussion.

We have not touched upon certain chemical views which have lately been propounded, in regard to the changes which the blood undergoes in Inflammation, by the increased *oxidation* of the protein compounds; —not because we do not regard them as possessing a high degree of interest, but because we deem them as yet too crude and unsettled to be combined, in any view of the subject, with facts which have been more satisfactorily ascertained; and because they belong to an entirely distinct category, and are quite subordinate to those we have been discussing. Supposing it to be true that this increase takes place, it is only another point of difference between healthy and inflammatory blood—the *essential* difference of which, in reference to the vital processes of nutrition and inflammation, consists in the increased plasticity of the latter.

Art. VIII.

On Regimen and Longevity; comprising Materia Alimentaria, National Dietetic Usages, and the Influence of Civilization on Health and the Duration of Life. By John Bell, m.d.—*Philadelphia*, 1842. 12mo, pp. 420.

We should have included this work in our leading article of No. XXXIII, had it reached us in time. The author addresses himself chiefly to the general reader, calculating also upon the advantages, as we learn from the preface, that the physician may obtain from his treatise. There can be little doubt of the propriety of instructing the multitude on the subject of food and diet, as a branch of hygiene; and the literary productions intended to fulfil this purpose come properly under our consideration. For several very obvious reasons, they demand the most jealous scrutiny. To the practitioner of the present day, if one thing be of more real importance, than another it is that the prejudices of society on the general subject of regimen should be dispelled by education, and the judicious writings of physicians are best calculated to effect this object. But the author who thus writes for the public can rarely, at the same time, expect to enlighten his brethren. In the capacity of a popular author he may well express his hope, that the matter laid before the reader may meet with the approval of the profession, and he ought to be extremely careful to secure that object. If, on the other hand, a work be intended to advance medical knowledge, by propounding new principles, or by novel applications of those that are already established, or if it encroach upon the proper province of therapeutics, it should relinquish entirely its popular character. In this case it is liable to become not merely useless, but most mischievous in the hands of the non-medical reader.

Dr. Bell's is a strictly popular work. At the same time it comprises many most interesting facts derived from various works on the physical history of mankind, on political economy and statistics, and from voyages and travels, the knowledge of which is not sufficiently diffused among medical practitioners. The author extols the advantages of agriculture to a country, and the influence of abundance of food over its inhabitants:

> "While advocating simplicity, he also recommends variety in dietetic regimen: he thinks that meat should be sparingly used, but he displays the endless variety of vegetable food, and the prodigal supply of fruits, with a luxurious enjoyment in their free use,—in the state in which, by a favouring climate, and skilful industry, they are met with in nearly all parts of the world. ...... Horticulture, the attendant and embellisher of agriculture, which provides so many palatable and healthful additions to the substantial produce of the field, and correctors of the undue stimulus and acrimony of much animal food, merits all the fostering care which an uncorrupted and yet educated and refined taste has ever extended to it. A well-cultivated garden, in due alternations of vegetable, fruit, and flower, gives us poetry without its allusions, nature divested of her ruggedness, and art of its constraint." (p. vii.)

Dr. Bell is strongly in favour of a diet consisting mainly of vegetables. The chapters on National Dietetic Usages, although little more than a collection of facts from the sources above mentioned, with but little reasoning or deduction, and none of that which the readers of our review of Dr. Pereira's work will understand as the science of the subject, are still worthy of perusal. In favour of a vegetable diet the Athenians and the ancient Greeks generally, the ancient and modern Egyptians, the wandering Arabs, and the Persians, and the peasantry for the most part of modern Europe are quoted. The Hindoo is also contrasted with the Esquimaux, the Chinese with the Laplanders, and the agricultural inhabitants of Mexico and the South with the carnivorous North American Indians. The author enters rather fully into the subject of fruits. He illustrates and dwells much upon the fact of an abundance of vegetable food, more particularly in favouring an increase of population. He regards the employment of corn and potatoes in distilling, as the "destruction of human food." He laments the excesses of the Americans in eating, and condemns, in no measured terms, their habitual drunkenness. He inculcates temperance to the letter, and is an enemy to the use of alcoholic fluids of every description.

We hope, at no very distant period, to bring before our readers the whole question respecting the use of alcoholic fluids, and shall accordingly forbear alluding more particularly to the author's views on the present occason, than by giving the estimate he has made of "the real state of French temperance,"and we may add of American intemperance:

"The sum total of alcohol in the different drinks used by the French people is as follows:

Gallons.			*Gallons of Alcohol.*
In 611,466,000	of wine,	at 9½ per cent.	55,000,000
234,121,000	of cider,	at 7 „	16,388,470
124,000,000	of beer,	at 5 „	6,200,000
17,954,800	of brandy,	at 50 „	8,982,400
987,551,800,	averaging 8·7 per cent.		86,570,870

This gives more than two gallons and a half per annum of pure alcohol for every individual in France. After making certain deductions, 7·7 gallons annually per man. By a similar calculation, the consumption per individual for Great Britain and Ireland is 1·28 gallons annually; and for the United States 5¼ gallons, without taking into the account the wine, beer, and cider consumed in the latter country. These estimates are drawn from statistical documents of the year 1830. From similar documents of 1840 it would appear that the whole of the alcohol consumed in America amounted to 3½ gallons annually for every individual in the States; being a reduction in the period of ten years of about 57 per cent.

Two hundred and fifty-five pages of the volume are occupied in the descriptions of the various articles employed as food. The period of publication precluded the author's acquaintance with the principles of dietetics promulgated of late on the continent and in this country. Our readers, therefore, will not expect us to open anew a subject that has been recently so fully treated of in this Journal, and we may content ourselves by stating, that, having examined the facts contained in the work before us, we find them upon the whole illustrative and confirmatory of the principles set forth in our review of Dr. Pereira's treatise.

On the subject of Longevity, we find Quetelet and Lombard quoted, and the author is indebted for his materials generally to Villermé, Edwards, Southwood Smith, Humboldt, Bulwer, Farr, the Reports on the Poor Laws, and of the Committee of Inquiry into Drunkenness, &c. &c.,

which authors and works are severally quoted. Dr. M'Culloch's 'Commercial Dictionary' has also been laid under heavy contribution.

The specimen we have given of the author's style is a favorable one. Throughout the work his sentences are frequently obscure and faulty in their construction. We meet with new words of American coinage, as "outranks," "evincive," &c. Others are misapplied, as where it is stated that magnesia is more "salubrious" than potash.

On the whole, however, we are bound to state that, as a popular work, advocating moderation and temperance, and, so far as they go, principles of dietetics in the main correct, Dr. Bell's work is well calculated to diffuse information and to do good to his fellow-men both in America and Europe.

Art. IX.

1. *On Feigned and Factitious Diseases, chiefly of Soldiers and Seamen; on the means used to simulate or produce them; and on the best modes of discovering Impostors.* By Hector Gavin, m.d.—*London*, 1843. 8vo, pp. 436.
2. *On the Enlisting, Discharging, and Pensioning of Soldiers, with the official documents on these branches of military duty.* By Henry Marshall, f.r.s.e., Deputy-Inspector-General of Army Hospitals. Second Edition.—*Edinburgh*, 1839. 8vo, pp. 260.
3. *Memorial de l'Expert dans la Visite sanitaire des Hommes de Guerre, ou examen des principales questions relatives aux maladies et infirmités qui peuvent donner lieu à l'exemption et à la réforme du service de l'armée de terre, et à leur simulation, provocation, et dissimulation.* Par L. Fallot, d.m., Médecin principal de l'Armée, etc.—*Bruxelles*, 1837. 8vo, pp. 414.

Although feigned diseases exist, to a certain extent, in all classes of society,—largely in workhouses and prisons, and especially in the navy and army—we shall, on the present occasion, confine our observations to the soldiers of the British army. In most particulars our remarks will apply to other departments, or they can be easily made to apply to them by practical men.

The study of feigned diseases is of great importance to the army or navy medical officer, who, in the course of his duty, is called upon not merely to treat disease where it really exists, but frequently to decide whether or not it exists at all. This circumstance renders his position one of considerable difficulty, because he is deprived of a most important aid to diagnosis, being compelled at all times to receive the statement of his patients with caution, and to trust very much to the information afforded by physical signs. As there are few men who will feign disease for the mere pleasure of deceiving, it seems necessary to examine what are the objects to be gained by successful simulation, in order to appreciate the extent to which it is likely to be carried among soldiers and sailors, and the means best adapted for its detection and prevention.

In the British army a recruit enlists of his own free will, but frequently he has no sooner done so than he repents, and endeavours, by false representations, to induce the surgeon before whom he is brought for approval to reject him. Failing in this, or if repentance does not so immediately follow, he commences a system of *malingering* (as it is termed,) on joining at head-quarters, by professing inability to learn some part of his drill, owing to the effects of an injury stated to have been received prior to enlistment, and the traces of which are either very slight or cannot be seen at all; or he throws himself upon the hospital on some pretence or other, and remains there with a perseverance worthy of a better cause, until he either obtains his discharge or, losing all hope of success, returns to his drill, and endeavours to make the best of his bargain. Again, after a soldier has been some time in the service, he becomes wearied with the constant repetition of the same duties, drills, and punishments, and is anxious, at all hazards, to return to civil life. The very nature of military service engenders a love of change; for the soldier being frequently moved from one barrack to another, and seldom occupying any station sufficiently long to attach to it the idea of a home, becomes fond of the excitement of a migratory life. The same feeling by degrees extends to his duties and occupations, with the monotony of which he gets disgusted. The only means of procuring a change is by his escape from the army; but, as he has engaged to serve " until he shall be legally discharged,"—and there is no prospect of this till such time as, from age, infirmity, or disease, he is deemed incapable, by the military authorities, of rendering efficient services—he frequently endeavours, by simulating a disability, to procure a termination of that life-servitude which has now become irksome to him. The Duke of Wellington has stated that " the objection to entering into the army is the severity and regularity of the duty; the regularity of the discipline; and the life which the soldier is obliged to lead, and which you must oblige him to lead; the climates to which he is exposed, and the constancy of the service in those climates."* These objections to entering the service well explain the motives which induce many soldiers, already in it, to endeavour to work out their discharge, after having experienced its inconveniences, and had the golden dreams of the recruiting-serjeants dissipated by sad and disagreeable reality.

Another class of malingerers are those who feign disease to avoid some particular duty, or to evade punishment. Although their object is merely temporary, and does not involve their ultimate loss to the service, it is of considerable importance to detect and frustrate their schemes: in the one case to prevent an act of injustice to the other soldiers; in the other to maintain the discipline of the service.

No soldier can claim his discharge as *a matter of right*, however long he may have been in the army; but if he is discharged in consequence of being unfit for military duty, a pension is granted him as a reward for past services, or as a compensation for the disability under which he labours. Formerly pensions for life were awarded to all men thus discharged, however short a time they might have served; and as they thus secured an annuity at the same time that they obtained their release from the thraldom of military discipline, a powerful inducement existed to feign disease. By recent regulations, however, those only are entitled to permanent pensions who have completed twenty-one years' service;

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* Gurwood's Despatches.

while men discharged at an earlier period receive a temporary allowance, proportioned to the length of time they have served, or the nature of the disability for which they have been invalided. As the soldier, even after having completed twenty-one years, must be reported unfit for military service before he is discharged and pensioned, it may naturally be supposed that a considerable amount of feigned disease is to be met with among these men. They are perhaps the most difficult cases with which the army surgeon has to deal; because, in many instances, the disease is not altogether simulated, but a slight affection is exaggerated, and the medical officer is called upon to decide, not whether it really exists, but whether it does so to such an extent as to disqualify the individual for the duties of his profession.

The causes, then, which give rise to the feigning of disease in the army may be divided into four great classes: 1. A desire to avoid entering the army or to be freed from it before the person has become habituated to its customs.* 2. A wish to escape some particular duty or punishment, or to get removed from some station abroad. 3. To escape from the irksome duties of a military life, with which the soldier has become disgusted. 4. To procure his discharge from the service, after having acquired a claim to pension. It devolves upon regimental medical officers, with the commanding officer's concurrence, to recommend soldiers to be discharged on account of disabilities, and it is obviously their duty to be careful not to bring forward any but such as are really unfit; for otherwise, at the same time that a useful man is lost, by unduly increasing the numbers on the pension-list the means of rewarding the really deserving soldiers are diminished, and occasion given for an outcry against the "burden" thus imposed on the country. The soldiers thus recommended are examined by a staff-surgeon, and any whom he deems fit for further service are ordered to return to their duty: they seldom, however, afterwards prove efficient, as they can always, when found fault with, fall back upon "the doctor's" opinion of their unfitness. This, therefore, should prove an additional reason for great circumspection in the performance of this important duty.

In all cases of malingering there is need of great patience and much professional skill on the part of the surgeon to enable him satisfactorily to investigate and decide upon the nature of the alleged disability. "The wiles of soldiers in hospital," says Dr. Cheyne, "will be with more certainty discovered by those who have an accurate knowledge of disease obtained from clinical observation and pathological writings of authority, than by those possessing natural sagacity, in the highest degree, if unassisted by a habit of carefully contemplating and studying disease." But when the surgeon has satisfied himself that the disease is feigned, his work may be said to be only begun: the most difficult part remains, namely, to induce the man to return to his duty. Marshall says, on this subject, "It is only after some experience that a medical officer is aware of the difficulties he has to encounter in his endeavours to reform persevering malingerers. Let him be ever so assiduous, and adopt the most judicious measures for the recovery of simulators who suffer under some real, although only trifling, cause of inability, he will frequently find his measures rendered nugatory by their unwillingness to be restored to the ranks, and the pains they take to retard their convalescence."

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* This cause is in much more active operation in armies raised and maintained by conscription than in those recruited by voluntary enlistment.

Impressed with the importance of the study of this subject, Sir George Ballinghall, in 1835, offered a prize to the pupils attending the class of military surgery in the University of Edinburgh, for the best essay "on the best classification of the feigned and factitious diseases of soldiers and seamen."

> "The object which the Professor more immediately had in view, was the formation of such a classification as would enable the surgeon, by it alone, to form an idea, not only of the frequency and success of imposition in any particular disease, but also of adjudging to soldiers who were discharged with a pension, the rate of that pension according as the disease on account of which they were discharged was or was not capable of simulation; premising that, though such a rule might (and probably would) be attended with individual injustice, yet its practical advantages would counterbalance such a minor grievance; because there would be a fixed scale for the amount of pension to be awarded to the soldiers discharged on account of a particular disability, and a great consequent reduction of the pension-list." (Gavin, Preface.)

We have given this quotation in our author's words, because we are by no means sure that we completely understand what the learned Professor wished; but we feel certain that if we have rightly conceived the object he had in view, it has not been attained by Dr. Gavin's work, which gained the prize. Before proceeding to examine the classification adopted in it, however, we must protest against the doctrine implied in the preceding paragraph,—that, for the sake of obtaining a uniformity in the rates of pension awarded for particular disabilities, and a great consequent reduction of the pension-list, we are entitled to disregard the *just* claims of the soldier. We regret deeply that an officer of Sir George Ballinghall's standing, and of such deservedly high reputation, should for a moment have advocated the doctrine of awarding pensions according to an artificial and arbitrary arrangement, without any reference to the merits of the case or the character of the individual, and even in total disregard of the *rights* of those who, in the service of their country, have perilled their lives, and returned to their native lands shattered in constitution, and often unable to earn a livelihood by their own labour.

In the work before us Dr. Gavin states:

> "Such a classification as that desired by the Regius Professor—viz. that by it we should be enabled to proportion the rate of pension to be given to soldiers who are to be discharged on account of disease—cannot be obtained by following the arrangement of any nosologist, or the divisions of any of the authors who have treated more particularly of feigned diseases..........An artificial arrangement must therefore be adopted for the occasion; and it appears that that which will conduce most to the stability of such a classification will be an accurate analysis of the symptoms of each disease, as it generally occurs, and a division of those symptoms into *such as are referrible to the feelings of the patient*, and for which the physician must trust nearly *in toto* to his patient, and into such as are *cognizable by the senses or acquired knowledge* of the physician." (p. 47.)

Now, if the existence of disease or disability were evidenced by one symptom alone, this principle of classification might be adopted; but how very seldom is this the case? In almost every instance—external injuries excepted—we must depend partly on the individual's statement, and partly on our own observation; and it is by the harmony or discrepancy existing between these we are led to form conclusions as to the reality and extent of the disease. But suppose it possible to refer every case to one or other of these classes, would this form a just basis for settling the amount of pension; or, because a disease is easily simulated, would it be equitable to assign a low rate in every instance? Most assuredly not: every case ought to be decided on its own merits, without reference to any arbitrary classification of diseases.

Let us take an example of the mode in which this proposed system would work. "Palsy," says Marshall, "is easily and frequently simulated; and, in many instances, considerable difficulty exists in determining whether an alleged disability of this kind is real or feigned." This, therefore, is a disease for which a small pension only should be granted; but we feel sure no one will support this opinion who has witnessed the deplorable cases of paralysis that occasionally occur, in which the individual is reduced to a state of mere vegetative existence. We have, it is true, selected an extreme instance; but the same principle holds good as regards other diseases—that it would be most unjust to award a trifling amount to a man labouring under a *real* disability, because it happened, unfortunately for him, to be one that was easily or frequently simulated with success. It may also be observed, that *time* is an element of considerable importance in deciding upon the claims of a soldier to pension. A disability which, in the early part of his career, would not justify his discharge, might, at a later period, disqualify him for military service, and justly entitle him to a pension; but, under the proposed system, no cognizance could be taken of this circumstance. The object sought for in this classification, then, appears to be either unattainable, or, if attainable, to be a measure fraught with great injustice.

Let us now briefly examine the arrangement adopted in the other two works whose titles are prefixed, and which are the productions of officers who have spent the greater part of their lives among soldiers, and whose attention has been long and successfully directed to this branch of their duty. Their object has been to group the diseases in such a manner as to facilitate the study, by rendering them more easily understood and retained in the memory; and, with this view, they have classed them physiologically.

Dr. Fallot has divided them into three great classes, as they affect the functions of Relation, of Nutrition, and of Reproduction. The first of these is subdivided into, 1st, the internal sensitive apparatus, comprehending the affections which Mr. Marshall has arranged under the head of diseases and lesions of the nervous system; 2d, the external sensitive apparatus which includes the organs of vision, of hearing, of smell, and of touch; 3d, the organs of sound; 4th, the organs of locomotion. In the second class, namely, the functions of nutrition, are comprised affections of, 1st, the organs of respiration; 2d, those of circulation; 3d, those of digestion; 4th, the urinary organs; and, 5th, the organs of assimilation. The third class comprehends the diseases and lesions of the organs of generation.

The classification adopted by Mr. Marshall is similar in its principles, but the details are somewhat differently arranged, and he has added a most important class, under the title of moral disabilities. This similarity affords a striking example of two persons being led to adopt precisely similar views as the result of extended practical observation, because it appears that M. Fallot was not in possession of Marshall's work until he had made very considerable progress in his own. (Preface, p. 9.)

It is not our intention to enter upon a consideration of the numerous expedients adopted by malingerers to attain their object, nor of the ingenious devices occasionally resorted to for the purpose of detecting the imposition, and causing them to return to their duty. Even those who are not likely to be called upon to carry into practice the principles laid down in these works, will find much amusement, and can scarcely fail to derive benefit also, from their perusal. The following may be taken as a fair specimen of Marshall's mode of illustrating his subject. In treating of crooked back, he says:

> "Private J. W., — regiment, was sent home from India to be invalided; and on the 10th of June, 1828, was admitted into Fort Pitt General Hospital. He stooped so much that the upper half of his body formed nearly a right angle with his inferior extremities; and he usually moved from place to place with the help of a stick, about two feet long, which he grasped by the middle. This disability was eventually presumed to be feigned, and he was consequently placed on his back on the floor, under the superintendence of a medical officer, when he held his legs in nearly a vertical direction, and complained most piteously upon any attempt being made to put his inferior extremities in the same line with his body. This position was reversed, and for some time he supported himself upon his head, hands, and feet. He soon became tired of this state of prostration: and at last, when he could endure it no longer, he stretched his legs fully out, and lay flat upon his chest and stomach. He then warmly expressed his gratitude to the medical officer for having so effectually cured him of a disability under which he had long suffered; and observed, that if the surgeon of the corps to which he belonged had done as much for him in India, what a happy man he should have been! He was discharged to duty on the 7th August, 1828."

We have already observed how extremely difficult and important it is to induce the simulator to return to his duty. When, after careful observation, the surgeon has satisfied himself that the disability does not exist, or is not of such a nature as to disqualify the man for military service, he should take an opportunity of quietly pointing out to the individual the folly of his conduct and of stating his firm determination to prevent him obtaining a discharge by such means. It does not follow that in these cases the man must be immediately sent out of hospital, for it is often advisable, particularly where the disease is not altogether feigned, but is one of exaggeration of an unimportant lesion, to retain him for a short time under treatment, and thus permit him to return to his duty without the stigma of being a suspected malingerer. "A man who has been in hospital for an alleged disability of a doubtful character should invariably be afforded an opportunity of *giving in*, (in other words of apparently recovering,) without taking him much to task for his conduct; or, in the language of the hospital, he should be *let down softly*." (Marshall, p. 92.) There are two rules connected with this portion of the subject which ought invariably to be remembered by the medical officer. The first is never to allow even the most flagrant imposition to cause him to lose command of his temper, or lead him to use harsh or unjustifiable language to his patient. Such a method, says Dr. Cheyne, "may sometimes intimidate a raw soldier, but will only afford a stronger motive to the hardened knave for perseverance; and it may likewise subject the officer to the just displeasure of his military superior." The other rule is never to adopt any treatment but what would be appropriate if the alleged disease really existed. Occasionally violent remedies, as the actual cautery, blisters, &c., have been employed with the view of forcing the malingerer out of the hospital, but such means are rarely successful. Moreover, if the disease should eventually prove to be real, what must be the feelings of the surgeon who has been pursuing such a line of treatment towards his unfortunate patient? On the other hand, if it is feigned, what confidence can the soldier place in the medical officer who could adopt such measures avowedly for the removal of an affection which never existed?

When the medical officer has failed, after due perseverance in these means, to induce the simulator to return to his duty, it is incumbent on him to report the particulars of the case to his commanding officer, detailing the circumstances which lead him to believe the disease to be feigned. By the articles of war, "any soldier who shall be convicted of malingering, feigning, or producing disease or infirmity, absence from hospital, protracting cure, &c., shall be tried for disgraceful conduct, and be liable to the punishments attached to that offence." When a soldier therefore has been reported as a malingerer, the duty of adopting any further measures rests with the commanding officer, who has the power to assemble a court martial for his trial. As the case must depend mainly on the evidence of the medical officer, who is placed in the position both of accuser and chief witness, he ought, before thus reporting an individual, to be well satisfied in his own mind of the accuracy of his opinion, and be prepared to substantiate it by evidence likely to prove satisfactory to the court. To break down in his proof would be not only injurious to the discipline of the regiment, but most detrimental to his own reputation.

Having thus briefly noticed the means to be used in endeavouring to reform malingerers, we would earnestly impress upon young medical officers, and aspirants for military employment, the necessity of confining themselves strictly to such duties as are clearly within their province. It occasionally happens that an assistant-surgeon, on first joining the service, is affected with a species of military monomania, if we may so term it, and seems to think it incumbent on him to labour zealously to support discipline. Now, it is of great importance, medical officers should guard against this mistake. If they confine themselves to their own duties, treating the sick with skill, attention, and kindness, cooperating with the commanding officer, when requested, in any measures he may wish to be adopted with a view to preserve discipline amongst the men in hospital, and endeavouring, by firm, but mild and judicious measures, to prevent malingering, they will assuredly gain the esteem and respect of their brother officers, and may safely content themselves with leaving the discipline of the regiments to the adjutant; while a contrary line of conduct will give rise to much disagreeable feeling, disturb the harmony of the corps, and eventually bring them unpleasantly under the notice of their commanding officers. This interference in military matters by medical officers arises from a very mistaken notion of their powers,—a notion which we are the more anxious to correct, that it has (no doubt inadvertently,) on one occasion at least, been sanctioned by very high authority. Sir G. Ballinghall, in speaking of corporal punishments, (Outlines of Military Surgery,) cautions military surgeons "against any untimely or undue interference with the discipline of the service, or any vain parade of authority, in the *only case in which their authority can be considered as at all paramount to that of the commanding officer.*" Now, by the regulations of the army, although the medical officers are granted a relative military rank for certain purposes, it is expressly stated that such relative rank is not to extend "to the exercise of any military authority or command whatever." In the case in question therefore, so far from having an authority "paramount to that of the commanding officer," they have no more right to *order* the punishment to be stopped, than the smallest drum-boy in the regiment; their duty is simply to report to the commanding officer when they consider the culprit unable to bear any further punishment without risk of life, but beyond making this report, they have no power to interfere.

The following remark of the Duke of Wellington, though not specially addressed to medical officers, merits well their consideration: "Abstinence from interference in which those who interfere have no authority, and in which they are not called upon to interfere by duty, will save the officers in command or authority abroad, and those placed over them at home, a great deal of unnecessary trouble."

In the preceding pages we have spoken unfavorably of the classification of diseases proposed by Dr. Gavin. We entertain, however, a different opinion of the general merits of his book, which reflects much credit on his talents and industry. It contains an excellent compilation of all that has been written on the subject, and we can confidently recommend it as a work of reference. It is not, however, so well suited for the study of persons unacquainted with the subject, because, like many compilations, it states the opinions of all the various authors without drawing definite conclusions as to their respective value or deducing practical conclusions. The other two works, on the contrary, are eminently practical, and ought to form part of the library of every army surgeon, and be the frequent subject of study of every candidate for admission into the public service.

Art. X.

1. *Anatomico-Chirurgical Observations on Dislocations of the Astragalus.* By Thomas Turner, Esq., m.r.c.s.l.—*Worcester,* 1843. 8vo, pp. 138.

2. *Mémoire sur l'Extirpation de l'Astragale, lu à l'Académie des Sciences dans la Séance du* 13 *Février,* 1843. Par MM. Rognetta et Fournier-Deschamps, d.m.

A Memoir on Extirpation of the Astragalus; read before the Academy of Sciences, 13*th Feb.* 1843. By MM. Rognetta and Fournier-Deschamps.

When reviewing the last volume of the 'Transactions of the Provincial Medical and Surgical Association,' we were compelled, from want of space, to pass by Mr. Turner's paper on Dislocations of the Astragalus, which is now published in a separate form. We take this, the earliest opportunity of supplying that omission; and shall, at the same time, advert to the memoir by MM. Rognetta and Fournier-Deschamps, mentioned at the head of this article.

Mr. Turner's object in the present treatise is "to assist in rescuing the surgeon from the painful dilemma in which he may be placed, for want of established rules to guide him in the arrangement and treatment of accidents occurring to the astragalus, respecting some of which we are left, by our best surgical authorities, in no inconsiderable degree of doubt and difficulty," (p. 7;) and he certainly adopted the best plan of attempting to remove whatever doubts and difficulties exist on the subject, by collecting and analysing recorded and unpublished cases, so as to deduce general inferences from a large number of facts. Accordingly, we are told that, "with a view to correct statistics, the author has made diligent search for published as well as unpublished cases," (p. 79,) and has accumulated forty-five; being "all the cases of dislocation of the astragalus which the author has, after much labour, been able to collect." (p. 78.) The work is therefore based on statistical research; and as completeness, copiousness, and accuracy of facts are the three things above all others needful in statistics, we must first inquire how far the author has complied with these essential requisites. We feel constrained to say, that his facts are neither complete nor, so far as they go, always accurate; for though we have to thank him for adding no fewer than sixteen unedited cases to those already on record, we can hardly admit that he has shown sufficient diligence in collecting, or correctness in quoting, the cases already known. It is, of course, but right that we should show cause for expressing this opinion; and, in attempting to do so we shall only notice omitted cases which are contained in books referred to by Mr. Turner himself, together with a few very generally known, either because of their importance, or of the celebrity of the works in which they are to be found. Mr. Turner refers to but twenty-nine published cases: we cannot pretend to say how many may exist in the books, but we conjecture they must amount to at least three times that number; but as we are not about compiling a statistical monograph on the accident in question, we must confine ourselves within the very limited field of inquiry just indicated.

In the last edition of Sir A. Cooper's work on 'Dislocations and Fractures of the Joints,' fourteen cases of dislocation of the astragalus are mentioned; four copied from other works, and ten either original or communicated to the author. Of these fourteen cases Mr. Turner quotes but eight, and consequently omits six; all of which were compound dislocations: three were reduced and recovered, (pp. 328-9-36;) one was kept unreduced, the bone sloughed out, and the patient recovered, (p. 328;) and, in the remaining two, excision of the astragalus succeeded in one case (p. 328), and terminated fatally in the other. (p. 331.) Before going further, we may observe: 1st. That the omission of Sir A. Cooper's 200th case (pp. 336-7) is the more to be regretted, as it is an example of an exceedingly rare form of dislocation of the astragalus not noticed at all by Mr. Turner; that, namely, in which the bone is displaced from the os calcis and os naviculare, while its connexion with the tibia and fibula remained undisturbed. We believe there are very few cases of the kind on record; and we are ourselves only acquainted with another, which is recorded by Dr. Macdonald. (Dublin Journ. of Med. Sc., vol. xiv, p. 235.) 2d. Though Mr. Turner, at p. 79, questions the truth of Sir A. Cooper's dictum, that "a simple dislocation of the astragalus sometimes, though rarely, occurs, a compound dislocation is still more rare;" yet he fails to observe that Sir A. Cooper's own work contains evidence, as far as it goes, in favour of Mr. Turner's doubts; for, of the fourteen cases therein recorded, but four are simple, and ten are compound. 3. Sir A. Cooper's 195th case (p. 130) is inaccurately quoted: it is Mr. Turner's seventh case, and, it is said, presents "a perfect illustration of compound dislocation forwards." (p. 40.) Whereas, the astragalus was dislocated *inwards;* and, further, the important fact is not mentioned that the posterior tibial artery was ruptured, and the accompanying nerve partially torn, and yet excision of the astragalus terminated very favorably. Mr. Hey, in his celebrated 'Practical Observations on Surgery,' (p. 388 et seq.,) mentions four cases of compound dislocation of the astragalus: in the first and fourth the astragalus was excised successfully; in the second, excision of that bone terminated fatally; and in the third, the patient having refused to suffer the projecting portion of bone to be cut away, the astragalus sloughed out. Not one of these cases is referred to by Mr. Turner; but only three of them can be reckoned here, as Hey's first case, which occurred in the practice of Mr. Trye, of Gloucester, is quoted by Sir A. Cooper, and is included amongst those just adverted to. The omission of Mr. Hey's fourth case (p. 386) is the more unfortunate, as it is expressly stated, that some motion remained in the ankle-joint, though the head of the astragalus was not extracted; and a simple reference to this and several other similar cases would have saved Mr. Turner the trouble of devoting eleven pages (pp. 126-36) to prove that Boyer and several others are wrong in supposing that excision of the astragalus must always terminate in anchylosis. Boyer cites twelve cases of dislocation of the astragalus, of which Mr. Turner quotes but seven. Amongst those he does refer to, some are very inaccurately given: thus, Mr. Turner's thirteenth case (p. 42) is Boyer's last case, (Traité de Malad. Chirurg. t. iv, p. 405;) and is given by our author as an example of dislocation of the astragalus "*forwards and inwards;*" whereas, it is expressly described by Boyer as an "*incomplete* luxation of the *head* of the astragalus *upwards* (not forwards) and *inwards.*" We are the more surprised at this mistake, as this case of Boyer's is remarkable for being the *only known* instance, as we believe, of this peculiar displacement; and thus we have a second rare form of dislocation of the astragalus totally unnoticed by Mr. Turner. But this case involves a series of mistakes: its issue is stated in Table II (p. 85) to have been "deformity and permanent lameness;" but at p. 42 it is more correctly stated, that ultimately there was no lameness, though the deformity (it should be *slight* deformity) continued. Again, Mr. Turner, from confounding this *comparatively* unimportant accident with the much more serious one of dislocation of the astragalus *forwards*, is led (in the section on the consequences of leaving dislocations of the astragalus unreduced,) to say, that as the motions of the foot ultimately become free, this case confirms the belief that in simple and partial irreducible dislocations of the astragalus, either forwards, outwards, or inwards, we should not interfere, but leave the case to the resources of nature. We quite agree in the rule of practice, as a general rule; but if it had no better foundation than the case appealed to in its support, we fear it could hardly be maintained. As another, but, we admit, a trifling example of inaccuracy, we may observe that Daniel's case is given as if it occurred in Boyer's practice. Such trifles, however, give no small trouble to the scrupulous inquirer; especially as Boyer's work, from which the latter case seems quoted, is not referred to; and we may further remark, that Mr. Turner is so very loose in and sparing of his references, that it is often extremely difficult to identify the cases he quotes, and to discriminate those that he omits to quote. So much confusion, indeed, arises from the way in which the cases are given, that it was only while penning these lines we discovered another mistake with reference to Boyer. Mr. Turner's forty-second case is given as an example of dislocation of the astragalus backwards; and purports to be extracted from Boyer's works. We were positively certain that Boyer had recorded no such accident; but after some research we discovered the case alluded to by Mr. Turner, (Boyer, p. 403;) which is not an example of dislocation of the *astragalus* backwards, but of the *entire foot* backwards; the connexions and relations of the astragalus to the tarsus remaining unaltered—an accident which Sir A. Cooper names dislocation of the ankle-joint, or of the tibia, forward, but which most writers term dislocation of the foot, and which no author since the days of B. Bell considers as a dislocation of the astragalus, and which is utterly unlike the other cases adduced by Mr. Turner himself, which are really displacements of the astragalus from the os calcis and scaphoid bone. It is not a little curious to observe with what very different views Boyer and Mr. Turner adduce this case. Boyer says that the signs of dislocation of the foot backwards "are *so evident* that a mistake respecting the accident *would be very difficult;* nevertheless, there is an example of such a mistake having been committed," (p. 403;) and then he gives the case, "the history of which" —we now quote from Mr. Turner—"illustrates the fact, that dislocation backwards, owing to the space being so considerable between the posterior part of the tibia and tendo Achillis, *may be overlooked;* a mistake scarcely likely to happen in dislocation in any other direction." (pp. 74-5.) This observation, of course, arises from the error as to the real nature of the case commented on: and here we must observe, that, deducting this case, as not being on dislocation of the astragalus, Mr. Turner quotes six cases from Boyer, and not seven, as we stated above, and the sum total of his cases is reduced to forty-four. Among the cases quoted by Boyer, but overlooked by Mr. Turner in his statistics, though incidentally mentioned elsewhere, is one of successful extirpation of the astragalus by F. Hildanus, celebrated as being the first example of the operation on record. The remaining five belong to Dessault, who has further recorded two others, making seven in all, which may be found in his 'Œuvres Chirurgicales,' by Bichat, (t. i, p. 435, et seq.) and 'Journ. de Chirurg.,' (t. i;) but not one of those well-known cases is quoted by Mr. Turner. Of Dessault's cases, two were simple, and one was reduced with facility; but it is a remarkable fact in the history of this accident, that the second being irreducible, Dessault attributed the failure to the smallness of the opening made in the capsular ligament by the head of the astragalus, and freely divided the integuments and the ligaments; after which the dislocation was easily reduced, and the case did well. Nor is this an isolated case; for the same practice, we may here observe, was successfully adopted, under similar circumstances, by Navula. (Bull. de la Fac. de Méd., 1812.) The five other cases mentioned by Dessault were all compound, and in all the astragalus was excised: four of the patients recovered and but one died; not, indeed, it would appear, in consequence of the accident, but yet it is safer to reckon this among the fatal cases. We can the less account for Mr. Turner's neglect of Dessault's cases, as he actually incidentally alludes to two of them, (pp. 93, 131,) which, we suppose, caught his eye in turning over Boyer, for they are both mentioned by that author: but, we repeat, they are only incidentally alluded to, and are not included in the cases for which our author made "diligent search" "with a view to correct statistics." Dupuytren is said to have met with ten or twelve cases of dislocation of the astragalus, but only five of them are distinctly recorded in his works; one in his 'Mémoire sur les Fractures du Peroné;' and four in the 'Leçons Orales.' Of these five cases, Mr. Turner quotes but three, and contrives to involve them in almost inextricable obscurity. The first case cited from Dupuytren is Mr. Turner's fifth, (p. 38,) and is most remarkable as one of the very few instances on record in which a *simply* luxated astragalus was cut down on *immediately* and extirpated. The reference given for this case at p. 38 is the first volume of the 'Medico-Chirurgical Journal;' but it *must* be the case recorded in the above-mentioned memoir by Dupuytren, published in the 'Ann. des Hôp. de Paris,' as no other similar case has been published as occurring in Dupuytren's practice; yet, strange to say, we find this very case again incidentally mentioned by Mr. Turner (p. 53), where the correct reference to the original memoir is given. But this is not all. At p. 39 we are rightly told that "the bone was cut away," but it is added that the result was recovery, "with anchylosis of the joint;" and yet, in Table II, facing p. 85, which purports to give a "*classification of treatment, and results of dislocations of the astragalus,*" this very case (No. 5), in which the bone was cut away, is enumerated amongst those headed, "*Bone allowed to remain in its new situation;*" and the result is again stated to be anchylosis of the joint. We need not stop to point out the contradictory statements respecting the treatment of the case contained in the text and in the table; but we must observe that there is not one word in the original memoir warranting the averment that anchylosis occurred: we are merely told that, three years after the accident, the patient walked "avec *un peu* de claudication, il est vrai, mais d'ailleurs aussi vite et aussi longtemps qu'avant son accident." The twenty-second case in the present work is also inaccurately given. "Reduction," it is said, "was attempted, but without success." (p. 49.) But the fact is, Dupuytren nearly, though not completely, replaced the bone. This twenty-second case is therefore wrongly classified in the table already referred to, as it should have been placed under the head, "partial reduction." Furthermore, as already stated, two of Dupuytren's cases are totally omitted; one simple reduced, the other compound and irreducible, the wound having healed, the patient applied to Dupuytren six months after the accident, because of the deformity and uselessness of the foot, the astragalus then was excised, and the patient obtained a very useful foot.

We have now referred to twenty-two cases, which are all, save two, contained in the works of Cooper, Hey, Dessault, Boyer, and Dupuytren, and have also, we think, abundantly shown how imperfectly the facts borrowed from some of those authors have been dealt with. Many more cases exist in the books: we do not pretend to be in a predicament to cite them all, or even a small portion of them; nor, if we were, would this be the place to do so; but we shall print a few that we do happen to know, all of which bear on more or less important points of the history of the accident. De Guignieres (Bull. de la Fac. Méd. de Paris, t. iii, p. 238,) records a case of compound dislocation of the astragalus inwards, with fracture of the fibula. The astragalus was excised, and the patient recovered *without anchylosis*, a *new joint* having formed between the tibia and the os calcis; a circumstance which we thus particularize for a reason already mentioned. To this case there is appended one by Chaussier, of irreducible compound dislocation of the astragalus forwards, in which, as the resistance of the tendons was the chief obstacle to reduction, the tendons of the tibialis anticus, the tibialis posticus, and the extensor proprius pollicis pedis were divided; the dislocation was then easily reduced; but the patient died. No doubt the practice adopted here is by no means analogous to the modern application of tenotomy; but still the case deserves to be mentioned in connexion with that subject, for it at least shows that division of the tendons facilitates reduction. Follot (Archiv. Gén. de Méd., t. xx, p. 292) relates a case—or rather, there is a report on the case—of complete compound luxation and comminuted fracture of the astragalus, with rupture of the tendon of the tibialis anticus muscle; but, notwithstanding the immense violence sustained, and the great injury to the soft parts, extirpation of the astragalus terminated in cure by anchylosis, rendering the case one of the most important on record in favour of the practice adopted, as compared with amputation. M. Barbieux also successfully removed the astragalus on the second day, in compound dislocation of that bone. (Journ. Complm. du Dict. des Scienc. Méd., t. ix, p. 285.) Again, M. Rey gives a case of complete compound dislocation inwards, and semi-rotation of the astragalus, and partial dislocation of the tibia on the astragalus forwards, *without fracture* of either bone: excision of the astragalus terminated in recovery. (Gaz. des Hôp., t. x, p. 470.) Mr. Norris of Pennsylvania, in a case of *simple* irreducible luxation of the astragalus forwards and outwards, as the integuments over the bone were very tense and discoloured, cut down on and removed it, excepting its head, which, being broken, was left adhering to the os naviculare. Gangrene supervened, and amputation terminated in death. This is the only case we know of in which the practice adopted by Dupuytren, in one instance, of *immediately* excising a *simple* luxated astragalus, has been imitated; and we think the condition of the soft parts in each fully justified the operation. (Gaz. Méd. de Paris, 1837, p. 796.) Finally, Dupuytren being unable to reduce a simple dislocation of the astragalus, left it in situ; the limb gangrened and was amputated, but the patient recovered. On dissection, the astragalus was found rotated on the os calcis. (Archiv. Gén. de Méd., 1833, p. 506.)

We had intended to examine how far the cases we had added to those given by Mr. Turner, alter the various numerical results he has arrived at, but to do so fully would carry us too far, and, moreover, could not lead to anything conclusive, inasmuch as a statistical investigation should be founded on all, or some reasonable approach to all, the facts connected with the subject under inquiry. We shall therefore allude to one point only: Mr. Turner gives two to cases of *simple*, and twenty-one of *compound* dislocations of the astragalus, and is thence led to doubt the accuracy of Sir A. Cooper's opinion, that *compound* dislocation occurs more rarely than *simple* dislocation of that bone: the addition of the *unselected* cases we have referred to would lead to a result directly contrary to Sir A. Cooper's opinion, for the numbers then would be thirty-one *simple*, and forty-two *compound*. It remains to be seen, however, whether Sir A. Cooper's opinion may not prove true, if ever a very extensive series of cases is collected.

The mechanism of dislocations of the astragalus is as yet but imperfectly understood, and as the subject is important and interesting, we shall here notice M. Rognetta's experiments on the subject which originally appeared in the Archiv. gén. de Méd.*(* 1833, t. iii, p. 485, and are in fact republished in the memoir mentioned at the commencement of this article.

When we inspect the astragalus in the skeleton, it appears most liable to be displaced inwards, because of the deep notch between the os calcis and the os naviculare, but in the living subject, primary displacement inwards must be difficult, or rather impossible: 1, Because the notch in question is filled by very powerful ligaments; 2, because the head of the astragalus can most readily escape directly upwards, as the upper part of the capsular ligament is loose and thin; and 3, because, according to M. Rognetta, the only power capable of dislodging the astragalus is the tibia acting as a lever of the first order, in the direction of the long axis of the foot. In walking, the astragalus may be considered a moveable pivot, on which a threefold motion takes place: 1, That of the bones of the leg on the pulley of the astragalus: 2, that of the astragalus itself on the bones of the tarsus; and 3, that of the astragalus conjointly with the bones of the leg on the bones of the tarsus. Now there is no lateral motion between the astragalus and the tibia, and fibula, and inversion and eversion of the foot are exclusively effected in the joints of the tarsus, but the 3d, or conjoint motion above referred to, is more complex.

When the leg is raised from the ground to take a step, it is first slightly flexed on the foot, and the foot is simultaneously flexed on the leg, so that there is a double inverse motion, that of the pulley of the astragalus backwards, and that of the articular surface of the tibia forwards. But when the whole foot is flexed, and the pulley of the astragalus rotated backwards, the head of that bone must be depressed in the scaphoid cavity, towards the sole of the foot. When the foot is now thrown forwards, it is extended on the leg, and the leg is extended both on the foot and on the thigh, and now it is that the conjoint motion of the bones of the leg with the astragalus comes into play. The tibia bearing on the astragalus posteriorly depresses it in that situation, and the head of the astragalus consequently becomes more or less prominent on the dorsum of the foot. That this is the case is easily ascertained by placing the fingers over the head of the astragalus while an individual is walking, when it will be felt elevated or depressed, according as the foot is extended or flexed on the leg. Hence it follows that when the foot and leg are both forcibly and simultaneously extended, the head of the astragalus projects towards the dorsum of the foot, has a tendency in fact to be dislocated from the os naviculare; and, generally speaking, the conditions necessary for the occurrence of dislocation of the astragalus, are that the foot shall be forcibly extended on the leg, and the leg powerfully extended on the foot by the weight of the body; the luxating force impinges on the astragalus entirely through the tibia, which acts as a lever of the first order, the power being applied at its upper extremity, the resistance being at the astragalus and the os calcis, constituting the fulcrum, but the astragalus can scarcely be dislocated unless the tibia acts in the direction of the antero-posterior axis of the foot, for if either malleolus acts singly or obliquely, and tends to displace the astragalus laterally, the malleolus itself will first give way.

M. Rognetta, with the object of illustrating those views, performed a series of experiments, which, though analogous to those instituted by Dupuytren, require to be noticed, both because they more especially relate to the tarsal articulation of the astragalus, and because his results are not quite in accordance with those obtained by Dupuytren.

When the foot of the dead subject is engaged between the bars of an iron grating, and the body is forcibly drawn back; M. Rognetta found that the tibia and fibula were easily fractured, and then the head of the astragalus ceased to project on the dorsum of the foot; in some young subjects, however, the tibia offered great resistance, but the fibula broke near the ankle, and then dislocation of the *pulley* of the astragalus and of the *foot* outwards could be effected, but dislocation of the *foot* inwards, or dislocation of the *astragalus* properly so called, could never be accomplished. M. Rognetta now repeated the experiments. Having denuded the bones of all the soft parts, except the ligaments, and, acting on the lever formed by the leg, gradually instead of forcibly and suddenly, observed that the anterior ligament of the ankle-joint first gave way; on continuing the extension, the external malleolus, which is larger than the internal, came in contact with the posterior part of the os calcis; further extension fractured the fibula, inferiorly and partially, or completely ruptured the ligaments that unite it to the tibia,—a circumstance which greatly facilitated the production of dislocation of the astragalus, because the tibia, when thus separated from the fibula, has only the resistance of the astragalus to overcome, and, except when the tibia gave way, the head of the astragalus, on continuing the extension, was always luxated forwards; once this effected, the astragalus could be easily thrown either outwards or inwards, but it was impossible to displace that bone laterally, before its head was thus thrown forwards, as the tibia or fibula respectively were always fractured in the attempt.

From these experiments M. Rognetta, among other conclusions, maintains that the astragalus must always be dislocated forwards before it can be displaced laterally. The extent of the luxation may of course vary infinitely from simple dislocation of the head of the astragalus upwards, (as occurred in Boyer's remarkable case already mentioned,) to complete dislocation forwards, or from the slightest to the most complete lateral displacement.

One of the most remarkable circumstances connected with dislocation of the astragalus is the occasional rotation of that bone on its axis, of which M. Rognetta offers the following ingenious explanation. If during a fall the foot is extended, the tibia pushes the astragalus forwards, and the head of the latter bone being thrown upwards from the scaphoid cavity, distends the integuments and tendons on the dorsum of the foot; the body of the astragalus being still impelled forwards, its head is now placed between the two opposite forces, that exerted by the tibia through the body of the bone, and that offered to it by the integuments, which, as they present more resistance towards the toes than on the dorsum of the foot, tend to throw the head of the astragalus backwards, and may thus cause complete rotation of the bone. We may thus understand how the astragalus has been rotated on its transverse axis, but M. Rognetta offers no explanation of the more difficult case, where the bone is rotated on its antero-posterior axis, and its head consequently remains in front.

This article has already exceeded the length we originally anticipated; but we cannot conclude without briefly considering the "principles of treatment in dislocations of the astragalus" laid down by Mr. Turner, (p. 88 et seq.) the more especially as we think they are, on the whole, very judicious, though in a few particulars we, perhaps erroneously, differ from the author.

Reduction should always be accomplished in partial and simple dislocation of the astragalus, but if it is impeded by the astragalus being twisted, &c.; operative interference should be abstained from, as the patient has a reasonable chance of obtaining one useful foot by trusting to the resources of nature. In *complete* and *simple* dislocation, the chief and too often insuperable obstacle to reduction is the approximation of the tibia to the os calcis by muscular action, which being effected by all the muscles passing from the leg to the foot, can be but partially overcome by flexing the leg on the thigh. Reduction, however, can sometimes be accomplished, especially when there is fracture of the leg, or of the os calcis, or dissevering of the bones of the tarsus, as the purchase of the muscles is then impaired, (p. 99;) but is it always safe to return the bone? This question Mr. Turner does not answer very explicitly, but seems to imply that when the bone is so loose as to be *easily* returned, it will be likely to die, and thus place the patient in a worse predicament than if it had been excised or left unreduced. (p. 92.) We doubt the wisdom of this precept, and at all events *if* we were so persuaded that the bone must die as to abstain from reducing it when practicable, we would greatly prefer excising it immediately. In complete compound dislocations of the astragalus alone, without fracture or solution of continuity and connexion in the bones and joints of the tarsus, all attempts at reduction are, in Mr. Turner's opinion, not only hopeless but prejudicial, (p. 96;) this rule may possibly be too absolute, but is, we believe, generally true. Mr. Turner admits tenotomy to be justifiable as a means of facilitating the reduction of partial dislocation, but speaks, we think, too doubtingly as to its applicability in complete luxation. (pp. 94-5.)

Leaving the astragalus in its new situation. In dislocation backwards, the bone scarcely seems to act as a foreign body, and the cases in which it was left unreduced did well. (pp. 92, 101.) In *partial* and *simple* dislocations, either forwards or laterally, we should not interfere either. If the bone is left unreduced in *complete but simple* dislocations, the bone is very apt to excite inflammation, &c., and to slough out, but still Mr. Turner would, if the bone was protruded *directly* in reference to it, leave the case to nature, and subsequently, if inflammation set in, act as specified in the section on excision.

Excision of the astragalus. In *partial* and compound dislocations, (reduction failing,) the *protruded* portion of the bone should, if practicable, be excised. In reference to complete excision of the astragalus, we shall borrow the author's words:

> "It may be summarily stated, that in simple, direct, and complete luxation, the author advocates the practice of allowing the bone to remain in its new situation, without any operation, until it manifests a tendency to ulcerate the skin, in which case he would make an excision over the bone, to relieve tension and pressure, and that when the bone is so far detached from the circumjacent textures by the natural process of separation, he would remove it. In simple, indirect, and complete luxation, he would anticipate, as a matter of certainty, that the bone would die and require dislodgment; to take off tension and pressure from the angles of the displaced bone, he would at once make an excision over it, but not remove the bone, wishing to benefit by the probability that the exposure of the cavity of the joint may have an injurious effect. [*Sic* in orig.] In complete compound luxation, whether direct or indirect, or complicated with fracture or with dislocation of the ankle-joint, he would immediately proceed to the removal of the astragalus, from believing that the limb will be put in a better condition for the reparative process of the joint, by the abstraction of the processes of inflammation, suppuration, ulceration, and sloughing, (processes necessary to the disengagement of the astragalus by natural efforts.") (pp. 110-11.)

On this passage we have to observe that we differ from Mr. Turner in this respect only, that whenever we saw reason to cut down on a simply luxated astragalus, we would certainly proceed to remove the bone at once, if the case were one of partial dislocation; circumstances might indeed render it difficult to do so, but at all events we think the accessible portion of the bone should be removed. In a word, if we converted a simple into a compound dislocation, we would, as a general rule, deal with it precisely as if it had been compound in the first instance.

In conclusion, though we cannot compliment Mr. Turner on his diligence or accuracy in research, we have to thank him for an useful essay, which he could, we think, convert into an extremely valuable monograph by the adoption of more extended and accurate statistics, and a somewhat more methodical arrangement of his practical commentary.

ART. XI.

1. *Untersuchungen über den Bau des Nerven-systems.* Von Dr. B. STILLING und Dr. J. WALLACH. *Zweites Heft, enthaltend Untersuchungen über die Textur und Function der Medulla Oblongata.* Von Dr. B. STILLING. Mit 7 Tafeln abbildungen.—*Erlangen*, 1843.
Researches into the Structure of the Nervous System. By Dr. B. STILLING and Dr. J. WALLACH. *The Second Part, containing Researches into the Structure and Function of the Medulla Oblongata.* Seven Plates.—*Erlangen*, 1843. 4to, pp. 72.

2. *Mikroskopiske Undersögelser af Nervensystemet.* Ved ADOLPH. HANNOVER, Lic. Med.—*Kjöbenhavn*, 1842.
Microscopical Investigations of the Nervous System. By Dr. ADOLPH. HANNOVER. With Plates.—*Copenhagen*, 1842. 4to, pp. 112.

3. *Untersuchungen über die Physiologie der Nervenfaser.* Von Dr. G. H. MEYER, Privat-docenten zu Tübingen, &c.—*Tübingen*, 1843.
Researches into the Physiology of Nervous Fibrils. By Dr. G. H. MEYER, &c. 8vo, pp. 316.

IN a former article (see our last Vol., p. 379) we reviewed some of the recent discoveries in the anatomy and physiology of the sympathetic system, and of the spinal cord. In the present, we propose to trace those facts respecting the structure and function of the medulla oblongata and encephalon, which the works before us may furnish.

I. DR. STILLING'S RESEARCHES. Dr. Stilling divides the medulla oblongata into four portions: the first extends from the origin of the second pair of cervical nerves to the roots of the hypoglossal nerve; the second is that portion circumscribed between the lowest twig of the hypoglossal nerve, and the lowest of the vagus; the third is that part between the last-mentioned point, and the origin of the glosso-pharyngeus; and the fourth from hence to the pons varolii. The general organization, and the special microscopic anatomy of each of these portions are detailed at length.

Dr. Stilling observes that the structure of the spinal cord, as described in our last Volume, is the same throughout upwards to the origin of the spinal accessory nerve; it is at this point that a change is first observable. The organization is also similar from the origin of the seventh pair of cervical nerves to that of the second; but from the insertion of the middle and inferior roots of this nerve, as far as the pons varolii, the organization of the cord gradually changes; so that these two points are the true limits of the medulla oblongata.

To state generally the results of Dr. Stilling's researches, we remark, that the longitudinal fibres of the white matter in the anterior, posterior, and lateral strands of the cords have an uninterrupted course from the cauda equina to the pons varolii, and beyond it; and so also the longitudinal fibres of the gray substance and of the gelatinous substance. How they terminate in the *cauda equina* is not known. The white fibres which form the lateral strands lie upon one another, and are distinct. The "spinal bodies" are situate among the fibrils of the anterior gray substance only along their whole course, and are collected in larger masses where the nerves of the extremities pass off. The longitudinal fibres of both kinds are also traversed by horizontal fibrils, which enter the posterior strands at the roots of the posterior nerves, cross the posterior longitudinal fibres of the white, gray, and gelatinous substance, and, after sending a part to the opposite lateral half behind and before the spinal canal, pass on in a direct horizontal direction through the spinal bodies, and crossing the white and gray longitudinal fibrils of the anterior columns emerge as the roots of the anterior nerves. The prolongation of the pia mater and the blood-vessels dip deep into the cord, and are accompanied by remarkably fine gray fibrils given off from the anterior gray matter, and are considered by Dr. Stilling to be nervi vasorum. In man, at several points where the gray and white matter of the lateral strands join, gray fibrils dip (but not deeply) into the white. These latter are principally seen at the enlargement of the cord, corresponding to the roots of the nerves of the arm. In the medulla oblongata there are additional elements:—1, new masses of gray substance, and new white longitudinal fibrils; 2, a new species of semicircular fibrils, running in a longitudinal direction, make their appearance; and 3, an important intermixture of white and gray fibrils unlike anything in the spinal cord. The gray masses are found on all the strands, and around the spinal canal; in the anterior columns they form the corpora pyramidalia; in the lateral, the corpora olivaria; in the posterior, the corpora restiformia, the funiculi graciles, and funiculi cuneati; and around the spinal canal, the nuclei (kerne) of the hypoglossus, vagus, accessorius, and glosso-pharyngeus. The new white fibrils are peculiar to the anterior strands. They commence near to the point where the medulla oblongata begins, namely, between the first cervical pair and the hypoglossus. These arising from the anterior gray commissure at the margin of the anterior sulcus, are applied to the white matter of the anterior column, and pass upwards to and through the pons varolii. The new semicircular and horizontal fibres spring from the nuclei below and behind the spinal canal, and traverse the posterior, lateral, and anterior columns. They form the fibræ transversæ and fibræ arciformes, described by German anatomists, as being in relation with the inferior surface of the corpora olivaria. Besides these changes in the medulla oblongata, its primary white and gray longitudinal fibres are more intermixed than the similar structures of the medulla spinalis, and this intermixture is so much increased by the introduction of the semicircular fibrils, that a most complete network is formed. The fibres of the hypoglossus, accessorius, vagus, and glosso-pharyngeus nerves pass inwards between these various kinds of fibrils as far as the gray matter round the spinal canal (the floor of the fourth ventricle) each to their corresponding nucleus, the hypoglossus to the anterior gray mass or nucleus, the accessorius, vagus, and glosso-pharyngeus to the posterior and lateral. The course of these nerves is analogous to that of the spinal nerves, and the relation of the hypoglossus to the other three is the same as that of the anterior roots of the spinal nerves to the posterior. The anterior sulcus is rendered shallower by the accession of the pyramidal fibres, and by the intimate intermixture of the white and gray fibrils, and at last disappears. There is no canal in the medulla oblongata the greater part of its length; the continuation of the spinal canal reappears, however, as the commencement of the fourth ventricle. At the point where the medulla oblongata commences, the two halves cease to be symmetrical. The greater part of the white substance of the anterior column appears forced from right to left, and pressed into the right lateral half of the medulla oblongata. Higher up between the superior root of the first pair of cervical nerves and the lowest root of the hypoglossus, the symmetrical appearance diminishes, and less white matter is seen on the right side. Can this increase in size if the left half have any connexion with the heart's action? or is it not rather connected with the *right* muscular half of the trunk? The elementary fibrils of the medulla oblongata resemble those of the spinal cord; but the spinal bodies in the nucleus of the hypoglossus alone resemble those in the anterior gray matter of the medulla spinalis. Those in the nuclei of the vagus, accessorius, and glosso-pharyngeus, are however much smaller, less than one half the size; and can only be seen as fine points with the aid of a glass magnifying fifteen times, while the "spinal bodies" may be seen with the naked eye, when a very thin slice is held up to the light. The smaller differ in size, those in the nuclei of the vagus and accessorius being the smallest. They contain a nucleus like the larger, and show a greater number of longer or shorter spines.

Microscopic anatomy of the respiratory nerves in the medulla oblongata. Dr. Stilling traces the microscopic anatomy of the medulla oblongata and of the nerves proceeding from it according to the four sections we before described. He gives a general and detailed description of the structure of each section, noticing the origin of the nerves, and the changes in the arrangement of the elementary constituents. We shall describe the origin of the nerves only, leaving the other details for the special attention of those of our readers who take interest in histological researches.

Nervus accessorius Willisii. At the commencement of the medulla oblongata, the fibrils of the posterior nerves, after traversing the substantia gelatinosa, pass through the lateral strands, (which now contain gray fibres,) and form a complicated network with the longitudinal fibrils. From this network, bundles of fibrils, which appear silvery under the microscope, are seen to pass towards the posterior surface of the lateral strands at a little distance from the point of insertion of the posterior nerves. Sometimes there is only one thick bundle, sometimes two, three, or more thinner bundles; they are composed of fibrils resembling the other transverse fibrils that are the continuation of the posterior nerves, and they are unmistakably seen to be the prolongation of a part of the nervus accessorius willisii. These fibrils differ, however, from those of the posterior roots of the second and third pair of cervical nerves in this, that whereas the latter penetrate the substantia gelatinosa, the fibrils of the spinal accessory do not touch it, but are separated from it by bundles of white longitudinal fibrils. The other root of this nerve passes transversely inwards to the new gray matter already described near the canalis spinalis, and which is the point whence the fibrils of the nerves of this part diverge. This canal is not now in the centre or axis of the cord, but is approximated to the posterior surface, getting nearer and nearer as it ascends towards the brain, until it forms the commencement of the fourth ventricle.

Origin of the hypoglossus. Anteriorly to the canalis spinalis, and partly on both sides, there is a congeries of larger "spinal bodies," constituting a round, oval, or heart-shaped nucleus or ganglion. Bisected it forms two cylinders, situate anteriorly to the spinal canal, and the one to the right, the other to the left. From this cylindrical mass, the hypoglossal nerves of each side proceed. These nuclei approximate the posterior periphery of the medulla oblongata *pari passu* with the canalis spinalis, and appear on the floor of the fourth ventricle, one being on each side of the groove in the calamus scriptorius.

The accessorius ganglion or nucleus, is situate below and behind the spinal canal, and is most intimately connected with the preceding by fine fibrils; it is the analogue of the posterior commissure, as the hypoglossal nucleus is the analogue of the anterior. This ganglion increases also in size from below upwards; it is of a crescentic form, the centre of the crescent being filled with white matter, and has two pointed extremities, an anterior or inner, and a posterior or outer. From the inner extremity bundles of fibrils pass directly outwards with a slight inclination forward, and appear on the surface of the medulla as the superior root of the spinal accessory nerve. The outer extremity gives off fibrils, a few of which join the preceding, but the greater number form arcs, as they pass forwards and inwards, crossing with radical fibres of the accessorius, and intersecting those of the opposite side, and of the hypoglossus, and uniting in the middle line. The two accessorius' nuclei form the lateral boundaries or walls of the fourth ventricle.

The origin from the anterior gray matter of the fibrils composing the pyramid may be seen with the naked eye, if a longitudinal section be made through the portion of the medulla named. Dr. Stilling agrees with Rolando in stating, in opposition to a host of authorities, that there is no commissural union of the anterior pyramids, no white fibrils of the anterior column decussated from right to left, or *vice versâ*. Two nuclei (kerne) are described as existing within the pyramids, which Dr. Stilling names the greater and the less.

Origin of the vagus, and its nucleus. The fibrils of the vagus pass horizontally into the medulla, and enter the mass of gray substance which rests on the floor of the fourth ventricle, and is known as the alæ cinereæ; this is the nucleus or ganglion of the vagus. It is a direct continuation of the nucleus of the spinal accessory, which it closely resembles in form, in the mode in which the fibrils pass off, and in structure; only it is much finer and softer, presenting a peculiar, gelatinous, bluish, semitransparent appearance. Its inner margin is in relation with the nucleus of the hypoglossus with which it is most closely connected, and it passes so insensibly into the nucleus of the accessorius, that the limits of the two are not perceptible.

The glosso-pharyngeus, and its nucleus. The roots of this nerve arise like those of the preceding, from a mass of gray matter situate, internally, and posteriorly, in the medulla oblongata. Both the nucleus and its radiating fibrils have the most intimate union with the preceding.

Anatomy of the fourth ventricle. According to Dr. Stilling, the floor of the fourth ventricle is made up principally by the nuclei just described. First, at the inferior extremity, embracing the point of the calamus scriptorius, there is a crescentic flat mass of gray substance, having a soft reddish appearance, from which, on each side, the funiculi graciles and funiculi cuneati diverge. This is the nucleus of the spinal accessory nerve. Adjoining to and just above this, on each side, there is a triangular mass of gray substance, whose points form the points of the calamus scriptorius, and whose bases project, and are directed upwards and outwards. This is the nucleus of the vagus. On each side, close to the middle line is situate the hypoglossal nucleus, its outer margin is applied to the nucleus of the vagus nerve; its inner forms the slit of the calamus; it is also triangular in shape, the point being downwards, and the base upwards. Close to the base of the triangular mass of gray matter which forms the nucleus of the vagus, is another triangular mass; this is the glosso-pharyngeal nucleus; superiorly and externally is the continuation of the funiculus gracilis, and funiculus cuneatus. It ceases at the point where the transverse fibres of the acoustic nerve rests on the floor of the ventricle.

There is a short notice of the anatomy of the medulla oblongata in the calf. The organization is in general similar to that in man. Dr. Stilling notices, however, the following points of difference. In the calf, the two sides are perfectly symmetrical; the nucleus of the larger pyramid is larger than in man, and a section presents two or three sigmoid convolutions; there are no new pyramid-fibres, and the pyramids themselves and the olivary bodies are less strikingly developed. The sulcus is less deep, and the white portions do not interlace as in man.

Functions of the medulla oblongata and its nerves. Dr. Stilling attributes two principal offices to the medulla oblongata, similar indeed to those of the spinal cord. 1. It excites and maintains the energy of the nerves derived from it. 2. It receives impressions from the sensitive nerves and conducts them, by means of the longitudinal and transverse fibrils in every direction, upwards to the brain, and downwards to the cord, thereby exciting those parts; or it distributes a stimulus and excites an activity within its own structure, and through this, in the nerves arising from it. In the spinal cord, the close relations of the substantia gelatinosa, and of the "spinal bodies" to the sensitive and motor nerves lead to the inference that the former is subservient to sensation, and the latter to motion. So in the medulla oblongata, we find the nucleus corresponding to the layers of "spinal bodies," and the nuclei of the accessorius, vagus, as glosso-pharyngeus correspond to the substantia gelatinosa; so that the latter have a sensitive, the former a motor function. But indeed, the accessorius is of a mixed character; while the inferior and middle twigs correspond in their origin to anterior or motor nerves, the superior (those nearest to the vagus) arise from the nucleus already described, and are therefore to be considered sensitive twigs, and this is shown not only by their origin from the analogue of the substantia gelatinosa, and their close relation to that structure, but also by the intimate union of the elementary fibres with those coming from the hypoglossal nucleus. According to this view the twigs given off by the spinal accessory to the hypoglossus and vagus nerves are sensitive, and derived from this portion of the medulla, while the principal or motor branch is made up of the inferior and middle filaments. Dr. Stilling says, that he has excited reflex motions in the tongue of an animal by irritating the superior twigs, but does not seem to place much reliance on his experiment.

The hypoglossus. Dr. Stilling has confirmed by vivisections the views he adopted à priori, respecting the functions of this nerve. On irritating the hypoglossal nucleus of cats and whelps with a couching-needle, the most distinct motions of the tongue were excited, and of its right and left side, accordingly as the left or right nucleus was irritated.

The skull of the animal experimented upon was opened, the cerebellum removed, and the walls of the fourth ventricle exposed. Dr. Stilling denies that irritation of the hypoglossi themselves will excite motions in the tongue. As the connexion between the hypoglossus, glosso-pharyngeus, and vagus is so intimate, he tried whether irritation of the latter would excite reflex action in the lingual muscles, but without effect.

Functions of the vagus. From Dr. Stilling's previous remarks on the anatomical origin of this nerve, we have inferred that in his opinion it belonged to the class of sensitive nerves. But he subsequently states it to be of a mixed character. This mixed function he attributes to the "lesser spinal bodies" which first show themselves in the nucleus of the vagus, and are analogous to the greater in the anterior columns. He supposes that the former may be subservient to involuntary muscular action, as the latter is to voluntary. Irritation of the vagus nucleus did not induce those movements of the viscera which irritation of the roots of the nerve excites.

The functions of the glosso-pharyngeus, like those of the vagus, are of a mixed character, (for the reasons stated respecting the preceding,) being both motor and sensitive. With regard to the general functions of these nerves, Dr. Stilling observes, that all impressions which traverse the substantia gelatinosa, from the spinal cord upwards to the medulla oblongata, excite the superior roots of the accessorii, vagi, and glosso-pharyngei, and through these the roots of the hypoglossi. We can thus easily understand how disagreeable and painful impressions excite sudden outcries, changes in the respiration, the various movements of the tongue, trunk, &c. The brain may also excite the medulla oblongata in this way from above, as well as the spinal cord from below. Dr. Stilling also remarks that the condition of the medulla oblongata will have an influence on both the brain and spinal cord, and thus we can understand why diseased states of the viscera may influence the central organs by acting on the nerves in connexion with the former. We need not enumerate the functional diseases of the brain and spinal cord that may be thus produced: tetanus, trismus, delirium tremens, hallucinations, &c. are some of those enumerated by our author. We should infer, from the preceding, that the pathological changes in hydrophobia will be found in the structures forming the floor of the fourth ventricle.

Seven plates exhibit the anatomical details (magnified fifteen times) above described, and correct the errors of the plate given in the first number of the work. It appears from our author's observations, that the latter are not worthy of credit; indeed, that is sufficiently proved by the plates themselves, for they have rather the characters of theoretic diagrams than of true representations of nature, resembling in this respect the plates of our earlier anatomical works.

The "spinal bodies" so frequently alluded to in the present number, nowhere appear in the preceding plates nor scarcely a trace of the distended capillary vessels for which they were mistaken. Dr. Stilling takes great pains to convince his readers that the present plates are done with the greatest care and accuracy; without actually impugning the latter, we certainly must suspend our judgment, simply, in fact, because they appear so very complete.

The method Dr. Stilling followed was this. First, the medulla oblongata and spinalis was placed in weak alcohol, (the membranes being removed ?) for twenty-four hours. This was then poured off, and stronger added, and then in two or three days poured off again and the strongest alcohol substituted. Of course, the mouth of the jar is closed with a bladder. In four to eight days it is sufficiently hard to be sliced for examination. This is done by a sharp and highly-polished razor; the blade must be wet with alcohol each time it is used, and the slice must be so thin that the polish of the razor can be seen through it. Dr. Stilling gives directions for obtaining this excessive tenuity, which nothing, we are certain, but frequent practice can arrive at. After a slice sufficiently thin is obtained, it is removed to a glass-plate, and wet with alcohol, &c. No compression is used, so that the structure is seen in its natural state. Dr. Stilling very handsomely offers to send slices to those physiological inquirers who cannot "cut thin" themselves.

In the next number we are promised a description of the organization of the pons varolii and cerebellum, and of the origin of the nerves in connexion with the former. Undoubtedly, the mode of examination adopted by Dr. Stilling, is a great improvement in physiological research, and we await the result of his future inquiries into the structure of the brain with some interest.

II. DR. HANNOVER'S RESEARCHES. A prize having been offered by the Royal Society of Copenhagen, for the best essay upon the "advantages and results which have accrued to physiological science from the recent microscopical investigation of the nervous system," Dr. Hannover became a candidate for the honour, and the work before us is the result of his labours. The observations detailed by him were made exclusively upon aged or adult animals, and were restricted to those which could be easily procured, in order that his investigations might be repeated. In examining the brain and spinal marrow, his attention was chiefly directed to the cells, which exhibit much more variety than the fibres. The drawings were all made from the microscope by the camera lucida, and perforated mirror fixed to that instrument in an ingenious way, and they are all of about 340 diameters.

Microscopic anatomy of the nervous system of vertebrata. Dr. Hannover states that the brain and spinal cord consists of two principal elements,—cells, and fibres,—the cells being vesicles consisting of a membrane enveloping fluid contents, with one or more nuclei, and the enveloping membrane, a very fine-grained substance. The cells are generally round, often oval, sometimes triangular, or one end is prolonged to a point, while the other is rounded. Their circumference varies from that of a human blood-corpuscle to that of six or twelve blood-corpuscles of the frog. Their form is much modified by pressure one against the other. The fluid contents of the cells are seen with difficulty, but the nucleus can be observed to change its place, when the object beneath the microscope is slightly shaken. The cells certainly contain a fluid, and not air. The nuclei of the cells are of a darker colour, and are defined by a sharp dark outline. Their diameter varies from that of a human blood-corpuscle to that of a blood-corpuscle of a frog, and in very large cells is even larger. There is seldom more than one nucleus in each cell.

More nuclei than cells are often met with, apparently under the microscope, for the cells are sometimes entirely filled with the nuclei. The nucleus itself is a vesicle filled with fluid. Dr. Hannover asks whether these are to be considered as complete cells of small size, or whether the nuclei are vesicles surrounded by a cell-membrane, so that one vesicle is contained within the other. Dr. Hannover inclines to the former idea.

Within the nuclei are found one or more bodies, (kjerne legern, corpora nucleorum.) Their size sometimes equals that of a human blood-corpuscle, but they are more generally mere points. The largest that he has met with were in the spinal cord of sucking animals, and from their occasional double appearance, he supposes them to be likewise vesicles. The cells of the brain are to be found wherever the cerebral substance is not entirely white, but in the purely white substance there is not a single cell.

Fibres of the brain. (Hjerne traadene.) They are cylindrical, and consist of a cylindrical axis, a pith or marrow, and a surrounding sheath, and are best seen in the floor of the fourth ventricle. They appear to be enveloped in a double sheath, the inner sheath lying next to the cylindrical axis. This latter often extends itself, like a fine thread, beyond the broken portion of the fibre. The most minute fibres of the brain are probably composed in like manner; and this may be seen when the sheath of any of them becomes *rumpled* or varicose, when a small central line or cylindrical axis can be perceived, which is not affected in a similar way. The white matter of the brain is formed exclusively of fibres, but they also occur in small numbers in the gray substance, where they are much softer and more tender.

The fibres of the brain lie generally parallel, *without* forming anastomoses; and they also frequently lie in bundles, as in the corpus striatum and optic thalami, &c., as may be seen with the naked eye.

The origin of the cerebral fibres, and their continuation into the nerves of the periphery of the body. The fibres of the brain spring from the cells—from the cell itself, in fact, and not from its nucleus, though the latter appears to be the case when the nucleus occupies the entire cell. No more than two fibres spring from one cell, but *one* is quite as frequent. Dr. Hannover also thinks that the connexion between the fibres and the cells may cease when the former are completely formed. A torn and overhanging portion of the varicose sheath of the fibres may be mistaken for an attached cell. Nor must we mistake the tail-like prolongations of some cells for fibres.

The fibres run in the brain from the superior portion to the bases, and have consequently a perpendicular direction; but in the layer of white matter immediately below the gray, they run parallel to the surface. Many fibres pass off to the roots of the nerves that spring from the brain; others descend into the spinal marrow, and, bending from their downward course at an obtuse angle, enter the roots of the nerves, and become nervous fibrils without interruption. The transition from the one to the other takes place rather suddenly, and Dr. Hannover states that he can discover no *real* difference of structure between the two, though the cerebral fibre differs in thickness, and is more tender, and more liable to form varicosities.

Certain fibres cross the spinal cord from one side to the other, besides those which descend perpendicularly from the brain. Dr. Hannover imagines these to end in the roots of the nerves, but acknowledges that he has had the greatest difficulty in investigating the microscopic anatomy of the spinal cord. It is certain, however, he says, that no real *crossing* of the fibres takes place in the cord.

Arrangement of the fibres of the brain. Dr. Hannover confirms by the microscope the observations of Remak and Baillanger, that the superficial gray matter of the brain is composed of several layers. We pass over the details exhibiting the microscopic anatomy of the brain, and spinal marrow of the perch, common frog, triton cristatus, and domestic fowl, to some details respecting that of mammalia. In the brain of large mammals and of the human species, the gray substance is seen to consist of six layers, which Dr. Hannover has figured. The fibres run horizontally on the convolutions in the external or first layer, which is exceedingly thin. The whiteness of some of these layers depends upon the deficiency of cerebral cells (ganglion-globules;) which latter present great varieties in size and form, as they occur in different parts of the brain. These varieties are described by Dr. Hannover. It appears that, with respect to the ganglion-globules of the pituitary body in man, the prolongations of the larger cells are very remarkable. Dr. Hannover has found no fibres in any of the glands.

In the lower animals (frogs) Dr. Hannover has seen ciliary movements in those parts that lie contiguous to the cavities of the brain, and he has often also observed an undulatory motion, where the cilia (flimmerhaar) could be seen. He does not believe in the ciliary motions reported to be observed in the nerves.

In discussing the formation of the fibres of the cerebro-spinal nerves, Dr. Hannover states, that the component parts of the fibres, as observed in the brain, are still more marked in the nerves. The fibres of a nerve, immediately after being taken from the body, only appear thin and transparent, bounded by two darker lines; but as the pith before alluded to coagulates, then the axis or central canal becomes visible, and the whole appears more opaque. The central canal Dr. Hannover considers to be a hollow cylinder. The inserted figure in p. 39 is to prove this; but Dr. Hannover saw it only once, and therefore wisely declines to assert this as a fact.

Microscopic anatomy of the ganglionic system. The ganglionic nerves consist of three nervous elements, viz. cerebro-spinal nervous fibres, ganglion-cell or globules, and vegetative nervous fibres. The proportions of these vary greatly; the cerebro-spinal nervous fibres existing only in very small proportion in the ganglia, and are entirely wanting in the ramifications that spring from thence, while the presence of cells (ganglion-globules) first announces the existence of a ganglion. The number of the cerebro-spinal nervous fibres is greater in the cerebro-spinal than in the sympathetic ganglia.

The ganglion-globules are observed in ganglia of every size. Dr. Hannover considers them as distinct from those of the brain. They consist of an enveloping membrane, its contents, and one or more nuclei, and nucleus-corpuscles. The membrane of the cell or globule appears to consist of small (perhaps hexagonal) plates, which give a good deal more consistence to the globule than is possessed by those of the brain.

Their form is generally round or oval, seldom with any caudal prolongation. The contents of the globule are probably the same as in those of the brain: the nuclei are darker coloured, and often more numerous, especially in fish. The "vegetative nervous fibres" are found in the ganglia and in the nerves proceeding from them, and are distinct from the cellular tissue of the ganglia. They are very tough and pale, and do not exhibit any lines internally. Dr. Hannover has never observed an axis-cylinder (the term is expressive) in them; but he thinks that he has perhaps detected a cavity in those of fishes. A number of nuclei are visible upon these fibres: they are round, oval, or oblong, and their outline dark and sharp. They are often connected with very fine fibrils, and appear as if placed within a very fine sheath. The vegetative nervous fibres are spun round the ganglia, so that we see the same fibre several times. They spring from the cells (ganglion-globules) as a caudal prolongation, and many arise from one cell.

To determine the distribution and termination of the nerves in the muscles, Dr. Hannover made his observations chiefly upon the muscles of the eyes of frogs, and of the water-newt, as, from their small size, the whole muscle could be brought at once under the field of the microscope. He is of the commonly-received opinion, that the nervous fibres form loops *upon* the muscular fibres, and that they terminate in this manner. In the recti oculorum of frogs he fancies that he has discovered a curious numerical proportion between the nervous loops, and the number of muscular fibres, the latter being double the number of the loops, or *equal* to that of the *arms* of the said loops. The relative numbers are given at p. 40.

In prosecuting his inquiries as to the distribution and termination of the nerves of the skin, Dr. Hannover examined the membrana nictitans of newly-hatched birds (pigeons, &c.) He found that the nerves supplying the skin terminate either in large loops, or else end suddenly, sometimes in a point, and sometimes they remain of a similar thickness as the rest of the fibre, or they are thinner, smaller, and are pointed or rounded off. The ultimate distribution is often in one of the glands of the skin, in which case they exhibit the same structure as before described; but the internal pith and the size of the central canal? (axis-cylinder) appear to diminish gradually. Dr. Hannover confirms, by the microscope, the opinion, that both the cerebral and spinal nerves send branches from one side of the body to the other.

The nerves of smell and sight consist of simple cerebral fibres; those of hearing and of taste consist of cerebro-spinal nervous fibres, of the same character as those found at the roots of other nerves. Dr. Hannover has never been able to trace the terminations of the nerves, in the tongue, or in the nasal cavity; the density of the mucous membrane not permitting him to follow the fibres into the papillæ of the tongue. The retina and its cerebral matter were examined in all the vertebrata, excepting man; for the human eye can rarely be obtained fresh enough for microscopic examination, and no portion of the body changes so rapidly after death.

The peculiar structure of the retina is best seen in fish (especially in the pike, Esox lucius) and in these cold-blooded vertebrata, the eye will remain for twenty-four hours fit for use. Dr. Hannover separates the retina into two portions, the true retina, and its coating of cerebral matter. After scraping off the pigment *from without*, the true retina is seen to consist of two distinct elements, viz. 1, prisms sharp at one end, and blunt at that turned inwards towards the central part of the eye. These Dr. Hannover terms "*Staavene*," staves or rods, prismata peracuta, &c. When single, they appear round, when in numbers and pressed together, they are generally hexangular. The points break off after a short time, and the form is altered. The other portion or element of the retina, as seen from the outside, are the double cones (coni gemini). The innermost portion of these cones, viewed with reference to their position, is the single part or root; this appears more polished than that portion which is directed outwards, and which consists of two cones inserted into the hexangular cells of the pigment. The relative positions of these two elements upon the retina are as follows: The prisms surround the coni gemini as with a stockade. This appearance may be exhibited by scraping off both these bodies and exposing their roots, which then present a beautiful and regular mosaic work. The retina of birds, when placed under the microscope, exhibit beautiful colours, caused apparently by globules of an oleaginous matter, which strongly reflect the light. Dr. Hannover has never found in the vertebrata that the optic nerve terminates near the ciliary processes by plexuses or loops; he has always observed them to disappear gradually in the immediate neighbourhood of those processes. He also denies altogether the existence of the membrana jacobi in the animals which he has examined.

The investigations of our author into the distribution of the portio mollis are next detailed; and then the results of his researches into the microscopic anatomy of the embryo of various animals, and of several invertebrata, as the astacus fluviatilis, helix nemoralis, libellula grandis, hirudo medicinalis, &c. Seven plates, containing ninety figures (some of which are coloured), illustrate the details in the text. Altogether, the book, if not containing much which is absolutely new, displays great industry on the part of its author, and is valuable as corroborating many doubtful points in neurology. We can only complain that it is written in Danish, and we do this as much for the sake of the author as for our own. Very few Europeans indeed, out of Denmark, read Danish.

III. DR. MEYER'S RESEARCHES. The researches of Dr. G. H. Meyer into the physiology of the fibrils of nerves, are literary and not experimental. His volume is one of the works which are professedly written for the express purpose of arranging the new facts connected with the subject communicated to the world from time to time, and placing them in new and varied relations to each other. Dr. Meyer has a theory of his own, for of course a connecting link of theory is as necessary to literary and meditative researches, as it is to experimental.

General physiology of the nervous system. Dr. Meyer's object, in the present work, has been to consider the fibril in its elementary and immediate function rather than in the complex phenomena it displays when collected into masses. He considers the latter, however, by arranging the nervous fibrils into two groups, the peripheral and the central. The peripheral is divided into motor and sensitive, animal and sympathetic fibrils. Dr. Meyer makes the nervous fibrils of the vascular system a subdivision of the motor, being of opinion, however, that they belong both to the animal and sympathetic system.

Neither our author nor our readers will expect us, we hope, to give a detailed analysis of a work comprising a review of the whole of modern neurology, and compressed into 316 small, closely-printed, octavo pages. It will be better, perhaps, to refer generally to the works before us, and inquire how far the facts detailed elucidate certain obscure points in this branch of natural science. It is generally taught that there is a gradation in the development of the nervous system of animals from the lowest radiata to the mammalia, and from the lowest mammal to man. Physiologists have traced the points of connexion of each animal with the other with an enthusiasm not dissimilar to that which inspires the antiquary. The striking analogies between portions of the osseous, vascular, muscular, and nervous systems in each and every class have been pointed to as proving a unity of plan throughout the whole. Now, if this leading doctrine of transcendental physiology be true, (and we think it is,) the nervous system of animals must be studied as one whole, and the structure and function of distinct classes and tribes be elucidated by the knowledge derived from this study. In this respect, the works before us present little information. If the nervous system of the polype, or of the radiata, be analogous to the sympathetic system of mammals, our knowledge of the former is in no respect made to elucidate the latter. This doctrine of unity of plan has another important application. A priori, a knowledge of the structure and function of the sympathetic and its ganglia ought to throw light on the texture and functions of the spinal cord; and a knowledge of the texture and function of the spinal cord should elucidate cerebral anatomy and physiology. Now, we certainly do not find this application of the doctrine alluded to systematically carried out in the works before us. Isolated facts are mentioned, however, proving that it has not been quite neglected. Bidder and Volkmann could detect no difference between the fibrils of the cerebro-spinal nerves and sympathetic nerves of the embryo calf, even with the best microscope, and we have seen that Purkinje and Rosenthal inferred from their researches that the cerebro-spinal nerves of the fœtus were not different from the permanent sympathetic nerves of the adult, and that the latter should be considered as a lower grade of development of the former. Again, both Van Deen and Stilling insist upon the theory that the spinal cord is a chain of ganglia, as we know its analogue in the articulata to be, basing this theory on the general fact established by their experiments, that frustra of the spinal cord have the functional activity, proper to the whole cord, complete within themselves. Now this we know to be true also of the vermes and articulata. Sections of the spinal cord of the frog are capable of originating combined movements, so also sections of the spinal ganglia of articulata. Again, the connexion of the motor power of the heart and abdominal viscera with the spinal cord, is undoubtedly elucidated by the experiments of Budge; they confirm, by cruel vivisections, what we knew already by the effects resulting from the influence of the passions, &c. The automatic motions observed in animals of the lowest grade are reproduced in the organs of mammals connected with the sympathetic system. These motions, essentially the same, although phenomenally different, are reproduced again in the automatic or reflex movements of the spinal cord, consciousness being superadded or not, just as the case may be; but if superadded, complicating the phenomena, and connecting them with the physiology of the encephalon.

And then, with regard to the physiology of the brain—what inference can we draw, or rather can we avoid drawing from the doctrine alluded to? Is it not that the analogue of cerebral structure and function is in spinal structure and function? And if so, what is the analogue of that so-called reflex action in the spinal cord which mimics the motion excited by the will and guided by the understanding? The field of conjecture and experiment which this view opens to both the physiologist and pathologists, is boundless. The pathology of insanity, and other results of disturbed cerebral action, of animal magnetism, and of other changes not rigidly to be termed morbid, may possibly be both elucidated and explained to an unhoped-for extent, by classing their phenomena with the reflex phenomena of the spinal cord. Nor would the good results end there, for as the brain is the organ of mind, mental philosophy would necessarily participate in the general progress. The instinctive actions of the lowest animals are reflex in character; are those of mammals less so? If not, then this is the route by which we should seek knowledge respecting the physiology of the passions.

Meyer advocates some of these views directly or indirectly, although his ideas are given too much in the Kantian jargon, apparently in deference to immaterialism, to be intelligible. Meyer argues also that there is no real difference between the motor and sensitive nerves, and with some plausibility. We will not venture to state the analogies which support his notion. The resemblance between the ganglion-cells of Hannover, and the "spinal bodies" of Stilling, is too striking to be passed over unnoticed. There are one or two other points of detail, to which we would shortly refer. It will be seen from Stilling's researches, that the elementary fibrils of the nerves interlace with each other in the spinal cord. Now, what takes place within the cord, takes place more visibly without; indeed, we think that the various plexuses of the spinal nerves strongly prove the accuracy of this part of Dr. Stilling's researches. It is precisely analogous to what is seen in the venous and lymphatic systems. There we find small branches uniting with larger, and those into still larger, until they constitute a main trunk. The minuteness of the component parts is indeed merely relative; what we see with the naked eye to exist is only that on a larger size which we observe under the microscope.

But how can we understand that continuity of structure between the central and peripheral extremities of motor and sensitive nerves generally supposed by physiologists to exist? If there be the intimate intermixture stated, first in the ultimate distribution of the nervous fibrils, next in the plexuses usually described, then again within the spinal cord, and in a mode analogous to that observed in the ultimate distribution, and again very probably within ganglia after ganglia in the encephalon, a structure necessary indeed to the production of those multiform phenomena which a single impression on a sensitive nerve will produce, how can be understood the isolated and perfect continuity of the nervous fibrils in their whole course? To increase the difficulty still more, we find that lower animals, as vermes, arachnida, and certain species of reptiles, have whole limbs reproduced, and in man himself, new portions of resected nerve are formed with little or no disturbance of the motor or sensitive powers. Without venturing upon a decided opinion, we may here at least observe, that this doctrine of isolated conduction and continuity, so commonly asserted as undeniably true, requires further investigation, and such modification as will harmonise it with the results of more recent anatomical research.

Art. XII.

Darstellung der Verfassung und Einrichtung der Baumwoll-Spinnerei-Fabriken in Nieder-Oesterreich. Mit besonderer Beziehung auf die moralisch-intellectuelle und physiche Erziehung der daselbst verwendeten Kinder und die diessfalls bestehenden gesetzlichen Vorschriften. Von Dr. Jos. Joh. Knolz.—*Wien*, 1843.

An Account of the Constitution and Management of the Cotton-mills in Lower Austria. With especial reference to the moral, intellectual, and physical education of the children employed in them, and to the legal regulations subsisting in this respect. By J. J. Knolz, m.d.—*Vienna*, 1843. 8vo, pp. 96.

A brief period only has elapsed since we had occasion to examine, at some length, the oft-discussed question regarding the sanitary influence of the factory system; and our investigations brought us to conclusions varying, to some extent, from those ordinarily prevailing with respect to this matter. It did not appear to us, upon any well-founded evidence, that cotton-mills, in their prejudicial influence upon the public health, differed materially from other work-places which furnish employment to large numbers of the population. That health was deteriorated, and life shortened, by factory working, particularly in childhood, we never doubted; we considered, however, that a similar result flowed, more or less, from almost every occupation characterizing and associated with a high state of civilization; and again, that, whatever ills did really arise to health from the *industrial* relations of large numbers of artisans, their *domestic* condition was yet more in fault. It was clear to us that the position of the labouring classes, as a whole, was far less compatible with health and longevity than that of the superior grades; that much that was injurious in these regards resulted, however, from an aggregation of the evils brought to light by recent researches into the state of our modern cities; and, in a word, that in the instance of our manufacturing population, the sanitary grievances came much more from the *great town* than the *factory system.*

It was not our object to constitute ourselves the apologists of the factory system, and still less to deny the deleterious consequences resulting from the wholesale employment of young children in cotton-mills, as at present constituted: our aim was to strip this question of much of its garniture that was false and exaggerated, in order that the actual evil might be recognized at its true source; and it seemed to us that this was but little likely to occur whilst violent and unjust denunciations of factories and their occupiers, took place on the one hand, to be neutralized by excessive and partisan panegyric on the other. We had not, nor have we, any notions or views upon this subject, other than those which have issued from an impartial examination of the evidence; and, in committing to paper the conclusions at which we may have arrived, we are influenced by no considerations foreign to our professed purposed, that of anxiously striving, with truth for our cynosure, to attain accuracy in our statements, and justice in our criticism; a principle, we may add, which can alone guide the progress of this Journal under its present management.

We have been led to these remarks, in brief recapitulation of our former argument, by the perusal of the concise and valuable little work, the title of which we have given above; a brochure published in the course of last year, and full of interest from the circumstance of its demonstrating by its statements the correctness of our position, that factory working *per se* is not necessarily more prejudicial to health than most other occupations, but that the ills which in many cases have arisen from and been associated with this branch of industry are of a nature, to a considerable extent at least, admitting of removal. We think it most important that just notions regarding this question should obtain currency, for it is one which has been, and is likely yet to continue, a subject of legislation with a view to improvement; and, under present circumstances, we very much think that any mere shortening of the hours of labour, hitherto the principally prescribed method of amelioration, would only diminish to the workpeople the means of supplying themselves with due sustenance, and at the same time condemn them for a longer period to breathe the impure atmosphere of their own dwellings, and in such way operate still more deterioratingly upon their health.

We gather from Dr. Knolz's publication that, in Lower Austria, the hours of labour, in the cotton-mills, are more extended than in our own country, and yet that, generally speaking, the health of the operatives is in a better condition than that of other classes of workpeople,—a result attributable not, we conceive, to the character of the occupation itself, but to the excellent regulations, hygienic and otherwise, employed by the authorities in the management of this department of administration. As involving points of considerable interest in themselves, and as indicating, in our judgment, the direction in which legislative interference should run, we shall supply to the reader some of the particulars; and although the special circumstances of different countries will but rarely allow one state to take another as an exact pattern, even in things relating to the subject of our present discussion, yet the exhibition by one nation of a broad truth, developed by experience, may always be rendered, in some measure, available by every other, bent on progressive advance and improvement. We proceed to furnish a brief analysis of some of the matters contained in the little work now before us.

The factories described by our author are situated, for the most part, not in the centre of large cities, but in hamlets, villages, and small country towns; localities, generally speaking, through which great brooks take their course, the waters of which can be turned to account in the prosecution of the work. Most of these districts are open plains at the extremities of the villages, well exposed to the air, and so far most advantageously situated with regard to hygienic conditions. All considerations relating to security against fire, and the healthfulness of the foundations, and other collateral circumstances of this kind, are determined by the authorities, prior to the erection of the mills; and it is stated that, even if no legal regulations subsisted in this respect, the disposition of the capitalist would generally direct him to entertain, in his projects, the question regarding the probable health of his future workpeople. In building the required structures, every care is bestowed upon the height and the width of the rooms, and upon the facilities for ventilating them; and, further, in the immediate vicinity of the workplaces, are situated dwellings of a very superior character, the property of the mill-owner, for the operatives themselves, as well as apartments for their treatment in sickness; and, when schools do not otherwise exist in the place, certain divisions of the erection are appropriated for this purpose, and also as residences for the teachers. The due management of all these matters is rendered obligatory upon the occupiers of the establishment. Under all these circumstances, it is affirmed that the children of the operatives, many of whom are associated with their parents in factory-working, are better educated and fed than those of the other villagers, and, being removed from the bad example furnished by artisans in large towns, are strangers to the vices and delinquencies attributed to corresponding classes in France and England.

With respect to the age of the children employed in the cotton-mills, it is at the present time seldom that any are admitted under the age of twelve, the interest of the occupier, in the estimation of our author, demanding that younger children should not receive admittance, as from frivolity, want of thought, and general unfitness, they would too often be likely to occasion mischief. However, occasionally, they are received into the mills so early as the age of nine, from consideration to the parents, who, being themselves employed in the factories, could not always, if the contrary rule admitted of no exception, retain a suitable watch over their moral and physical welfare; and the children out of work might thus pursue, uncontrolled, the road to destruction. These infant labourers, however, are stated to be very few; and in all the mills of Lower Austria, those from nine to twelve years old do not amount to a twentieth of the children; and these latter come to about one fourth of the whole number of the employed.

The time of labour for those of twelve years of age, is about six hours, or six hours and a half, in the forenoon, and the same period in the afternoon, with an interruption of an hour and a half at mid-day.

The species of occupation exacted from children in these estsblishments requires but little bodily exertion; and, as a matter of prudence in the management of the business, the division of labour is governed most carefully by the physical strength and capability of those to whom particular kinds are awarded. It consists, among the elder children, in the placing of cotton upon the carding-machines, in the carrying to and fro of small tin cases filled with cotton-wool, and such like light employment; and, with the younger, in piercing the threads and screwing the caps, at the spinning-machines. The continuance of this exceedingly easy and simple work is rendered still less fatiguing by the fact of its requiring a frequent change, both of posture and place. Labour requiring any exertion is consigned altogether to adult persons. The author bestows a glowing eulogy upon the mildness, the probity, and the general excellence of the young people engaged in these establishments, and the result he assigns to the wise and benevolent discipline to which they are systematically subjected. He affirms that nowhere, in his part of the world, are children treated with the barbarity which has obtained in some districts of England; that such a disposition does not exist in the character of the mill-owners, and that any deviation in this point of view would quickly be visited, not only with the indignation of the people, but with an interference of the government. From these last remarks, and some others occurring in the course of the work, we are led to believe that the author's notions respecting our own manufactures have been obtained, directly or indirectly, from the publication of the parliamentary evidence on which we animadverted at some length in our former article. In referring to the way in which the operative classes are treated in the Austrian factories, he observes that, beyond the care arising from the paternal character of the government regulations, and the disposition of the masters, they are necessitated by a regard to their own interests, in consequence of the great demand for labour, to protect and welltreat the workpeople, as well as to provide for excellence in the sanitary state alike of the mills, dwellings, sick-wards, and the schools.

We have given the above description of the physical conditions in which the Austrian factory-people are placed, as a just illustration of a reasonable and practicable state of things, calculated to exhibit, in an isolated manner, the destructive effects to health, if any, resulting from the working in factories; inasmuch as here most of the other provocatives to disease, commonly obtaining amongst corresponding classes in this country, seem to be withdrawn.

Now, if the amassing of individuals in mills, to the extent necessary to the successful prosecution of the work, and the required temperature, and the demanded constraint of position, and so on, were really the cause of all the horrors, revealed or anticipated, by many who have dwelt upon this subject, we should certainly have them displayed even in Austria; for it seems that the establishments are usually in full vigour of operation for somewhere about fifteen hours a day, and the young children are engaged in actual work for about thirteen hours. Let us, then, see what Dr. Knolz states upon this matter—the account which he has furnished being stamped, so far as it can be judged from style, with all the outward characteristics of candour and truth.

Our author very properly premises that the physical condition of the children employed in the factories may be deduced from the circumstances in which they are placed, as already described. They work, he says, everywhere, in the mills in question, in lofty rooms, which, on account of the business itself, are kept in the cleanest state possible, being in summer most carefully ventilated, and in winter well heated by means of Messner's stoves, or by a circulation of hot water in tubes. Their occupation, he repeats, is light, requiring varying postures; and, in every way, they are better protected against influences prejudicial to health than young people engaged in other kinds of work. He goes on to say, that their excellent state of health must strike every observer who sees them, at the close of day, leave the workplaces in a frame of mind the most cheerful conceivable. The larger mills have been in operation now for almost thirty-six years, within which time there must have been a change in the children employed at least nine times, yielding, within one generation, according to a calculation made, an aggregate of 21,000 individuals who passed four of the best years of their childhood in factory labour; a circumstance, he affirms, proving by a long experience, that no needful damage is done to young children by this occupation, considered in and for itself, and with an exclusion of all the other removable sources of physical deterioration. Notwithstanding so large a proportion of the villagers have, for so many years, been as it were brought up in the mills, not the slightest trace of a crippled or stunted race is discoverable in the vicinity, either during a military conscription, in the medical visitations periodically taking place, or in the hospitals or alms-houses. In the ravages of recent epidemics, which always devastate and scatter away the feebler members of any community, experience furnishes a still more important demonstration, how carefully the health of the factory-people is preserved. Everywhere, near the larger mills, infirmaries are instituted, which are furnished with well-paid physicians, surgeons, in order that all—adults as well as children—may receive appropriate care and attention during illness; and, in all cases of epidemic disease, the mill-owner is compelled by existing regulations to provide everything at his own cost. As the successful result of all these arrangements, it is stated by Dr. Knolz that the instance may be adduced of what obtained during the prevalence of cholera in 1832, in the largest factory in the monarchy, at Pottendorf, where upwards of 1200 persons are employed, and of whom only three fell victims to this pestilence. Moreover, he affirms, that at no time is an unusual mortality to be observed among the factory-workers; on the contrary, he conceives that many diseases are avoided, through an absence of their causes, such as exposure to undue heat or cold, over-exertion, and the like causes, to which many other classes of artisans are exposed. Further, he says, it has been observed that the children who are engaged in factories, under careful supervision, are much less subjected to the ordinary causes of juvenile illness than those otherwise occupied.

No diseases are noticed to prevail endemically in those regions which are peopled by factory-hands; and the sickness and mortality are stated by our author to be in far greater proportion amongst the children of the villages unconnected with cotton-mills. The illness that actually does arise amongst the factory-labourers has its origin much oftener in the hours of recreation than in those of work; and when occurring it consists, for the most part, of some gastric derangement or catarrhal affection, which cannot well have had its cause in the apartments of labour. When the factory-children suffer and die from scrofulous disease, its germs are brought with them, and not contracted in the mills. Generally speaking, however, their state of health is most satisfactory, that of which every unprejudiced observer may convince himself by watching them at the periods of recreation, or on festival-days. In vain will a search be made for those unhappy and crippled youths who may have been discovered, probably, in some other states, and especially in England, the circumstances of which country differ so thoroughly from those of Austria, and which have given occasion to the philanthropic tirades (philanthropischen tiraden) of so many writers. Dr. Knolz winds up his remarks on this department of his inquiry by stating that it has been rendered quite certain, by a long experience, that, in thoracic disease, an abode in the warm atmosphere of a factory impregnated with oily particles, is very beneficial to all so affected; and thus, that children and old people often resist maladies of this kind years long, ever feeling themselves best whilst engaged in the workrooms.

We have now exhausted the substance of Dr. Knolz's pamphlet, so far as it relates to the influence of the factory system, in Lower Austria, upon the health and the general physical condition of the population, to whom it constitutes the source of subsistence. Although it is very likely that a little exaggeration in some of the details, and an unduly favorable colouring in the portraiture may occur, we yet feel no doubt that, in the main, our author is just and faithful. We think that the positions taken in our more extended article of last year, have here been strikingly corroborated, and their accuracy indeed demonstrated. We maintained, and again maintain, that working in cotton-mills is not *essentially* more prejudicial to health than the bulk of other kinds of labour, and that it is less so than many that have not occasioned the same degree of public sympathy and excitement. As an object of national polity, we are quite sure that it is its *regulation*, and not its *discouragement* that is needed; and, with respect to any legislative modification of the factory system in a sanitary point of view, we distinctly assert our conviction that further reduction in the hours of labour, involving, as it must, diminished wages as a consequence, will only, *as a beginning*, lead to an aggravation of the evils that beset the physical condition of the working classes; although, after every other practicable improvement shall have been accomplished, in the state alike of their homes and their work-places, such a proceeding may probably have many advantages, especially to the well-doing of the children. We take no cognizance of the economical or political considerations which attend this question; we have neither the knowledge nor the taste for the discussion of matters relating to the justice or expediency of interference, in special cases, with commercial enterprise, or of how far greater restrictions upon the duration of factory labour may have the effect of placing British capitalists at a disadvantage with foreigners; we have never studied these things. We feel confident, however, that, in all matters which affect the health and strength of the factory people of these kingdoms, we have some right to form a judgment; and this judgment before given we now reiterate, strengthened by perusal of the work we have partially analysed, that the ills which afflict this class of persons appertain to their *domestic* much more than to their *industrial* relations; and that the great mass of disease constantly witnessed in our manufacturing districts, flows from the *great town* rather than the *factory system*.

Art. XIII.

Mémoires de l'Académie Royale de Médecine. Tome Dixième.—*Paris*, 1843. 4to, pp. 806.

Memoirs of the Royal Academy of Medicine. Vol. X.—*Paris*, 1843.

Whatever be the differences of opinion regarding the utility of such institutions as the Royal Academy of Medicine of Paris in advancing the interests of medical science, and developing the mental resources of those whose turn it has become to enter within their august pale, there can be no question in the mind of him before whom this very large volume lies, as to their charitable influence in providing work for needy printers. For ourselves, who have been accustomed to take rather a gloomy view of the direct benefits accruing from the discussions carried on by the members of such bodies—in which quickness of repartee too commonly passes for the evidence of superior attainment; in which, while the establishment of truth is the assumed object, its perversion is often the real and scarcely veiled aim; in which the dignity of the scientific inquirer is too often lost in the unseemly display of uneasy egotism; in which the vigour of philosophic disputation merges but too frequently in the noisy brawling of personal recrimination; in which all seem persuaded of the necessity of glorifying themselves, and making their neighbours look as small as possible; in which the point at issue remains at the end precisely as it was at the beginning, because those who possessed some little information regarding it have not condescended to instruct the οἱ πολλοι (*their* way of hugging themselves on their superior wisdom);—for ourselves, we say, taking, as we do, this view of the scientific and moral influence of the greater number of debating societies, it is matter of gratification that such small good even, as that mentioned, may arise from their existence. Was there ever, we would ask, a really valuable contribution, owing its production to such bodies, which would not have seen the light without them? While, on the other hand, look upon the sheets of trash annually generated by the vanity that regards the getting up of a volume in each twelve months as essential to the dignity of the society. To go no further, scan the contents of the unwieldy tome last issued by the Academy of Medicine, with its 750 pages; all that is in the remotest degree valuable here might have been readily conveyed in 150 pages—perhaps even this is too liberal an allowance.

First and foremost come three *éloges* of as many defunct members of the Academy, (whether the persons in question deserve such tribute to their memories is a secondary point, the main fact remains the same, "they were of *us*",) supplied by the indefatigable pen of M. Pariset, the Jules Janin of the medico-scientific arena. If the great and "*à jamais célèbre*" feuilletoniste has been known to make the humblest *ballerina* the celebrity of the town for at least a week—which at Paris means, in these matters, half a century at least—by dashing off a few sparkling lines in her praise, M. Pariset has done no less for many a small exhibitor in the Academy; only, unfortunately, the eulogised *savant* has not been alive to enjoy his brief hour of greatness. We are not disposed for an inquiry into the good taste or honesty of this system of indiscriminate laudation; and, however we might prove to our own satisfaction its deficiency in both, as long as the Academy has a secretary of M. Pariset's caliber, we fear there would be no chance of spreading our conviction to the Rue de Poitiers.

Some of the papers collected in this volume are medical, others surgical; we shall first anatomise the former.

I. M. Hippolyte Royer-Collard, Professor of *Hygiène* at the Faculty of Medicine, proposes to enlighten the Academy upon "Temperaments, considered in their relations to health." In an exordium startling, even among the Memoirs of the Academy of Medicine, for its verboseness and grandiloquence, M. Royer-Collard laments over the imperfection of existing "Hygiène" as a science,—signifies his conviction that, so long as its study is not closely bound up with a sound physiology, this imperfection must continue, and winds up by defining scientific "*Hygiène*" as "the profound knowledge of the relations of human life to *other* natural existences." A most lame and impotent conclusion, deficient even in verbal accuracy; for human life is not an existence in the sense given to this word by the definition.

The author considers first the actual state of knowledge on the subject of his paper; endeavours next to show that the current notions thereupon are false; and lastly, points out the path which he believes best calculated to lead to a more accurate and practical estimate of the various nature of temperament, and of the modifications, compatible with health, produced thereby.

Health is not, we are told, an absolute unvarying state, an entity always identical; it varies in individuals, and this difference has been referred to and expressed by the phrase peculiarity of constitution. This peculiarity of constitution has naturally been made the subject of investigation, and observers led to refer it to this or that special condition of organic structure. Upon this special condition (the predominance of some organic system or fluid of the body) has been founded, as is well known, the distinction of temperaments, *nervous*, *lymphatic*, *bilious*, &c. The author hesitates not to denounce this doctrine as more or less completely false in all its parts, conceived, as it was, under the influence of the old humoral doctrines or an equally erroneous solidism. Take, for example, the sanguineous temperament: Is the quantity of blood here unduly great? Its celerity of circulation above the ordinary standard? Are the heart and great vessels more vigorously endowed than in other temperaments? Is respiration more active and the lungs more fully developed? All these questions are, according to M. Royer-Collard, commonly answered in the affirmative, whereas he would give them either a very decided negative or maintain the affirmative to be unproved. The lymphatic, bilious, athletic or muscular, and nervous temperaments are similarly passed in review in respect of their commonly admitted causes. In all, the result is the same; the author attempts to show the error of accepted doctrines. And he succeeds, truth to say, in the easy task; all has been admitted on the grounds of plausibility and facility. The matter required reexamination; and though the process might have been less prolixly and less pompously performed than by this "candidate for the honours of the Academy" (possibly, aspirants have learned to trust to the avoirdupois, as much as to any other kind of weight of their papers), it is perhaps well that even he has been found willing to undertake it.

No, says M. Royer-Collard, if temperament be a *general* state of the economy, it follows that its source must be sought in something similarly general in the economy, not in an "isolated fluid" as the bile, or a special organ, as the liver or muscles. Its cause then must be sought in the two "principles that animate all the parts of the organized machine, ... the blood and nervous action." Hence he concludes that, "not the cause, but the *characters* of temperaments should be studied in the various manifestations presented by those two principles." And so "*voilà le premier pas fait*," according to our philosopher.

Commencing with the blood, the writer admits the mean relative proportions of the main ingredients obtained by the inquiries of recent experimentalists; insists upon the tolerable wideness of the limits within which those proportions may oscillate, and yet health be preserved; remarks upon the increase of the colouring corpuscles which Andral has shown to attend the state of plethora, as also the diminution of those particles found in individuals of lymphatic temperament. The organic materials of the blood generally follow the same law of increase and decrease as the corpuscles; the ratio of these different organic materials to each other varies, and hereby hang sundry variations in temperament.

These are the views of M. Royer-Collard,—at least a specimen of them, a brick of his Babel. Those interested in such edifices must examine the entire structure for themselves; we have given it as much space as it merits.

II. M. Mélier investigates with a philosophic spirit the important medico-political question of the influence of a varying state of the price of provisions on general disease and mortality, in a paper entitled "*Etudes sur les Subsistances envisagées dans leurs rapports avec les maladies et la mortalité.*" The statistical documents he has collected, and the tables into which his figures are thrown, bear out the following inferences, to the enumeration of which we must for the present limit our notice of the paper. "1, The mortality of a country is influenced by the price of corn and bread; 2, this influence was extremely marked formerly; 3, it is less so at present; 4, the diminution of this influence has been gradual; 5, various causes have contributed to this result; 6, the cultivation of the potato is one of the chief of these; 7, less bread appears to be consumed at the present day than formerly; 8, the consumption has not been calculated accurately, but its amount appears to be on the decrease; [these two propositions refer, as far as the author is concerned, to France only;] 9, the question now considered is one of morality as well as of hygiène; for it is demonstrated that crime increases with the dearness of provisions." It is inferrible too, the author considers from his inquiries, that in a well organized state of society, provisions tend constantly to increase in abundance; this tendency, in France at least, is more marked than that of the population to increase,—"a powerful argument against the theory of Malthus."

III. *Sur la valeur de l'examen microscopique du lait dans le choix d'une nourrice.* Par M. Alph. Devergie. Since the announcements made some years ago by M. Donné, on the subject of the micrology of milk, and especially since that gentleman was intrusted with the very momentous office of choosing, microscope in hand, a wet-nurse for the Count of Paris, it has been a sort of "*passion*" among the very excitable Parisians to study the milk of all manners of women with that instrument. M. Devergie, stricken with the reigning enthusiasm (and naturally enough, for it appears that in 1838 and 1839,* just at the period when M. Donné's discoveries shone with all the eclat of novelty, he was invested with the office of Director of Nurses,) set to work, like the rest of the world, "having first made himself sufficiently skilful in the use of the instrument" to undertake the appreciation of M. Donné's statements. In the course of seven months he collected 172 cases, taken without selection, noting in every instance the following particulars: 1, the date of observation; 2, the nurse's name; 3, her age; 4, her temperament; 5, her constitution: 6, the colour of her hair; 7, that of her skin; 8, the size and various physical characters of the breasts; 9, the width of the chest; 10, the state of the teeth; 11, the length of time elapsed since the cessation of lactation; 12, the age of the milk; 13, the microscopical examination of the milk since the cessation of lactation; 14, the microscopical examination of the milk subsequently to the recommeucement of lactation."

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* The author does not state whether this was the time when the very ingenious observer above named made his remarkable discovery that the conversion of blood-corpuscle into pus-corpuscles is producible by the admixture of pus and blood out of the body! Such achievements should not be dateless.

In respect of its globular character, the author considers that human milk is of three kinds: 1, large-globuled milk; 2, small-globuled, generally "pulverulent" milk; 3, milk of medium-sized globules. Not that any given specimens of milk will contain one or other of these kinds of globules to the complete exclusion of the others, but that there will be a sufficient predominance of one or the other to distinguish it. The large-globuled variety is the most nutritive; and the nutritive properties of the others are proportional to the size of their globules. This statement results from a comparison of the strength and bulk of infants, with the microscopical characters of the milk used to feed them, and also from comparing the milk with the general health and vigour of the nurse. The results were confirmed by examinations of cow's milk. The microscope then will enable us to determine in doubtful cases whether a given milk be of the strong or weak class, and will guide the physician in the choice of a nurse, wherever the question turns upon the advisability of one or other of these kinds.

Milks differ not only in respect of the size of their globules, but also of the abundance of these; high or low amount of the globules signifies richness or poorness of the milk generally: the author uses an arbitrary scale, running from the number five to forty to express variable degrees of these qualities. The milk furnished by the two breasts is not always the same; a difference may exist in respect of the number or size of the globules,—the former much more commonly than the latter.

Out of 100 nurses, 17 had the large-globuled variety of milk; the majority (10) of these persons were of sanguineous or sanguineo-lymphatic temperament. The large size of the globules does not always entail a maximum amount of number of these, in other words, of richness of the fluid. Thus, in these 17 nurses, the degree of richness is expressed by the author as follows:

In 4 cases,	the richness equalled	10
3	" "	15
3	" "	20
3	" "	25
4	" "	30

In 10 of these cases an increase in richness took place during lactation, and

In 5 cases,	the richness equalled	40
2	" "	35
1	" "	30
2	" "	25

This state of the milk does not always coincide with the external appearances of a good nurse; 4 of the 17 individuals referred to were thin, puny, and with prominent clavicles. It is, however, true and well known, independently of microscopical examination, that puny women sometimes make better nurses than those exhibiting a far more favorable physical condition.

The small-globuled or pulverulent milk existed in 22 of 100 nurses; here the temperament of the individual varies, as in the case of the last variety, and in about the same proportion. Of 23 cases of small-globuled milk,

In 9	the richness	equalled	5
2	„	„	10
5	„	„	15
5	„	„	20
1	„	„	25
1	„	„	30

And, as respects the increase of richness produced by lactation, the author found that among 17 nurses,

In 4 cases,	it reached	40
1	„	35
5	„	30
2	„	25
1	„	20
1	„	15
1	„	10
2 no change.		

Whence, comparing these results with those having reference to the large-globuled variety, it follows that the small-globuled is generally poorer in quality, and generally less capable of acquiring increased richness from the resumption of lactation. So that suppose a woman presents herself as a candidate for a nurseship, not having given the breast for some few days, there is less chance of the milk becoming eventually rich, if it present small than if it contain large globules. Milk of medium-sized globules is the kind the most commonly observed; 61 of the 100 women furnished this description.

A woman may present differences in the milk of the two breasts, as already mentioned; these differences of richness and size of the globules are often dependent upon the habit nurses have of suckling with one breast rather than the other, so much so that when the difference is very great, we may be certain that the individual uses only one breast.

Increased richness of the milk is an almost constant result of resumption of lactation after temporary interruption. The age of the milk has no influence in increasing the number or size of the globules: nor does the age of the nurse influence them either. Very large and very small breasts furnish milk of the least favorable qualities; the colour of the hair has no obvious connexion with varieties in the microscopical characters of the milk, and the same is true of the development of chest in different individuals.

Morbid changes in the microscopical characters of the fluid are rare. The most common consists in the agglomeration of globules, a state which may disappear under the influence of lactation. Important changes in colour—the milk may become blue or green to a marked degree—occur without any concomitant modification in the microscopical appearances. Yet the physiological character of the fluid is materially altered, so that the infant's health suffers at once; the microscope, then, is not the sole necessary guide in the question of choosing a nurse.

A good nurse, the author concludes, should be from twenty-five to thirty years old, strong in constitution, full-chested, of sanguineo-lymphatic temperament, brown-haired, having white healthy teeth, and well coloured lips. She should have pyriform breasts, with well-formed nipples, and without too much development of veins. Round prominent breasts with large veins, and broad areolæ are comparatively very inferior. The milk drawn into a spoon should be white with a slight bluish tint, its taste saccharine; it should not be too thick. These facts ascertained, the microscope comes into play, and demonstrates that the milk is or is not altered in the character of its globules, that it is more or less rich in globules, that its globules are more or less large. But the microscope cannot prove that a given milk should be preferred to another in a given case, because the health of the infant depends entirely upon the assimilation of the milk by the digestive organs, and the instrument gives no information upon this head. But the microscope may be of great utility in aiding the physician in the choice of a new nurse in cases where a previous one has furnished milk disagreeing with the infant; whatever microscopical peculiarity exists in the globules of the latter, will of course be studiously avoided in the milk recommended for new trial.

This is a paper of interest and utility.

IV. *Etude de la phthisie à la Martinique.* Par le Dr. E. RUFZ. M. Rufz having returned to his native island, after a series of scholastic triumphs at Paris, (in the last of these, the concours for some vacant places of Professor Agrégé at the faculty, his was probably the most brilliant exhibition made for years, even in that arena of display,) proves himself not forgetful of the scientific pursuits of his earlier years. The Academy of Medicine signifies its desire for information respecting the frequency and peculiarities of pulmonary consumption in warm climates, and the ex-professor agrégé responds to the desire by a paper describing the malady as it prevails at Martinique.

Pulmonary affections, with the exception of phthisis, appear to be singularly rare. The writer has met with but three cases of pneumonia in five years. Chronic bronchitis even among old subjects is similarly uncommon; gastro-intestinal affections cut off the advanced in age. This infrequency of other pulmonary diseases facilitates the diagnosis of phthisis.

The author practises at Saint-Pierre and its environs, in a district containing, according to the official census of 1835, a population of 17,000. Some other practitioners divide the care of this population among them. The following is an approximate view of the frequency with which phthisis occurred in M. Rufz's practice.

In 1836,	among about	600	patients,	40	were noted as phthisical.
1837	„	378	„	21	„
1838	„	445	„	35	„
1839	„	531	„	27	„

Thus, out of 1954 patients, 123 were tuberculous; in other words, about 13 per cent. Phthisis is the most common chronic disease in the island. Scrofulous allied affections are nevertheless comparatively rare; the author has seen but one or two cases of white swelling; no example of tuberculous caries of the vertebræ; glandular swellings also are uncommon, and but two examples of tuberculous meningitis have fallen under his notice. Phthisis is excessively rare in the child at Martinique; the author having encountered pulmonary tubercles but twice in all the post-mortem examinations made of children,—he does not say, approximatively even, how many these were. But it follows from this, that all the author's statements are to be understood as applicable to the adult only.

One hundred and eight of the 123 phthisical patients observed by the author had been followed by him in such manner that at the time he wrote he knew whether they were living or dead; 55, or somewhat more than half were dead. Nine only of these subjects were examined anatomically. The thoracic lesions were the same in these subjects as in European victims of the disease; but it is remarkable that in seven cases hemoptysis was the immediate cause of death, whereas in these countries such a *termination* of the malady is extremely rare. In none of these instances was the author able to discover a ruptured vessel of any caliber, to account for the extravasation of blood.

Tuberculization of other organs besides the lungs is much less frequent at Martinique than with us. In 2 only of 9 cases did the author detect tuberculous ulceration of the intestines, a rarity testified too also by the infrequency of colliquative diarrhea in the phthisis of the island. The mesenteric glands are rarely tuberculous. The liver was fatty in 1 only of the 9 subjects; as we have had occasion to state in this Journal, fatty liver is very rare in this country also, but in France M. Louis met with it in about one third of his post-mortem examinations.

Of 53 fatal cases, 1 only would be regarded as acute in its course; all the others lasted at the least three months, many of them several years, as appears from the subjoined table.

3	terminated by death	in	3 months.
11	"	from	3 to 12 months.
7	"	"	12 to 18 "
8	"	after	3 years.
1	"	"	15 "
3	"	"	30 "
13	"	"	several months, *i. e.* from 6 to 18.

The course of the disease in respect of duration seems to be very much the same, consequently, as with ourselves.

M. Rufz proceeds to the examination of some of the symptoms of phthisis. The consideration of hemoptysis leads him to inquire whether that symptom when severe, is as indubitably significant of the existence of phthisis in Martinique, as it has been shown to be by M. Louis in France. In the course of four years, 73 persons, who had had hemoptysis, fell under his observation. Of these 73, 39 (27 females, 12 males,) died of phthisis; 34 were still living, when the author wrote. Among these 34 persons, 19 were women, and 15 men. Of the men, 10 were in the enjoyment of good health, free from evidence of any kind of phthisis or cardiac disease, and the author is certain that the hemoptysis in them was not an effect of external violence or accident; the other 5 are of weakly constitution, cough and exhibit other signs of phthisis. Of the 19 females, 12 could not be considered phthisical, 7 might be so considered. Hence 51 of 73 persons who had had hemoptysis are phthisical.

The author considers that these results do not differ much from those which led M. Louis to regard hemoptysis as an almost unfailing sign of tuberculous disease. They differ less, we believe, than might on first sight appear; but did they swerve infinitely more from them than they do, the fact would supply no shadow of an argument against M. Louis' proposition, limited as it was to France and countries of similar temperature. If there be a practical point in medicine well established, it is, in our minds, the diagnostic signification in these countries of copious hemoptysis, as pointed out by M. Louis.

M. Rufz found diarrhea exceedingly rare in the phthisical inhabitants of Martinique: this is a remarkable point of difference between the diseases here and in that island.

M. Rufz has employed emetics in the treatment of the disease with the following results: In 3 out of 20 cases, he was obliged to give up their use, simply because they fatigued the patients, and not from any other cause. In the 17 other cases, their employment was almost always followed by relief of cough, increased freedom of respiration, and improvement of appetite. He never observed diarrhea follow their employment, nor was the number of patients suffering from symptoms of softening of the stomach considerable. In two cases emetics arrested hemoptysis which venesection had failed to control.

The author concludes from his own observation that, *except* age, sex, constitution, habitation of large cities, and hereditary influence, all presumed causes of phthisis have in reality a very secondary influence. We know not where M. Rufz has obtained his assured belief of the very essential influence of some of those agencies to which he ascribes such power.

Of the various races inhabiting the Antillas, Europeans and Africans are less subject to phthisis than creoles; and among the creoles, the white and mulatto are more prone to the disease than the negro. M. Rufz further infers that the influence of the climate of the Antillas on patients coming from Europe, in a state of declared phthisis, is not deducible from his own experience; but that, on the other hand, the removal from those islands to Europe of phthisical persons, is extremely pernicious.

On epidemic cerebro-rachidian meningitis and encephalo-meningitis. By M. Rollet. An epidemic disease, as rapid in its course as destructive in its results, has, it appears, for some years past, ravaged the garrisons of Versailles, Lyons, Bayonne, Groit, Metz, Strasbourg, &c. Nor does it appear to have been altogether confined to the military: the civil population of the environs suffered in 1838 under an epidemic visitation, as described by Drs. Lamothes and Lespès, closely analogous to that affecting the garrisoned soldiers of the towns enumerated. This epidemic M. Rollet regards as an encephalitic affection, in which he follows other writers; while, in respect of the precise mode of change and sequence of affections of the membranes and the cerebral substance itself, he differs from them. We shall, as briefly as we possibly can, give some notice of the observations of this writer.

In respect of predisposing causes, extreme youth of the soldier, his having recently joined the service, and being unaccustomed to military exercise appear to be the most obvious. Prolonged exposure to the sun, with or without subsequent chill, but above all violent exercise followed by chill, appear to have acted most frequently as immediate causes. Miasmatic influences, and those productive of infection, are regarded by the author as completely unconnected with the development of the malady.

The symptoms are regarded as belonging to three separate periods; and this both in the cases where the disease is simple meningitis and when the encephalon is likewise implicated. These symptoms are very carefully described; but the enumeration occupies too much space to admit of its extraction, nor could it be condensed to any useful purpose.

The morbid changes detected in the brain, cerebellum, spinal marrow, and meninges perfectly justify the nomenclature adopted for the disease by M. Rollet: Injection of the membranes, with effusion of red-coloured serosity, pus in the pia mater, softening of the medullary substance of both brain and cord.

The treatment recommended for the cases of simple meningitis is essentially the antiphlogistic, accompanied with derivatives to the extremities, purgatives, cold applications to the head, &c. This method of treatment appears to have invariably succeeded, and no fatal case occurred.

But when the substance of the brain and cord is implicated, the case is infinitely more dangerous. Occasionally, when the patient is first seen, he is in a state of collapse, such as renders venesection inapplicable: should, however, a state of reaction exist, bleeding ought to be copiously employed. The great point upon which the writer lays most stress is this: When patients are in the state of collapse — the limbs relaxed, the sensibility completely gone, the skin cold, and the pulse filiform—bleeding is contraindicated, until reaction towards the surface shall have been effected. Now the ordinary modes of exciting such action—blisters, sinapisms, ammoniacal ointment—fail; the skin is completely insensible to these irritant agents. This M. Rollet had learned by his own experience, as well as by the utter failure of such means as employed by others in a similar conjuncture. He determined to try still more energetic means of exciting the skin—none other than the application of the actual cautery along the spine. While applying this, sinapisms are placed on the feet, two large surfaces vesicated on the thighs with ammoniacal ointment, if there be room at the nucha some blood is drawn there by cupping, and a purgative enema administered.

The scarifications excite no manifestations of sensibility on the part of the patients; the first applications of the hot irons are similarly without effect, and it is only towards the fourth or fifth that slight muscular movements, or, in some instances, cries on the part of the patient give evidence that he feels. All of them immediately relapsed into their previous comatose state, no matter what slight demonstration of suffering they may have made. An hour or two after the cauterization, reaction commences; when it is established, general and local bleeding must be had recourse to, to an amount proportional to the strength of the individual, and associated with the other means of treatment already enumerated. Numerous subsidiary points are considered with due care by the author: for these, as well as for the answers adduced in reply to several objections which have been urged against the method of treatment generally, we must refer to the original.

The numerical results of all the cases treated are as follows:

Of 14 cases of meningitis, not one terminated fatally.
Of 14 cases of encephalo-meningitis,

4 treated on the old plan	4 *died,*	0 *recovered.*
10 treated on the author's plan . . .	4	6

Of the 4 who died of these latter 10 patients, 2 were so far advanced when admitted as to be beyond the reach of influence by any treatment whatsoever; another fell a victim to a relapse, having previously appeared in a favorable way to recovery.

Dr. H. Parrot communicates a *History of the epidemic of miliary sweating-sickness, which ravaged the department of Dordogne in* 1841. This is an elaborate and very admirable history of an epidemic of this disease, which affected one seventh of the population of the districts in which it appeared, and destroyed the lives of one thirteenth of those seized. The admirable topographical descriptions of the regions where the disease prevails, and the accurate investigation of its causes, are particularly worthy of attention. But the paper sins, like almost all its fellows, by its unconscionable prolixity, and is not of sufficient interest for the general medical reader to justify us in devoting any space to its analysis.

M. Mélier insists, in an essay on *Intermittent affections, in which the period of intermission is short,* on the importance of quinine as a remedial agent. Thus, in encephalic irritations of infants, with convulsions, in severe cases of hiccup, (so severe, that the author thought they must—as has been the fact in some instances on record—prove fatal, unless speedily relieved;) in uterine neuralgic pains, in neuralgia of various nerves, he has prescribed sulphate of quinine with almost magical success.

The fundamental point in the paper is the author's position, that the quotidian, the tertian, the quartan, &c. are not the only types of *intermittent* diseases, but that there are others to be regarded as of the same category, wherein the period of intermission is extremely short. In all such affections quinine is absolutely indicated. The paper is well worth perusal.

A learned, ingenious, and elaborate essay on the *Seat, intricate nature, symptoms, and diagnosis of hypochondria* follows, from the pen of M. F. Michéa. We can only, however, refer to it, as deserving the study of persons interested in the subject of which it treats.

M. Prus furnishes a memoir on *Pulmonary emphysema, considered as a cause of death.* The author commences by a brief summary of the position established by some of the chief writers on emphysema, closing this survey by an affirmation, that numerous points remain still to be elucidated in the history of this morbid state. The principal questions which he conceives require examination are: What is the disease to which the name of pulmonary emphysema is given? Does pulmonary emphysema consist in the dilatation of the pulmonary vesicles, or in distension of the intervesicular cellular tissue with air? Is asthma a distinct disease from pulmonary emphysema? What are the causes of pulmonary emphysema? Have the modes of prognosis and consequences of this morbid state been sufficiently studied? What should be our prognosis in cases of pulmonary emphysema? Is the affection susceptible of cure? May it of itself cause death? May it determine disease of the heart, more particularly of the right ventricle? What are the affections of which it increases the danger? What has been the influence of the study of pulmonary emphysema on the treatment of asthma?

We have enumerated these queries because we agree with their author in considering that the points they refer to require, every one of them, additional investigation. M. Prus limits himself, in the paper before us, to the subject of the prognosis of the affection, incidentally examining the question of the intimate seat of the disease.

The great object of this writer is to show that Laennec, Louis, and others, who regard uncomplicated pulmonary emphysema as an affection which does not directly destroy the patient's existence, are seriously in error. In M. Prus' estimation, the disease is one of great danger in a certain number of cases, while he admits multitudes are on record, and present themselves in daily practice, exhibiting on the other hand its comparatively frequent harmlessness. But this difference of opinion is more apparent than real, and arises altogether from (as we believe it to be) the serious error committed by M. Prus in regarding all cases, known symptomatically as examples of emphysema, to be constituted anatomically by effusion of air into the intervesicular and interlobular cellular tissue of the lungs. That this latter affection is occasionally a most serious one, and that it has occasionally proved the cause of sudden, and even instantaneous death, is a fact which has been known and recognized by pathologists for years. We find several examples referred to in illustration of the fact in the article Emphysema, in the 'Cyclopædia of Practical Surgery,' (vol. ii, p. 77,) by Dr. Walshe. The whole point at issue with M. Prus, (who adds eight cases of the kind just now referred to in the memoir before us,) lies then in a nutshell,—the simple question is whether in all cases of Laennec's emphysema, effusion of air into the cellular membrane is a constant existence. Now we hesitate not to affirm that M. Prus fails to demonstrate this. He simply asserts that in all cases air exists between the cells, but he by no means proves such to be the fact. And in stating the opinions of his predecessors, he commits the inaccuracy of leading his readers to suppose that they maintain dilatation of the vesicles to constitute the whole amount of morbid change existing in these cases. On the contrary, it has been taught by many of them that atrophy or hypertrophy of the walls of the cells, attended in some cases with destruction of those walls, exists at the same time. Now we should be led, by *à priori* reflection, to imagine that interlobular and some general subcutaneous emphysema would prove a frequent accompaniment of dilatation of the cells, inasmuch as it is frequently associated with rupture of their walls. But experience proves it (notwithstanding the affirmations of M. Prus to the contrary) to be singularly rare. Now the cause of this is a question of no little interest, and it appears to us to be plausibly pointed out in the following passage taken from the article by Dr. Walshe just referred to. (p. 77.) " Effusion of air into the cellular membrane is, however, a very rare consequence of an opening effected in the vesicles by the slow process of atrophy and distension. This rarity is probably owing to the following circumstances. In the majority of instances, perforations of the cells, as we have had frequent opportunities of ascertaining, open, not into the investing cellular tissues, but into the adjoining vesicles, the walls of which coalesce with their own. Secondly, the parietes of some vesicles are hypertrophous, and hence by obstructing the surrounding interstices, contribute with the natural density of the cellular membrane, to impede the circulation of any bubble of air that may escape from the cells; while freedom of circulation is the more necessary for the productions of subcutaneous emphysema, because vesicular emphysema is commonly most highly developed at a considerable distance from the root of the lung, namely, at its anterior border."

Experiments and observations on the poisonous properties of disulphate of quinine. By M. F. MELIER. This paper, to which the author's apprehension, that the dangerously large doses of quinine recently recommended in the treatment of acute rheumatism by M. Briquet, might be generally adopted, has given rise, is full of practical interest. M. Briquet alleged, be it remembered in the first place, that he had cured acute articular rheumatism, accompanied with violent pain, swelling, redness, fever, &c., in two or three days with disulphate of quinine, administered to the amount of six grammes [about a drachm and a half] daily. Whether this system of treatment does or does not cut short, with very surprising rapidity, many cases of rheumatism, (and indeed we may admit that it actually has done so,) is not the present question ; but that it is a most dangerous mode of treatment is rendered obvious by the simple statement that it killed a fair number of rheumatic patients during the short period of its vogue at Paris.

First, as respects the experiments of M. Mélier on dogs, the general result is that the salt appears to prove fatal through its influence on the blood. It is more rapidly fatal, and in smaller doses if given dissolved in sulphuric acid than in powder, and when the stomach is empty than after taking food.

In the human subject the ill effects distinctly observed to follow the immoderate use of quinine have been, death ; delirium and coma ; pneumonic symptoms ; hematuria ; amaurosis ; deafness ; obstinate gastralgia ; diarrhea ; epileptiform phenomena; paralysis. All these conditions are illustrated by reference to cases ; and a most melancholy and remarkable one added, in which a medical practitioner, blindly confiding in the curative powers of the drug, ended in poisoning his wife with enormous doses of it, and very nearly crowned his folly by destroying his own life in the same way ; he remained for a long time both blind and deaf after his recovery.

ART. XIV.

1. *Contributions to Proleptics.* By THOMAS LAYCOCK, M.D., Physician to the York Dispensary, &c. (*Lancet*, vols. i-ii, 1842-3.)

2. *Untersuchungen über periodische Vorgänge im gesunden und kranken Organismus des Menschen.* Von GEORG SCHWEIG.

Inquiries into the Periodic Occurrences in the Healthy and Morbid Organism of Man. By GEORGE SCHWEIG.—*Karlsruhe*, 1843. With Five lithographed Tables. 8vo, pp. 166.

3. *Instructions pour l'Observation des Phénomènes périodiques.* Par M. QUETELET, &c. &c. (*Bulletin de l'Académie Royale de Bruxelles*, No. i, tom. ix, 1842.)

Instructions for the Observation of Periodic Phenomena. By M. QUETELET.

4. *Instructions pour l'Observation des Phénomènes périodiques de l'Homme.* Par M. SCHWANN, Correspondant de l'Académie Royale de Bruxelles. (*Bulletin de l'Académie*, &c. No. vii, tom. ix.)

Instructions for the Observation of the Periodic Phenomena of Man. By M. SCHWANN. (*Bulletins of the Royal Academy of Brussels*, Nos. i and vii, vol. ix.)

THE principal characteristics of the age in which we live are the discovery and development of new truths and the revival of old. The humoral pathology is coming again into vogue, with magnetizing wizards, gothic architecture, railways, steam machinery, illuminated prayer-books, decorated domestic architecture, ceremonial worship, and the conversion of substances into each other, and, of course, of the base metals into gold. Parr's pills are the elixir of life, Liebig, Schultz, and Co. are Rosycrucians, and the Pope hopes to be lord of Christendom once more. In Dr. Laycock's 'Contributions to Proleptics,' (that the movement of the age may be complete,) we have an avowed attempt to re-establish the ancient practice of predicting events, fossilized, to use his words, in the quackery of judicial astrologers, nativity-casters, &c.; not by means, however, of the mysterious agencies of occult science, but simply by a philosophical examination of the order in which phenomena arise and succeed each other. Dr. Laycock would have the law of sequence so investigated that we should be able to anticipate events, national and individual, just as we anticipate day and night, the storms of the equinoxes, the rising and setting of the sun, moon, and stars, the ebb and flow of the tide. He wishes proleptics to be understood as the art and science of predicting, deriving the term from the compound Greek verb prolambano, *anticipo*, I seize beforehand, anticipate. In the contributions before us he endeavours to develop the laws of recurrence of phenomena, drawing his facts from astronomy, meteorology, natural history, physiology, and pathology. We shall trace his views rather according to the order of his periods than of his "contributions."

The first period or "basic unit." This comprises a lunar or barometric day of twelve hours, being marked by one maximum and one minimum variation of the barometer. The subjoined table constitutes a condensed view of some of the general facts from which the definition of this period is estimated; we would premise, however, that the proofs of each are given in detail.

"Table of the Meteorological and Physiological Events occurring at the Barometric Hours, during a Solar Day of Twenty-four Hours.

Four to five o'clock a.m.

Barometer at its minimum height.
Minimum of electric tension, nearly.
Intermediate minimum variation east of magnetic needle.
Minimum of temperature.
Hour at which several genera of flowers bloom.
Hour when certain species of moth escape from the chrysalis.
Minimum consumption of oxygen.
Outset of cholera, epidemic diarrhea, egyptian ophthalmia, and quotidian ague.
Period of increased excitement of the insane commences.
Hour of alleviation of symptoms and of sleep in hectic and infantile fever.

Four to five o'clock p.m.

Barometer at its minimum height.
Minimum of electric tension.
Minimum variation east of magnetic needle.
Certain moths escape from the chrysalis.
Termination of a paroxysm of quotidian ague.
Exacerbation of fevers, accession of hectic fever.
Period of increased excitement in the insane begins.

Eight to ten o'clock a.m.

Barometer at its maximum height.
Maximum of electric tension.
Maximum variation east of magnetic needle.
Maximum excitability of the circulation.
Maximum of muscular power.
Period of increased excitement in the insane ends.

Eight to ten o'clock p.m.

Barometer at its maximum height.
Maximum of electric tension.
Maximum variation east of magnetic needle.
Meteoric lightning and thunder storms appear.
Certain insects escape from the chrysalis.
Consumption of oxygen at its minimum.
Minimum of muscular power.
Minimum excitability of the circulation.
Hour of natural sleep.
Period of increased excitement in the insane ends.
Paroxysm of a quartan ends." (Lancet, vol. i, 1842-3. p. 931.)

The period thus defined constitutes the primary or basic unit; doubled as in the preceding table, it forms the solar day or quotidian period; quadrupled, the tertian period, &c.

The period of seven days. This is made up of two weeks of lunar days, a lunar week constituting the quartan period. This hebdomadal or heptal cycle, according to Dr. Laycock's views, governs, either in its multiple or submultiple, an immense number of phenomena in animal life. The phases of development in insects appear to present the most uniform examples of its influence. In these Dr. Laycock makes four principal periods; 1, the hatching of the ova; 2, the caterpillar or larva state, and the moults which take place at that stage of development; 3, the pupa or chrysalis period; 4, the imago state or puberty.

The ova are hatched in periods varying considerably as to length. The shortest is a lunar week, or three days and a half, as in the wasp, common bee, and ichneumon; in the *cecidomia tritici*, it is two lunar weeks, in the black caterpillar and gooseberry grub, *tenthredo capræa*, three lunar weeks. The larva state rarely occupies less than two or more than twenty-four lunar weeks, and the moults of that state have usually an interval of two lunar weeks.

> "The period spent in the pupa state is the most in accordance with the general law of limitation by weeks; in fact, the more exact the observations are as to the length of this period, the more confirmatory are they of the general rule: for example, Mr. Denny had three larvæ of the *sphinx atropos*, which went into the earth on August 22d, 24th, and September 2d, respectively. They appeared as perfect moths on October 16th, 18th, and 27th; or in each case, in exactly eight weeks." (Lancet, vol. i, 1842-3, p. 125.)

The sexual functions of the adult insect (the imago state) exhibit the agency of the same law. Thus twenty or twenty-one days after the queen bee has begun to lay the eggs of drones, the bees begin to construct royal cells. If her impregnation be retarded beyond the twenty-first (Huber), or the twenty-eighth day (Kirby and Spence), of her whole existence, she lays male eggs only, showing then no jealousy of the young queens. Some insects attain puberty immediately after leaving the puparium; others occupy a definite number of weeks in growing, especially the coleopteræ, arachnida, and crustacea. Thus, the newly-disclosed imago of the *cetonia aurata* remains a fortnight under the earth; and that of the *lucanus cervus* not less than three weeks. The common cyclops is at first nearly spherical, and provided with no more than two antennæ, and four short feet. On the 14th day, a small projection appears on the hinder part of the body; on the 22d it acquires a third pair of extremities; and on the 28th it moults.

The periods of incubation or of development of the ova of fishes have not been closely observed, with the single exception of those of the salmon. From Dr. Knox's industrious zeal, we learn that the ova of that fish are hatched in exactly twenty weeks, or 140 days. These periods in birds are much better known to us; they are all regulated by this "heptal" law. The eggs of small birds, as fly-catchers, sparrows, &c., are hatched in two weeks; of gallinaceous birds,—the common fowl, pheasant, grouse, &c.—in three weeks; of the duck tribe in four weeks, of swans in six weeks.

The incubation of the ovum of mammals (the period of utero-gestation) continues also a definite number of weeks. Dr. Laycock has published a list of 129 species of birds and mammals, which proves (as he maintains) the operation of the general law, remembering how difficult it is to calculate the period of utero-gestation to a day.* It is not, however, the phases of development only that exhibit the law of periodicity just laid down; there are various other changes and functions amenable to it. The moults of adult insects, as the arachnida, myriapoda, and crustacea, the exuviation of serpents, and the renewal of the dermoid appendages in birds and mammals, are all regulated by it more or less. And so also are minor processes. The ring-pigeon not only sits fourteen days, but lays eggs previously to sitting for fourteen days. Birds of the goose and duck kind lay eggs in the wild state at tertian intervals, that is to say, seven in fourteen days, or one every other day. The goldfinch builds its nest in three days, and it is left unoccupied for four, the first egg not being laid until the 7th day from the beginning.

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* Treatise on the Nervous Diseases of Women, p. 48, seq.

From the preceding statements, our readers will be able to infer the extensive class of phenomena to which Dr. Laycock applies his law. Dr. Laycock insists that man, in his reproduction and development, is no exception to this law. He looks upon menstruation as analogous to the heat of mammals, and the period of oviposition in birds; but maintains that although in the human female the intervals between each oviposition is four weeks, the "basic unit" is one week. It is certain that in many, the period is three weeks, in others five weeks, while some have a white and red discharge alternately every two weeks. The whole period of utero-gestation (incubation) is ten menstrual periods, or forty weeks.

The chain of demonstration thus carried up link by link, would be manifestly incomplete if the periods of disease did not correspond with, and corroborate those of health. Aware of this, Dr. Laycock proceeds to examine those periodic phenomena observed by physicians from time immemorial, during the course of fevers, both remittent and intermittent. Dr. Laycock takes up the consideration of fevers generally; he shows that the periods of exanthematous fevers are for the most part heptal, that the 4th, 7th, 14th, 17th are critical days in smallpox; that the exanthematous typhus is a twenty-one day fever, that shingles run their course in fourteen days, and that pemphigus, rubeola, scarlatina, &c., are all amenable to the general law.

Of course no one will deny the periodicity of intermittents; Dr. Laycock argues that the remittent and malignant fevers are identical in origin with the former, and therefore subject to the same general law.

> "It has been found all over the world that intermittent, remittent, and malignant fevers pass one into another. In Spain and in India, in the West Indies, and in Sicily, in Africa, and in America, this fact has been substantiated by such numerous and competent observers as to set the question at rest. If the febrific poison be concentrated, and the temperature high, the fever is intense in a few hours or days, often indeed without a remission. But if the malaria be less concentrated, and the temperature comparatively low, as in Spain for example, at the beginning of summer and the end of autumn; or if the locality be an elevated one within the tropics, then the fever will be less malignant; probably a regular remittent or an intermittent. . . . . In temperate latitudes the remissions of a malignant remittent, become more complete towards the end of summer, until at last we have quotidian agues; later on, these assume the tertian type; and far into autumn, quartans take the place of tertians." (Lancet, vol. i, 1842-3, p. 126.)

Dr. Laycock compares the order of the critical days of fevers, with the paroxysms of intermittents, as in the following table:

"The critical days according to Hippocrates are	1.4.7.9.11.14.17.20 or 21.
"The days on which paroxyms of a tertian will take place are	1.3.5.7.9.11.13.15.17.19.21.
"Ditto of a quartan . . .	1.4.7.10.13.16.19.

> "Supposing a remittent fever has the quartan type, and that after the fourth paroxysm an intermission takes place, on what day would the amendment appear? Clearly on the eleventh; and the change of symptoms on that day would be judicatory of a mild paroxysm on the thirteenth, and final amendment on the fourteenth. If the intermission occurred after the fifth paroxysm, then the fourteenth would be the critical day; if at the sixth, the seventeenth, if at the seventh, the fever ends on the twentieth day..... Whatever type a fever may exhibit, there will be a paroxysm on the seventh day, and consequently this day should be distinguished by an unusual fatality or number of crises. For analogous reasons, the fourteenth will be remarkable as a day of amendment." .... (Ibid.)

To strengthen his case still more, by proving that the critical days are not confined to malarious or exanthematous fevers, Dr. Laycock analyses the order in which the paroxysms recur in a fit of the gout, and shows it to have a tertian type, terminating in fourteen days, and after seven paroxysms. However, just as a tertian ague may change into a quotidian or quartan or any other type, so may the gouty paroxysm. Dr. Laycock has seen it as a septiman. Indiscretion in diet or treatment may also double or treble the periods, or render them irregular, just as with agues.

Dr. Laycock quotes other pathological phenomena as illustrating the law. A case of tertian and quartan lunacy communicated to him by Dr. Thurnam, of the York Retreat, shows very strikingly the involution of the minor into the major periods, and the influence of the law on the nervous system; other neuroses, as epilepsy, neuralgia, &c., are referred to in detail. The period of incubation of febrific poisons is never more than a lunar month—a curious fact, which Dr. Laycock explains by the law in question. He adopts the hypothesis of a regular sequence of critical days in health continually going on in the system, and predisposing to the outbreak of disease. This sequence begins with conception. In the embryo it is marked by the successive stages of development in the teeth; in infantile and pubescent life by the successive stages of dentition. If therefore two persons were conceived in the same hour, and developed under the same circumstances, as might happen in the case of twins, the periods of the two would be coincident, and the phenomena of health and disease would be exhibited in both at the same time. An interesting example of this kind in twin-brothers, answering in all respects to the theoretical details above given, is quoted. We observed lately that a writer on mesmerism in the 'Dublin University Magazine,' noticing instances of this kind, attributes the singular coincidences observed to animal magnetism. The case of the twin-brothers Laustand, sick-nurses in an hospital at Bourdeaux, is one of those referred to. These two were always ill at the same time, and became affected with cataract together. Dr. Laycock explains on the same theory the curious coincidences of disease and death in members of the same family, children by the same mother. The time of conception may so far correspond in each, that the sequences of their periods may have a relation to each other, because they will have a common relation to those of the mother; these latter being defined by menstruation, or the escape of ova from the ovaria.

The causes of vital periods. According to Dr. Laycock's views, there may be three classes of periods, arranged according to their causes. The first he terms esoteric, from *εσω*, intus, within; these depend upon the law of sequence just referred to, and belong essentially to the individual, or are grafted upon the organism by the operation of exoteric or external periodic agencies. When the latter cease to act, the sequence of periods still goes on in virtue of "the habit of recurrence" of vital phenomena displayed by animals. The innate and congenital sequence is the most recondite, and as the number seven has so striking a part in the measurement of them, Dr. Laycock refers specially to the ancient doctrine of the harmony of numbers, and to astrology. The operation of the number seven is singularly prominent in all the literature, both of the ancient and modern Asiatics. What was really meant by the harmony of numbers and the perfection of the number seven? Pythagoras defined harmony to be the relation or *just proportion* of all parts, or *the natural order of all things.* Numbers or arithmetic formed the basis of all his science, and by these he measured the proportion and established the natural order of all things. He was a true inductive philosopher, as is proved by the remains of the sciences he introduced into Europe. The works of Boëtius, the last of the Roman writers, contain the Pythagorean philosophy of arithmetic, geometry, and music. The latter is considered altogether on mathematical principles. Sound from instruments can only be produced by the percussion of the air, and there can be no percussion without motion. Music is divided into three kinds, mundane, human, and instrumental. The first is caused by the swift motion of the machine of heaven in the firmament, for it is no proof that the planets do not give out sounds as they move, that we do not hear them,* &c. The second is seen in the mixing or union of the incorporeal vivacity of reason with body. The laws of the third kind Pythagoras discovered by experiments. He investigated the tension of sounds, as brought out by hammers of different weights, or from musical strings of varying length and thickness, and deduced from his investigations the numerical laws of chords. It is probably to the second kind of music that arithmetic had its physiological application. Then and up to a recent period, arithmetic had its prime and composite numbers, its perfect, abundant and deficient, its odd and even, its polygonal, prismatic, and oblong numbers. The perfect numbers of which Dr. Harris says, there are but ten between a unit and a billion, were formed thus: If the geometrical progression 1, 2, 4, 8, &c. be carried on until the sum be a prime number, and that sum be multiplied by the last term of the series, the product is a perfect number. Thus $1 + 2 = 3$, a prime, and $2 \times 3 = 6$, a perfect number. The number 3 was used principally in mythology and astrology, and has more curious properties than any other. The next perfect number was that used so extensively in physiology and pathology. $1 + 2 + 4 = 7$, a prime number, and $7 \times 4 = 28$, a perfect number; for its aliquot parts are $1 + 2 + 4 + 7 + 14 = 28$. Now let us refer to Dr. Laycock's summary of his minor periods. $1 =$ the day of twelve hours; $2 =$ the solar or quotidian period; $4 =$ the tertian period; $1 + 2 + 4 = 7$, a lunar week and the quartan period.

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* This magnificent philosophical hypothesis, that the planets, by their motions, must strike the ethereal medium around them, and so produce sounds, and that the sounds must be musical, because the motions vary in force and swiftness according to fixed laws, has been often used as a beautiful poetic idea, without any knowledge or at least acknowledgment of its author:

> "Sit, Jessica: look how the floor of heaven
> Is thick inlaid with patines of bright gold;
> There's not the smallest orb, which thou beholdst
> But in his motion, like an angel, sings,
> Still quiring to the young-eyed cherubims:
> Such harmony is in immortal souls;
> But whilst this muddy vesture of decay
> Doth grossly close it in, we cannot hear it." *Merchant of Venice.*

To take the critical days for another comparison.

Let $s =$ the solar day, or quotidian period,
$2s =$ the tertian,
$4s =$ the quartan,

and $1s + 2s + 4s =$ the 7th day or solar week, constituting the prime of a perfect number, and $7s \times 4s = 28s$, the menstrual period. Or to put the series of periods into the Pythagorean series of a perfect number,

$1s + 2s + 4s + 7s + 14s = 28s$, the menstrual period.

We might add illustrations of this kind; the above, however, will show the true bearing of the doctrine of numbers applied to vital acts. Physiology and pathology were manifestly brought under the same numerical laws as the other sciences more purely mathematical. There is harmony of numbers in all nature; in the force of gravity, in the planetary movements, in the laws of heat, light, electricity, and chemical affinity, in the forms of animals and plants, in the perceptions of the mind. The direction indeed of modern natural and physical science is towards a generalization which shall express the fundamental laws of all, by one simple numerical ratio. We would refer to Professor Whewell's 'Philosophy of the Inductive Sciences,' quoted by Dr. Laycock, (vol. i, p. 427,) but particularly to Mr. Hay's researches into the laws of harmonious colouring and form. From these it appears that the number seven is distinguished in the laws regulating the harmonious perception of forms, colours, and sounds, and probably of taste also, if we could analyse our sensations of this kind with mathematical accuracy. We think modern science will soon show that the mysticism of Pythagoras was mystical only to the unlettered, and that it was a system of philosophy founded on the then existing mathematics, which latter seem to have comprised more of the philosophy of numbers than our present.

So far as regards the causes of esoteric periods; the exoteric causes are principally meteorological. We have already noticed those which are diurnal; in investigating the cause of weekly and monthly periods, Dr. Laycock considers the question of lunar influence. He first gives a historical sketch of the doctrines held by the ancients and moderns, and collects numerous facts bearing directly or indirectly on the subject. The general result of his inquiries seems to favour the idea that the moon has an indirect influence on vital acts. The lunar periods had evidently some influence in originating the doctrine of septenaries. From the earliest ages they were used to measure time, as by the Babylonians and Chaldeans. The week of seven days was adopted by all nations who derived their knowledge from these. The Tartar tribes, including the Chinese, the ancient Mexicans, &c. adopted a week of five days. Mr. Cullimore explains some of the characters on the Babylonian bricks, by assuming the latter to be calendars. The characters on these bricks are seven in number, and were originally part of the symbols of the ten primary elements. The other three were the hieroglyphics of the triune deity; the first light, represented by a horn, or a single arrow-head, or a winged egg; the second represented wisdom; the third, love, intelligence, or imparted wisdom. The seven remaining symbols used for the calendar were those of the sun, moon, and five planets, in the order of the days of the week. It appears from a passage in Isaiah, that at Babylon there were monthly prognosticators—*ex-professo;* and Mr. Cullimore thinks that many of these bricks contained the meteorological predictions of this class of men. The latter had reference probably to the moon's influence. Those who are curious in matters of this kind will find more information on the subject in Dr. Laycock's papers than in any work we know of.

The transition from the meteorological influence of the moon to that of the sun is easy. We subjoin the following table exhibiting the influence of the seasons:

	Maximum.	*Minimum.*
Variations in the barometer:		
a. Near London, 1807 to 1816 .	December.	July.
b. In the Deccan . .	December or January.	July.
Variation in the hygrometer . .	January.	July.
„ thermometer . .	July or August.	January.
Amount of evaporation . . .	July.	January.
Number of births (Belgium) . .	February.	July.
„ deaths (Belgium) . .	January.	July.
Cases of Insanity . . .	June or July.	December or January.
„ of Suicide	Summer.	Winter.
Crimes against persons . .	June.	January.
„ property . .	December.	July.

We also extract the following passage as being worthy attention:

> "Correct estimates of the seasonal influence in each year become of much higher importance, when it is remembered that *it is from data of this kind the difficult problem of the periodic return of epidemics must be solved.* The difference between the *medical* constitution of each year is as great as the difference in the meteorological constitution, demonstrated by Mr. Howard .It becomes necessary, therefore, to obtain accurate statistical data of each year for a series of years, (at least for eighteen,) so constructed that the epidemical constitution may be compared with the meteorological." (Lancet, vol. ii, 1842-3, p. 829.)

Dr. Laycock discusses the special physiological and pathological changes induced by the seasons, but maintains that as there is a "habit of recurrence" in daily periods, so there is in annual periods; and quotes examples of the exact punctuality of migrant animals, fishes, birds, and mammals, in proof.

In discussing the septenary periods, Dr. Laycock first gives the literary history, then the actually existing knowledge we have. He reviews the whole life of man, but particularly that portion between birth and middle age. The critical days of infants, the first and second dentition, the evolution of the sexual organs, and the outbreak of hereditary disease at certain periods, are all considered in detail. The literary history displays the same use of the number seven as we have noticed in discussing the minor periods.

Periodic cycles. Under this head we have a dissertation on periods of years. The subject is full of interest to the physiologist, naturalist, and even statesman.

"Periods of years may be determined in various ways. The science of astronomy, as we have just seen, marks them; and so also do changes in the electricity and magnetism of the earth and air. Thus, comets, earthquakes, meteors, violent thunder-storms, hurricanes, immense floods, thick fogs, great heat and cold, &c., may be instrumental in determining these periods. The latter phenomena may react upon animated nature, and induce famine, fatal and wide-spreading epidemics, the sudden migration, or extraordinary multiplication, or mortality of animals, as locusts, mice, &c. Famine and pestilence may react upon society at large, and lead to civil strife, revolutions, wars, and the migrations of nations." (Lancet, vol. ii, 1842-3, p. 430.)

The periods, according to Dr. Laycock, may possibly be astrological; on this point a curious fact is stated to the effect, that the violent storm in October last was predicted astrologically for some time previously.

Mr. Howard has discovered a lunar cycle of seventeen or eighteen years by a digest of the barometric observations made daily at Somerset House. He found that the barometric mean is depressed on an average of years by the moon's position in south declination. This depression is gradual, and commences with the moon in full north declination. The effects of this influence are seen through a period of eighteen years. The mean weight of the atmosphere increases through the fore part of this period, and, having kept for a year at the maximum thus obtained, decreases through the remaining years to a minimum, about which there is some fluctuation before the mean begins to rise again. The revolution of the lunar nodes and apogee takes place within this cycle of eighteen years, and Toaldo thought the half of it, or nine years, would explain the recurrence of plagues, &c. The Rev. W. B. Clarke, having collated a large number of historical facts bearing on the subject, came to the certain conclusion that there is a positive periodicity in the derangement of the earth and atmosphere. So far as regards the measure of the periods of recurrence, his views resemble those of Mr. Howard; but he includes the phenomena of earthquakes, famine, pestilence, extraordinary irruptions of animals, &c. in his law. It is rather a curious coincidence that the interval between two epochs distinguished for the phenomena just mentioned, namely 1348-9 and 1833, is filled by exactly twenty-seven of Mr. Howard's lunar cycles. Indeed, there are ample reasons why this period should be recognized at once as established.

Dr. Laycock traces the history of cycles of years in like manner as he has traced the history of smaller periods. He shows that the ancients carried out this branch of proleptics to a great extent. They adopted an extremely general and hypothetical law of periodicity, by virtue of which all mundane phenomena were included within one great cycle or annus magnus of upwards of 300,000 years. At the end of this period everything (they said) in this world began over again. The whole assemblage of celestial phenomena, which are regarded as the influential causes of all changes in the sublunary world, being restored to the same initial order, and proceeding in the same catenation as before, the whole series of events that depend upon them follow in their former connexion of place and time. The same individuals are doomed to be born again, the same cities builded and destroyed, the same arts invented. If this doctrine be true, we have been manifestly in the wrong in denying the claim of a notorious individual to originality on this very point. No doubt the law is his, and was discovered by him above 300,000 years ago. But a sad fatality will attend him and us, for as many years hence he will be at his present follies, and we shall be crowning him with foolscap:

> "Alter erit tunc Tiphys ....
> .... erunt etiam altera bella."

II. Schweig's researches are intended to illustrate the periodic movements exhibited in the excretion of uric acid, in the uterine functions, in the number of deaths from various diseases, and in the recurrence of epileptic attacks.

The body of an individual is continually undergoing a change in its composition, new particles being deposited as the old are taken up. The chemistry of this change has been investigated by recent experimenters in various modes; and we have presented to our readers the general results of these inquiries. We refer to our review of Schultz's publication as more particularly discussing the relations of these changes to time. Schweig terms the periods within which they occur tropical periods, from the Greek τρεφω, nutrio, and, after discussing the question at length, adopts the quantity of uric acid excreted as the exponent of the intensity of the changes. He then details his investigations and their results. The following is the method he adopted:

> "The hour when the urine was passed was exactly noted, the quantity of the latter measured, and its specific gravity ascertained while yet warm. After the mucus has been removed by filtration, fifteen drops, or if the urine was concentrated twenty drops, of English sulphuric acid, previously re-distilled, were added to every fifty grains of urine, and the whole mixed carefully so as not to wet the sides of the glass vessel. The mixture was then covered up, and was allowed to remain for from thirty-six to forty-eight hours in a cool place, then filtered, ten more drops of sulphuric acid added, and the mixture again set aside for twenty-four hours, so that any remaining uric acid might be precipitated. This part of the experiment is in general really unnecessary, but it is better to do it, so that there can be no doubt that all is precipitated. The crystals remaining on the filter must be washed in distilled water, dried, weighed, and the result duly entered in the journal." (pp. 21-2.)

Schweig finds that the first filtration is absolutely necessary to remove the mucus and epithelial cells; it both facilitates the precipitation of the uric acid, and renders the results accurate. The glass of the vessel should be thin and not scratched, for in the latter case, the crystals of uric acid attach themselves to the scratches. Even if a mark be made on the glass, the surface not being roughened, the crystals will collect upon it. These and other practical hints are given.

The first inquiry is for the purpose of discovering the influence of diurnal periods on the excretion of uric acid. Schweig made 1520 observations on his own urinary secretion in the winter of 1841 and 1842. His life was regular; his three meals a-day were between six and seven in the morning, twelve and two, and between six and seven in the evening. The following table gives the result:

				Gramme.					*Gramme.*
"Night	12	to 1	o'clock . .	——	Morning	6	to 7	o'clock .	0·0147
,,	1	2	,, . .	——	,,	7	8	,, .	0·0183
,,	2	3	,, . .	——	,,	8	9	,, .	0·0170
,,	3	4	,, . .	0·0080	,,	9	10	,, .	0·0136
,,	4	5	,, . .	0·0130	,,	10	11	,, .	0·0120
,,	5	6	,, . .	0·0167	,,	11	12	,, .	0·0110."

"If the time be considered the ordinate, and the above numbers the abscissa, two curves are formed, a greater and a less. The latter begins at midnight, reaches its maximum between eight and nine in the morning, and ends at noon, where the larger curve begins, reaching its maximum between four and five o'clock p.m., and ending at midnight." (p. 35.)

Each of these curves has a deflection; in the larger it reaches its maximum about six or seven o'clock in the evening, in the smaller about seven in the morning. Schweig refers these to the influence of the rising and setting sun; the quantity decreasing as the sun sets, and increasing at his rising. This table can be compared with Dr. Laycock's. It will be remembered that the observations were made in winter. The average quantity of uric acid excreted per hour was 0·016 gramme. From this, it varied between nothing and 0·053 gramme.

Schweig inquired into the mortality at various hours of the day. The following table shows the result:

Morning.

	4—5	5—6	6—7	7—8	8—9
Winter . .	95	109	116	115	113
Summer . .	120	119	99	100	117

Evening.

	4—5	5—6	6—7	7—8	8—9	9—10
Winter . .	103	114	81	77	105	111
Summer . .	111	119	132	82	86	110

Comparing these data with the preceding, Schweig says they exhibit an unmistakable similarity.

The following tables show the hour of the day most influential in determining death from consumption:

"*Deaths in Berlin in 1836 from Phthisis.*

Midnight to 6 o'clock a.m. . .	165	Ratio per 1000 . .	220
6 o'clock a.m. to noon . .	220	,, . .	294
Noon to 6 o'clock p.m . .	197	,, . .	263
6 o'clock p.m. to midnight .	167	,, . .	223
Total	749 cases	. .	1000

Deaths from Phthisis in Carlsruhe in 11 Years.

Midnight to 6 o'clock a.m. . .	187	Ratio per 1000 . .	239
6 o'clock a.m. to noon . .	218	,, . .	280
Noon to 6 o'clock p.m. . .	195	,, . .	260
6 o'clock p.m to midnight .	180	,, . .	231
Total	780 cases	. .	1000."

The following shows the hourly deaths of the preceding:

"Night	12 to	1	21	Noon	12 to	1	42
,,	1	2	27	,,	1	2	31
,,	2	3	34	,,	2	3	29
,,	3	4	32	,,	3	4	27
,,	4	5	43	,,	4	5	30
,,	5	6	30	,,	5	6	36
Morning	6	7	44	Evening	6	7	32
,,	7	8	29	,,	7	8	20
,,		9	52	,,	8	9	22
,,	9	10	37	,,	9	10	31
,,	10	11	30	,,	10	11	33
,,	11	12	26	,,	11	12	42."

The next table is from Guerry's paper on suicide, in the 'Annales d'Hygiène publique,' Jan. 1831.

“ *Hours at which Suicides destroyed themselves.*

Midnight	12 to	2	. 77	Noon	12 to	2	. 32
„	2	4	. 45	„	2	4	. 84
„	4	6	. 58	„	4	6	. 104
Morning	6	8	. 135	Evening	6	8	. 77
„	8	10	. 110	„	8	10	. 84
„	10	12	. 123	„	10	12	. 71
			548				452.”

The minimum is at noon, the maximum from six to eight o'clock a. m. Schweig observes that the influence of diurnal periods in menstruation was first investigated by Metzler. (Versuch einer mediz. Topographie der Stadt Sigmaringen; Freiburg, 1822, p. 365.) At his request, seven unmarried females took note of the day and hour they menstruated. The following is the tabular result:

“About	1	o'clock a.m.,	—	times.	About	1	o'clock p.m.,	2	times.
„	2	„	3	„	„	2	„	7	„
„	3	„	9	„	„	3	„	11	„
„	4	„	11	„	„	4	„	6	„
„	5	„	4	„	„	5	„	6	„
„	6	„	8	„	„	6	„	6	„
„	7	„	1	„	„	7	„	2	„
„	8	„	1	„	„	8	„	5	„
„	9	„	—	„	„	9	„	3	„
„	10	„	—	„	„	10	„	1	„
„	11	„	—	„	„	11	„	—	„
„	12	„	—	„	„	12	„	—	„
		Altogether 37 cases, or 44 per cent.					Altogether 49 cases, or 56 per cent.”		

Schweig thus tabulates 246 instances which came to his knowledge.

“Night	12 to	1	o'clock,	1	Noon	12 to	1	o'clock,	8
„	1	2	„	—	„	1	2	„	13
„	2	3	„	4	„	2	3	„	14
„	3	4	„	13	„	3	4	„	19
„	4	5	„	15	„	4	5	„	14
„	5	6	„	13	„	5	6	„	14
Morning	6	7	„	20	Evening	6	7	„	7
„	7	8	„	8	„	7	8	„	8
„	8	9	„	17	„	8	9	„	13
„	9	10	„	14	„	9	10	„	9
„	10	11	„	8	„	10	11	„	4
„	11	12	„	7	„	11	12	„	3
				120					126.”

Wetzler connected the maxima and minima with the fluctuations of the barometer: Schweig denies the influence of atmospheric pressure, and shows that menstruation is governed by the same law as the excretion of uric acid, and the hour of dissolution.

Schweig has three major periods, namely, of five days, six days, and seven days. The period of six days is the basis of the other two, and each day is named after the Roman numeration, primus, secundus, tertius, &c. In the five-day period, the third and fourth are made into one day, which he terms *contractus*, and in the seven-day period, a day is interposed between these two, called *interpositus*. The day begins with noon, and not with midnight, for reasons set forth at length. The physiological and pathological value of each of these days are also stated, and with great ingenuity with reference to the excretion of urea, menstruation, fits of epilepsy, the attack of puerperal fever, measles, &c., and the deaths from pneumonia, phthisis, scrofula, &c. Schweig lays down several laws with reference to the pathological conditions mentioned. The urinary secretion exhibits two curves in the cycle of six days, a larger and a smaller. These curves Schweig traces in connexion with the motions of the moon; showing the differences of new and full moon, of apogee and perigee. We extract the following table of 2281 births at the lunar phases, and along with it the statistics of 1403 births in York, by Dr. Laycock; the latter comprise the number happening on three days only at each phase, the day before and after, and the day itself:

Hamburgh	New moon to first quarter	520
	First quarter to full moon	557
	Full moon to last quarter	594
	Last quarter to new	610

York	New moon	151
	First quarter	129
	Full moon	131
	Last quarter	154

The moon's motion is quicker when near perigee than apogee; Schweig found the mortality at Carlsruhe 6 per cent. greater before and after perigee than before and after apogee; the quantity of uric acid secreted is exactly the reverse. From an estimate of numerous facts, Schweig concludes that under certain circumstances, when the intensity of nutrition is at a maximum, the number of deaths is at a minimum, and *vice versâ*.

Some statistical details respecting the menstrual periods are given. The results of 200 menstruations in thirty-four individuals, show an average of 27·8 days, the maximum number in the table being 28 days; in one person in whom menstruation was irregular, sixty-five returns averaged 25·9 days. The relations of menstruation to lunar periods are investigated, and rules deducted; but the instances are too few for any useful purpose of this kind.

The periods of an epileptic are next investigated, the dates of each attack being recorded from January 5th, 1833, to November 7th, 1841. On analysing these, we find a singular complexity of heptal periods, varying in length from 7 days to 56.

In expressing a general opinion of the book under review, we cannot deny its author the credit of great ingenuity and industry. Some of the views are, we think, fanciful, and will not be substantiated; and the complexities of the subject are not sufficiently considered. We think he will derive some valuable information from a perusal of Dr. Laycock's papers.

III. The instructions of M. Quetelet refer, 1, to meteorology and to the physical changes in our globe; 2, to vegetable life; 3, to animal life. Fully sensible of the importance of investigating periodic phenomena on an extended scale, from having himself greatly elucidated them in his 'Essay on Man,' he sought the assistance of philosophic inquirers both at home and abroad. These instructions are published with the intention of securing uniformity in the observations of widely separated observers. M. Quetelet's attention, and that of his collaborateurs, seem to be directed principally to the annual periodic changes; the diurnal ones are expressly neglected. This is done to render the observation less arduous; but we question its policy: it may probably happen that contemporaneous diurnal observations only can render the annual complete. Periodic phenomena have such an infinite variety of relations, that it is impossible even to guess at them *à priori*.

M. Quetelet very wisely dwells on the importance of unity of time and unity of place in making observations, stating that these conditions are indispensable to the deduction of general results.

In looking over M. Quetelet's plan, we were inclined to doubt whether it be sufficiently comprehensive. That part referring to meteorology and vegetable life is more complete than the remaining portion, having reference to animal life. But if we were called upon for a system, we would first recommend a digest of all the observations already made. The various periodical publications, as journals and magazines of natural history, botany, agriculture, and the transactions of learned societies, all contain an immense number of observations already made, which are useless only because isolated. If these were brought together, and tabulated, we are convinced much good would result; not only would general principles be elicited, but we should more distinctly ascertain what ought to be observed. Dr. Laycock, noticing these instructions, suggests several improvements in the third division. The nature of them may be inferred from our analysis of his papers.

The instructions of M. Schwann constitute a supplement to those of M. Quetelet. They refer, firstly, to the periodic phenomena which recur at fixed epochs: these are subdivided into the annual and diurnal, the former including the seasonal influences. M. Schwann would extend the plan of observation to births, deaths, and epidemics, and indeed to all pathological phenomena. The second class of phenomena are those which refer to the epochs in individual life; these include of course, Dr. Laycock's esoperiodic phenomena. M. Schwann would have the organs of the body, brain, lungs, liver, testicles, &c. measured and weighed at all epochs, both during fetal and extra-uterine life. He would also fix the epoch of dentition, puberty, and of involution and decline. It is manifest that in England, M. Schwann's first class of phenomena are all either registered now, or easily may be, by our national system of registration. It only remains with the Registrar-general, as we have shown elsewhere, to connect meteorological phenomena, both diurnal and annual, with movements in the population; the births, epidemics, and deaths from all or from special causes. With regard to the second class, we would observe that M. Schwann's remarks prove that he is not yet acquainted with what has been already ascertained, and consequently, that his "instructions" come with little authority, seeing that he himself wants instructing on some fundamental points.

If the laws of periodicity be considered in all their relations, their mysterious antiquity, their practical importance, their infinite extent, their connexion at once with the most immense phenomena of the universe, and the most minute, they cannot fail to attract philosophic minds to their study. On these grounds, we venture to predict that the art and science of proleptics will rapidly rise into favour. It seems to us, however, an important point that the cultivators of it should have a right bias given them at the beginning. Our Registrar-general, Mr. Chadwick, and the British Association may possibly be brought to co-operate with M. Quetelet and his Belgian friends, and at the next meeting at York, a re-union of those interested in the subject might be so managed as to lead to important practical results. We commend this hint to the parties concerned.

ART. XV.

1. *Natural History, Pathology, and Treatment of the Epidemic Fever at present prevailing in Edinburgh and other Towns.* By John Rose Cormack, M.D. F.R.S.E.—*Edinburgh*, 1843. 8vo, pp. 182.
2. *Remarks on the present Epidemic Fever.* By William Pulteney Alison, M.D. F.R.S.E., &c. (*Scottish and North of England Medical Gazette*, vol. i, pp. 1 to 4.)—*Edinburgh*, 1843.
3. *Letter on the present Epidemic of Dundee.* By J. Arrott, M.D. (*Ibid.* p. 129.)
4. *Clinical Observations on Fever.* By Professor Henderson. (*Ibid.* p. 162.)
5. *On the Presence of Urea in the Blood in the prevailing Epidemic Fever and in Typhus.* By Michael W. Taylor, Esq. (*Ibid.* p. 289 to 293.) 1844.
6. *Remarks on the Epidemic Fever in Aberdeen during the year* 1843. By A. Kilgour, M.D. (*Ibid.* p. 321.)
7. *On some of the Characters which distinguish the Fever, at present Epidemic, from Typhus Fever.* By Prof. Henderson. (*Edinburgh Medical and Surgical Journal*, vol. lix.)—*Edinburgh*, 1843.
8. *Some Account of the Epidemic Fever prevailing in Glasgow.* By David Smith, M.D. (*Ibid.* vol. lx, p. 67.)
9. *Notice of a Febrile Disorder which has prevailed at Edinburgh during the summer of* 1843. By David Craigie, M.D. (*Ibid.* vol. lx, p. 411.)
10. *Some Account of the Epidemic Remittent Fever prevalent in Glasgow in* 1843. By William Mackenzie, M.D. (*London Medical Gazette, Nov.* 24, 1843.)

No one can have devoted any degree of attention to the study of continued fevers, without arriving at a conviction that the principles upon which the different varieties have been grouped or classified are vague, vacillating, and contradictory. Without referring to the numerous subdivisions of different writers, the distinctions between the three species of Dr. Cullen's classification, generally followed in this country, *synocha, synochus*, and *typhus*, are so indefinite and ill understood, if in themselves accurate, that even our most precise systematic writers are to be found describing different degrees of the same fever, as illustrative of all the three varieties; while, with authors less precise, all the difficulties and distinctions have been evaded by describing all the varieties of fever under the common designation of typhus.

The identity of the typhus fever of this country and the typhoid of M. Louis and other continental writers was very fully discussed by us in a recent volume, (Vol. XII, p. 293-326,) and we then arrived at the conclusion, that the continued fevers of both countries were the *same species* of disease, although *different varieties* of that species; and that the French variety was occasionally, though rarely, observed in this country, and the British variety in France.

The identity of these two forms of fever has again been discussed at some length by Professor Bartlett, whose work was reviewed by us in our last Number. Dr. Bartlett recognizes only four species of fevers: "to wit, typhoid fever; typhus fever; periodical fever, in its three forms, of intermittent, bilious remittent, and congestive; and yellow fever." He recognizes no inflammatory fever, or any other form of continued fever, distinct from typhoid and typhus.

Without entering again into a subject fully examined by us in the articles referred to, we may remark briefly that there is nothing in Dr. Bartlett's book which would induce us to alter the opinion there given. We do not think the differences pointed out by Dr. Bartlett between his typhoid and typhus sufficient to warrant his conclusion, that they are distinct forms, far less that they are the only forms of continued fever. Some of the points of difference referred to by him appear to us by no means made out, such as the distinction founded upon the different periods of life at which individuals are susceptible of the attacks of the two varieties. The absence of the Peyerian affection and diarrhea in our typhus does not appear to us to be so invariable as Dr. Bartlett represents it to be; nor is the distinction founded upon the darker and dustier hue of the eruption, or its extending to the extremities a sufficient reason for considering typhus a distinct form of fever.

With reference to the absence of lesion of the Peyerian glands, the dogma (for we must regard it as such) with which the author sets out, that there are only two forms of continued fever, has been the means of leading him into error; for he is evidently compelled to include under his description of typhus, forms of fever more distinct from typhus than it is from typhoid. He refers, for example, in illustration of his descriptions, to epidemics, in which, as we shall presently see, forms of fever prevailed very different from the common typhus of this country.

Nothing, we conceive, will tend more to give precision to our definition and arrangement of the different groups or forms of continued fever, than careful study and observation of the symptoms which characterize the great epidemics that occasionally pervade extensive districts. In such an epidemic, the features of a particular disease are displayed in all their different modifications and varieties, and it can then be seen what is essential to the species, what is distinctive, and what is to be attributed to various modifying influences.

In such an epidemic visitation, for example, *of scarlatina*, we might conceive that the essential characters of that disease were made out, its varieties distinguished, and a disease which had been long regarded as a distinct affection, under the name of malignant or putrid sore throat, received its proper place beside scarlatina simplex, and scarlatina anginosa.

Such, it may be anticipated, would be the result of a minute and extended observation of the forms of fever, as presented to us in the widely-spread epidemics of successive periods. An opportunity of this kind has occurred within the last twelve months, during which a very remarkable epidemic continued fever has pervaded the country, especially, the northern parts.

The works and papers quoted at the head of this article contain descriptions and notices of this fever as it appeared in Scotland, and a very cursory inspection of them will satisfy our readers that there are more forms of fever to be met with than those admitted by Professor Bartlett, and that the present form differs widely from the typhus described by him as the common fever of Great Britain. In the description of our typhus he has included certain points, such as the shorter duration of the attack, the benefits derived from bloodletting, and the occurrence of critical sweats, which are applicable in some measure to this epidemic fever, but not applicable at all to typhus. His description may justify his reference to the epidemics of former years, in which this fever undoubtedly prevailed, but goes far, at the same time, to prove that in order to comprise continued fevers under two species, he has been compelled to comprise more than one form under the name of typhus, his description of which appears to us to be taken from a mild form of continued fever, and an aggravated form of typhoid.

Before reviewing the peculiar type of fever described as the prevailing epidemic of the last ten or twelve months, we cannot but express our conviction that the study of typhus, as seen in different localities throughout this country, and in its different modifications in the same localities, during the prevalence of a great epidemic, would more than substantiate our general conclusion regarding typhoid and typhus, before referred to. Such a review of its modifications, we believe, would leave no doubt of the existence and *specific* character of a form of fever which presents certain general characters throughout all its modifications. Of these general characters, common alike to typhus and to typhoid, the most striking we believe are to be referred to the mode of invasion and propagation, the general appearance, the cutaneous eruption, the condition of the respiratory organs, the state of the circulation, the peculiar affection of the sensorial functions, and the progress of the malady.

Of the modifications which it presents, the only ones which appear at all material, are the greater frequency of abdominal organic lesions in some localities, as contrasted with the greater development of cutaneous eruption in others; varieties which afford no grounds, in our opinion, for referring affections, in other respects perfectly analogous, to distinct forms or species of continued fever. Remarkable varieties would be met with in such an epidemic, in the degree or intensity of the symptoms, in the severity of some cases as compared with others; but the general characters of the fever would still be sufficiently distinctive, both in kind and in duration, to establish the specific nature of the disease. The remarks of Dr. Bateman with reference to the London epidemic of 1810, as cited by Dr. Henderson, with his comments, are deserving of being repeated in this place, as bearing upon the point in question:

"Its character is greatly varied by the different circumstances in which it occurs, by the age, constitution, and previous health of the patient, by the intensity of the exciting causes, by the situation, and season, and by early neglect or mismanagement; but it is not more varied than other febrile diseases, the smallpox for instance, or scarlet fever, under similar circumstances, and examples of the most distinct modification which it undergoes are often observed in individuals of the same family. Thus, in the instance of a man and his wife, who were brought to the house of recovery together, the former was affected with the mildest symptoms of fever, which scarcely confined him to bed, and terminated in a speedy convalescence; while his wife was lying in a state of stupor, her skin covered with petechia and vibices; in a word, exhibiting the most fordmidable symptoms of the worst form of typhus. Yet these extreme degrees of the disease manifestly originated in the same cause; and it would be equally unphilosophical to account them different kinds of fever, and give them distinct generic appellations, as in cases of benign and confluent smallpox, which are generated in like manner from one contagion." (Bateman.)

"Every one," says Dr. Henderson, "who has seen much of typhus fever, must have met with many similar examples, proving the wide range which should be allowed, in the severity of the symptoms, to fever of that kind; and if degrees of severity of the symptoms were the only distinguishing features between the fever at present epidemic, and the less prevalent typhus, there can be no question that the attempt to distinguish them would be highly unphilosophical, because contrary to the analogy furnished by the examples of other diseases of whose identical nature in the extremes of mildness and severity there can be no doubt, and at variance with the testimony derived from the oneness of the source from which they have apparently sprung. But, by parity of reasoning, it would be highly unphilosophical to refuse to account those different kinds of fever, and to grant *them* different appellations, which differ widely, not in the degree, but in the nature of their symptoms, and which cannot be traced to one contagion, but in a very remarkable manner to two."

Diversities are observed in the nature of the symptoms developed in different cases of typhus, and in different epidemics, such diversities as have led, according to the prominence of one or another set of symptoms, to the denominations of gastric, pulmonic, cephalic, or bilious fevers, but these diversities do not affect the great features, and essential characters of the fever.

"Every disease," says Dr. Henderson, "has its accidents, no two cases of the same having features precisely alike, and it is, perhaps in a peculiar measure characteristic of typhus fever, that in different persons, at the same or at different periods of the year, and also in separate epidemics, local derangements may assume diverse forms, and may affect different viscera; while, notwithstanding, a residuary group of particulars remains common to all the cases, and serves to establish their identity as individuals of one peculiar disease."

The descriptions contained in the work of Dr. Cormack, in the different papers before us, of the epidemic which has recently prevailed, appear to us to justify the opinion advocated by Professor Henderson, that it is a different disease, a fever of a different and distinct *species* from our common typhus.

The earliest description of the peculiarities of this epidemic is contained in the brief notice published by Professor Alison, in the 'Scottish Medical Gazette.' While Dr. Alison apparently inclines to the idea that this kind of fever originated from the same poison as the usual typhoid fever of Edinburgh, he points out with great discrimination the characters by which it was distinguished from the strictly typhoid cases. The peculiarities referred to by him are: 1. The duration of the cases, which was uniformly short; the crisis occurring in most of them on the fifth or seventh day, very few being protracted beyond the ninth. 2. The absence of the measly eruption of typhus. 3. The frequent occurrence of jaundice, accompanied by more or less fulness and tenderness in the hypochondrium, and vomiting of green bile, or brownish matters like hare-soup. 4. The unusual degree of sickness and vomiting both in the jaundiced cases, and in others. 5. The constant, or almost constant, occurrence of a relapse, generally taking place on the fourteenth day. 6. The termination of the disease, in the great majority of cases, by profuse critical sweats. 7. The frequency of severe muscular pains of a rheumatic character, during and after the sweatings, and particularly after the relapse. 8. The mortality, which was very small, not exceeding one in thirty, in the cases which Dr. Alison had seen. In the notice given in the same journal by Dr. Arrott, of the epidemic as it was seen in the Dundee Infirmary, where it presented the same general features, with perhaps the more frequent occurrence of *black* vomiting, the mortality was singularly small, there being only seven deaths out of 672 cases. The last peculiarity noticed by Dr. Alison, is the fact that in every pregnant woman affected with the fever who came under his care, abortion took place.

In the paper of Professor Henderson, the differences between this epidemic and the common typhus constitute the principal object of his remarks; the distinguishing characters of the two fevers are, consequently, more fully pointed out, and contrasted. One of the most remarkable peculiarities, and the first which attracted the notice of Dr. Henderson, was the great frequency of the pulse in the new disease as compared with typhus, and the very different prognosis which was afforded by its frequency in the two diseases. This comparison, in consequence of the short duration of the epidemic fever, was necessarily limited to the first five days; and referring to examples in which it had been made, Dr. Henderson found the average frequency of the pulse in typhus on or before the fifth day, to be 100 per minute, while in the epidemic fever it was 123. He further observes, that according to common observation, a great frequency of the pulse in typhus fever, more particularly at an early stage, is deemed by practical men one of the most alarming symptoms next to those which proclaim immediate dissolution; while in this epidemic it appeared to have no special indication. In the epidemic fever, the pulse was frequently as high as 140 or 145 on the second or third days, and the crisis took place as usual on the fifth, without any indication of danger, while in typhus, even a less degree of frequency was followed by a very large proportion of deaths.

The next peculiarity noticed by Dr. Henderson is the mode of termination and rapidity of convalescence in the epidemic fever, as contrasted with typhus:

> "In the typhus of former years," he observes, "and in exanthematic cases of fever, or unequivocal typhus of the last nine months, the decline of the disease has invariably, in my experience, been gradual from day to day, nearly a third of the whole duration of the febrile state having been usually consumed between the period when the fever began to amend and that of its total cessation. In the epidemic fever, on the contrary, a few hours witness commonly the transition from a state of extreme febrile heat, and frequency of the pulse to that of complete freedom from every trace of fever. In the ordinary course of things, we find the individual who at one visit presented a temperature of 104° or 106°, and a pulse of 130 or more, at the next, if the term of his fever has arrived in the interval, with

> a temperature of 98°, and pulse between 70 and 80. Nor is the critical discharge or secretion of the epidemic fever a less remarkable feature of difference between it and typhus. In a very few instances of typhus I have noticed a favorable change coincide with the occurrence of copious perspiration, but never a total cessation of the febrile state. In the great majority of instances, however, instead of copious perspiration coinciding, in typhus, with symptoms of amendment, it happens that it ushers in or accompanies a state of hopeless prostration, stupor, hurried breathing, and increased frequency of the pulse."

The constant occurrence of one or more relapses is next pointed out by Dr. Henderson as one of the distinguishing features of this fever. Using the term relapse in the strict sense of the term, as a return to or "repetition of the same malady through which the subject of it has recently passed," Dr. Henderson affirms that typhus never relapses. Out of 1600 or 2000 cases he himself never met with one instance of relapse, the only affections which might have been termed relapses being febrile attacks dependent upon the existence of some local inflammation occurring during the convalescence of the patient. Similar to his own experience is that of Dr. Perry, who states that of 1145 cases of typhus treated by him in the Glasgow Hospitals, in 1831, nineteen of the so-called relapses occurred, but all these were either cases of fever supervening upon some local inflammatory affection, and caught in the hospital, or local affections occurring during convalescence from the fever; and afterwards adds that "it is as absurd to talk of a relapse of typhus fever, as to talk of a relapse of smallpox or measles." The same evidence is afforded by Dr. Alexander P. Stewart in reference to the extensive epidemic of typhus which occurred in Glasgow: "I have never," he observes, "among thousands of cases, seen a single case of relapse, in the proper sense of the term, after the symptoms had begun to decline." (Edin. Medical and Surgical Journal, vol. liv, p. 300.)

In the fever under consideration, however, after the critical sweat of the fifth or seventh day, and a total though temporary remission, almost invariably a relapse, in the strict use of the word, that is to say a fresh attack of the fever, took place, generally on the fourteenth day; ushered in by shivering, and running nearly the same course as the primary attack. These relapses recurred not only once, but in a great many cases twice; and in a smaller number of cases a third, and even a fourth and fifth attack took place. With reference to the bearing of these upon the specific difference between typhus and the fever in question, Dr. Henderson remarks:

> "That the difference in this respect between the two is a highly important one, I do not suppose that any one can doubt, and when I add that the cases of true typhus, which have occurred among the same class of the population, and at the same seasons, as the other disease, have presented no such tendency to relapse, or repetition of fever, as the latter does, it will appear, I conceive, that the liability to repetition cannot be esteemed other than a distinctive feature of the one disorder, and the absence of such a liability, a distinctive feature of the other, and that neither is dependent on accidental diversities in the epidemic constitution, either of the atmosphere or of the population, by which the effects of one and the same poison, might be presumed to be modified or altered."

The history of the epidemic, as relates to its progress, and propagation, and the persons affected by it and the common typhus, is next examined by Dr. Henderson, and some interesting facts are adduced which go to establish the non-identity of the two poisons from which the two fevers originate. Referring to the generally-acknowledged fact that an attack of typhus confers on the subject of it an immunity from a fresh attack for a considerable length of time, and to the circumstance, apparently established by the history of the present epidemic so far as it extends, that the same law pertains as regards it, Dr. Henderson cites a number of cases where the *two forms* of fever were exhibited in the same persons within a short period of time; some being affected with typhus soon after recovery from the epidemic, and some with the epidemic fever within a few weeks after their recovery from typhus. These facts, in the opinion of the writer, appear, and with justice, to lead to the conclusion not only that the dissimilarity of the fevers cannot be referred to the modifying influences of constitution and season, but that they must have arisen from poisonous influences originally different in their nature and source.

The history of the progress of the epidemic fever, and that of the cases of typhus occurring during the same period, develops other facts of interest in relation to the point in question; for, in the cases investigated by Dr. Henderson and others, the attack of the epidemic fever was invariably traced to intercourse with persons affected with the same form of fever, and in cases of typhus to persons affected with typhus, while in no instance could any case be referred to the contagious influence of the other form of fever. In some of the cases referred to, the two forms of fever prevailed in the same locality but in different houses; and in such instances the cases of the epidemic invariably came from that part of the locality where other cases of a similar kind prevailed, and those of typhus from the houses alone which the typhoid affection had visited,—facts which go very far to prove a difference in the kind of poison from which the two fevers originated.

Dr. Henderson does not dwell on the local affections presented so frequently in the epidemic fever, such as the jaundice, the affections of the urinary and biliary secretions, and of the blood, nor on the absence of the exanthematous spots of typhus, deeming these features to be less characteristic and peculiar; the former affections being sometimes observed in typhus, and the latter, the eruption, being occasionally absent to a greater or less extent in some of the epidemics of typhus.

In addition to the peculiarities pointed out by Professors Alison and Henderson, as characteristic of this epidemic, Dr. Cormack enumerates the sudden and violent invasion of the disease, and bronzing, leadening, or purpling of the countenance before and after seizure. With reference to the first particular, we believe that in a majority of cases of typhus there is a latent stage of some duration, and premonitory symptoms which indicate the approaching attack: nevertheless, we have, on the other hand, seen cases of typhus very suddenly formed without any previous indication of its approach; and on the other, we have observed many cases of this epidemic preceded by premonitory symptoms extending over one or two days. The peculiarity, therefore, we do not consider as characteristic of either affection, or at least, sufficiently well marked to establish a distinctive feature of the two affections. The latter peculiarity, the bronzing, leadening, and purpling of the countenance, in some instances described as deep and persistent, we do not remember to have remarked in the cases which have come under our own observation, nor can we refrain from expressing our surprise that a symptom, which Dr. Cormack mentions as one of the peculiarities which struck all the visitors to his ward as one of the most remarkable features of this epidemic, should have escaped the notice of all the other writers on the subject.

Dr. Cormack considers the peculiarity referred to as affording an interesting point of resemblance between the yellow fever of hot countries and this epidemic. (p. 86.) While it reminded him of the aspect of the inhabitants of the marshy districts of Italy, some of his visitors of the Walcheren epidemic, and others of the remittents and intermittents of Canada, the West Indies, and Italy, it coincides, he states, with the descriptions given by Audouard and others of the dingy, flushing, or leaden hue, which heralded the appearance of jaundice in cases of 'yellow fever.' While Dr. Cormack further rejects the idea of founding a diagnosis of fever upon the existence of yellowness or the occurrence of black vomiting, and shows that in some cases of typhus, in traumatic fever, in various epidemics, and in some kinds of poisoning, these symptoms are developed, he at the same time aims at establishing an analogy, if not identity, between this epidemic and the yellow fevers of tropical climates. This analogy he founds partly upon the resemblance in these features, and partly upon the pathology of the two diseases, giving at length a table of the appearances observed in his dissections, and a list of those observed in cases of yellow fever. With regard to the pathology of the two affections, we think there is little to found one, as we believe it must be acknowledged that the *true* pathology of either is still unknown. The post-mortem appearances observed by Dr. Cormack, which coincided with those of yellow fever, we would regard as not distinctive of either affection, for the same reason that Dr. Cormack considers the jaundice and black vomit as not distinctive of any fever, and the more so, as they are mostly those only which were connected with these particular symptoms. Of the other morbid appearances of yellow fever, Dr. Cormack contents himself with saying, "all these appearances are evidently the result of congestion; and many of them, not observed in my dissections, would, in all probability, have been observed had they been looked for." (p. 125.) On the whole, we think that the analogy drawn by Dr. Cormack between this epidemic and yellow fever is rather a hasty one, and somewhat fanciful. Although we would not fix upon the comparative mortality alone of the two affections as a *distinctive* character, modified as the termination of either may be by the difference of circumstances, we cannot avoid remarking the very great difference between them in this respect, and the discrepancy which exists between the results of Dr. Cormack's observations and those of the other observers as regards the mortality of the yellow cases with brown or black vomiting. The mortality in yellow fever is known to be very great, in some epidemics being as great as one in three, and in others even as high at the commencement of the epidemic as nineteen in twenty; and the black vomit is almost invariably regarded as a most unfavorable symptom. In the epidemic under consideration the mortality in Edinburgh is stated by Dr. Alison not to have exceeded one in thirty; according to Dr. Craigie, the mortality in his wards was not more than one in sixty; by Dr. Kilgour it is stated to have been less than one in thirty-five in Aberdeen; by Dr. Mackenzie, to have been about three and a half per cent. in Glasgow;

and from Dr. Arrott's paper, it appears to have been only about one per cent. in the Dundee hospital.

The other circumstance, not less interesting, is the fact pointed out by Dr. Henderson, that there is no proof that the jaundice is of any consequence, or that it has produced a fatal event in a single instance. Although Dr. Cormack appears to regard the yellowness and black vomiting as occurring in the worst kind of cases, it is remarkable that in the epidemic as it prevailed in Dundee, where the mortality was so extremely small, and so much less than in Edinburgh, these symptoms were much more frequently observed and much more fully developed; and that the black vomit in particular, which was very rarely observed in Edinburgh, was very common there. Dr. Craigie altogether rejects the idea of the fever bearing any analogy to yellow fever. "It is scarcely possible," he says, "with any consistency in nosology, or common observation, to admit even the resemblance." (p. 416.)

Among the writers of the various papers before us, several conjectures are hazarded regarding the place which this fever should occupy in our nosology, besides those referred to in the preceding remarks. Rejecting the idea of its identity with yellow fever, or its origin from the poison of typhus, Dr. Craigie asks whether it is a remittent fever, a synochus, or identical with the *suette* or sweating-fever of Normandy; or whether it should be considered a gastric, a gastro-hepatic, a gastro-enteritic, or a rheumatic fever. These curious questions we are as little prepared to answer as Dr. Craigie himself appears to be, for he contents himself with propounding them. Into any question, indeed, regarding the nosology of fever we are disinclined to enter, believing as we do that nosology is fully as imperfect as it could well be; and that one and the same affection is very often described as illustrative, in part at least, of synochus, synocha, and typhus. But while we agree with some of the writers before us, that the descriptions given of *synocha* are, in some instances and to some extent applicable to a fever of this form, we are more concerned to trace with them its resemblance to the epidemics of former years, and to ascertain, if possible, whether a fever exists having specific characters, running a definite course, and periodically prevailing in an epidemic form, which may originate, it may be, from a definite cause or specific virus yet to be discovered. Inquiries directed to such an object, and extended to the epidemics of other diseases, may yet, it may be hoped, throw some light on the obscurity which invests the whole subject of the origin and cause of fevers.

In the article Continued Fever, by Professor Christison, published in the first volume of the 'Library of Medicine,'* the description of *synocha* is principally taken from two epidemics which prevailed in Edinburgh, the one in 1817-20, and the other in 1826-9, and leaves little doubt that those epidemics were identical with the one which has recently prevailed; or rather, that, along with the cases of typhus at that time epidemic, there prevailed also a species of fever, as at this time, steadily and uniformly distinguished from it as the *five-day fever*. In the mode of invasion, the state of the pulse, the duration, the occasional jaundice, the critical sweats, the relapses, the occurrence of rheumatic pains during convalescence, and other particulars, the cases were precisely alike. In the work of Dr. Welsh,* in which the former of these two epidemics is described, the resemblance between it and the recent one is still more fully illustrated. These two epidemics prevailed also in Ireland, along with typhus, and presented the same general characters. "There appears," says Dr. Mackenzie, (Med. Gaz., p. 27,) "considerable resemblance between the present fever and that described by Dr. Stoker, as prevailing along with typhus gravior in Dublin, in 1816. He speaks of it as a *typhus mitior*, its usual course being from three to nine days, generally terminating on or before the seventh day, but very apt to relapse on the third or fifth day, from the favorable change." We are indebted also to Dr. Mackenzie for the citation of Dr. O'Brien's description of the Dublin epidemic of 1826, which coincides so closely with that of the fever under consideration, as to merit repetition here:

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* Library of Medicine, vol. i, pp. 126-8, 140, 143-4.

> "The other species of fever, or that of the new constitution, which constituted the bulk of this epidemic, was one of short periods, terminating in three, five, seven, or nine days; but the second of those periods was the most frequent. . . . . In this fever the chain of morbid actions was rapidly formed and rapidly terminated, and the disease developed itself with energy from the commencement. The access was sudden, and usually came on about mid-day. The person, previously in perfect health, would then be seized with sickness at stomach, headach, pain in the small of the back, and chilliness. On the approach of evening, all these symptoms increased, and the febrile paroxysm was fully formed; the chilliness increased to a rigor, and the nausea to vomiting, which harassed the patient for the first three or four days of his fever in the form of an empty straining, and frequently continued through its whole course. On the evening of the fifth or seventh day, the *exacerbatio critica* commenced, which, mostly with the intervention of a rigor, but very frequently without this symptom, terminated in a profuse perspiration, which continued through the night, so that on the following morning the crisis was complete, and we generally found the patient convalescent. We frequently received the glad tidings from himself, in the following words: 'Sir, I got the cool last night.' The *cool* however, was sufficiently visible in his countenance before he opened his lips; but, unfortunately, in many instances it proved only a delusive truce to his sufferings. The patient was destined, perhaps, to be harassed by one, two, or three relapses, which prolonged the whole duration of his illness even beyond that of the most protracted typhus. In fact, the liability to frequent relapses was one of the most striking characteristics by which this fever was distinguished from all previous epidemics—at least, which happened in our time. . . . . . . Relapses, generally speaking, were milder and shorter than the original fever; but to this many exceptions occurred. The general symptoms of the summer variety of this fever, in addition to those already mentioned, were—acute headach; delirium, always active, sometimes phrenitic; rapid and hard pulse; white tongue, with florid edges, but sometimes natural; muscular and arthritic rather than deep-seated pains, or, as they were termed, 'pains of the bones,' not accompanied, however, by swelling of the joints except in a few instances; the skin, in many cases, of a light yellow tinge, and sometimes, though rarely, assuming the intense icteroid yellow, characteristic of jaundice and true yellow fever."†

A further proof of the identity of the two epidemics is derived by Dr. Mackenzie from the fact, that the same affection of the eye followed many of the cases of 1826 as that which is described by him in his interesting account of the post-febrile ophthalmitis of the recent epidemic in Glasgow.

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* On the Efficacy of Bloodletting in the present Epidemic Fever of Edinburgh. By BENJAMIN WELSH, M.D. 1819.

† Transactions of the Association of Fellows and Licentiates of the King and Queen's College of Physicians in Ireland, vol. v, pp. 266-512; Dublin, 1828.

The prevalence of epidemics of the same kind at earlier periods in Ireland, appears probable from the statements of Dr. Rutty in his 'History of the Diseases of Dublin during forty years,' also cited by Dr. Mackenzie. "In July, August, September, and October, 1739, a fever prevailed, which was attended with an intense pain in the head. It terminated," continues Dr. Rutty, "sometimes in four, for the most part in five or six days, sometimes in nine, and commonly in a critical sweat: it was far from being mortal. I was assured of seventy of the poorer sort at the same time in this fever, abandoned to the use of whey and God's good providence, who all recovered. The crisis, however, was very imperfect, for they were subject to relapses, even sometimes to the third time."* He describes the same remittent fever as occurring also in 1740, 1745, 1764, and 1765.

The limits of this article prevent us from entering further into the history of this form of fever than it has been brought casually before us in the papers under review. These remarks and the references to Dr. Rutty's work given below may suggest the mode in which the inquiry might be extended.

Leaving the history and nature of this epidemic fever with these general remarks, one or two facts of considerable interest in relation to the pathology of the affection are recorded in the works before us, which demand attention.

The pathological appearances mentioned by Dr. Cormack are referred principally to congestion of the internal organs, and are described by him as consisting mostly in a dark-coloured state of the gastro-intestinal mucous membrane, with occasional extravasation of patches of blood beneath it. With regard to the state of the blood, his remarks are as follows:

> "That the blood really is in a dissolved state was made perfectly manifest to us—*first*, by the imperfect coagulation which it underwent when drawn from the veins of patients, a homogeneous spongy mass being formed in place of a firm fibrinous clot, with a supernatant serosity; *second*, by the ecchymosis which was uniformly observed to surround flea-bites or other slight injuries of the skin; *third*, the frequent occurrence of purpurous spots; *fourth*, the hemorrhages; and *fifth*, the discoveries made by the microscope. Professor Allen Thomson had the goodness to lend me his able assistance in examining the blood of a number of my patients by means of the microscope. A few drops were taken from the thumbs on the same day (Oct. 24th) of about a dozen persons, some of them in pyrexial and others in the apyrexial state of the disorder; and it was found that in all of them there was an unusual number of pus-globules; (?) and in some cases, in addition to this, all the globules were found serrated and notched." (pp. 113-4.)

In a considerable proportion of the cases treated by Dr. Henderson, the spleen was discovered to be affected; in some instances it was enlarged so much as to be easily distinguished projecting beyond the lower and anterior margin of the hypochondrium, and in others the enlargement was discovered by percussion only. This enlargement was accompanied with tenderness over the spleen, and pain in the region of that organ—a symptom complained of by many in whom enlargement was not determined. The enlargement of the organ yielded readily to local remedies; it was in some instances accompanied by symptomatic fever, which ushered it in, or continued during the remission from the paroxysm of the epidemic.

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* Chronological History of the Weather and Seasons, and of the prevailing Diseases in Dublin. By JOHN RUTTY, M.D.; London, 1770, p. 76, pp. 90, 130, 303-4, and 319.

One of the most interesting facts connected with the pathology of the recent epidemic was the discovery of urea in the blood and serous fluid of the ventricles of the brain, in some of the patients affected with symptoms of cerebral derangement. The suspicion of this morbid condition was suggested to Dr. Henderson partly by the occurrence of convulsions in several cases in which there was no jaundice, and partly by a case in which symptoms of oppression and confusion of mind, accompanied with diminution of urine, was relieved by the occurrence of diuresis after the exhibition of diuretics. In two cases which subsequently occurred, exhibiting indications of cerebral oppression, the state of the urine was attended to and the blood analysed. In both cases the symptoms came on after the critical sweat; and the urine was somewhat diminished in quantity, although not materially. In one case, the patient, after oppression, stupor, and a repetition of convulsive fits, died, and three drachms of serum from the ventricles of the brain, with some clots of blood from the head, were examined. "Crystals of nitrate of urea were obtained in moderate abundance from the serum of the brain, and a very considerable quantity from the blood." In the other case, after somnolence, confusion, and languor, blood drawn from the arm yielded crystals of urea in small quantity. The character of the symptoms which succeed *suppression of urine*, and the explanation afforded of such cases by the detection of urea in the blood, have been long known to the profession. The possibility that a similar condition of the blood might be found to occur in other affections, and might afford the true explanation of sudden death in diseases not expected to terminate thus, was suggested, we believe, by Dr. Christison. But we have here, for the first time, met with the realization of this conjecture, in the proof afforded by the observations of Dr. Henderson, that such an event may prove the immediate cause of death in cases of *fever*; and that, too, even in cases where the obvious cause which might give rise to the presence of urea in the blood—namely, the *suppression* of urine—was absent.

The facts elicited in these observations led to the further investigation of the subject in Dr. Henderson's wards, by Mr. M. W. Taylor; and in other two cases of fever—one of the epidemic, and one of typhus, both exhibiting the development of cerebral symptoms, with *diminution* of the urinary secretion—a circumstance otherwise indicating a favorable prognosis—urea was detected in the blood. The existence of urea in the blood, says Mr. Taylor (Scottish Med. Gaz., p. 289), in other cases "has been inferred from the occurrence of those symptoms of disorders of the nervous centres, which we know to be the consequence of its undue accumulation in the circulation. These phenomena have been observed in those cases in which, from some cause or other, the daily discharge of urine has undergone material diminution. This appears to take place chiefly at that critical period of the fever marked by copious sweating, at which the febrile symptoms begin to subside, or during the apyretic intervals between the attacks. Professor Henderson was the first who drew the attention of the profession to the fact of the occasional presence of urea in the blood, at this stage of the fever, under the above-mentioned circumstances." The inferences deduced by Dr. Henderson from these observations are of great practical importance, and may prove to be so in relation to the treatment of other fevers besides the one under consideration :

> "I have adverted," he observes, "to the jaundice which is apt to occur in the course of the fever, and was once inclined to regard it as a very formidable symptom. I am in a condition to assert now, however, that there is no proof that the jaundice is of any consequence,—no proof that the jaundice has caused those symptoms of cerebral disorder which have been ascribed to it, or has produced a fatal event in a single instance. One of the cases which I have detailed was a case of jaundice; and if the blood had not been examined, the death would have been ascribed, in that instance, as it has been in not a few others, to the jaundice. Henceforth, before any one can assert that he has lost a case of this fever from jaundice, he must be prepared to show that there was no urea in the blood. The examples of complete jaundice which have come under my care have amounted only to six. Five of them had not a single symptom which made them differ from the ordinary cases, excepting the yellowness, and that was as deep as in the fatal case, in which the jaundice and the accumulation of urea occurred together. It will, therefore, be no small advantage if the facts which I have been able to determine respecting the latter, shall teach us to inquire carefully into the state of the urine at that period of the disease during which the vigilance of the physician has usually become relaxed, from the supposition that it was no longer necessary, and more especially in such cases of jaundice as present symptoms of cerebral disorder; for in these circumstances it appears that our calomel, our cupping, leeching, and blistering the right hypochondrium should give place to the remedies which act upon the kidneys."

To what extent those principles may be applicable in the treatment of typhus and other fevers, remains yet to be determined ; but many of the facts before us would tend to show they were peculiarly applicable in the treatment of the more formidable cases of this fever. Dr. Smith, in his interesting and valuable paper on the Epidemic as it appeared in Glasgow, states that, in the majority of cases where there was great depression of the powers of life, the urine was considerably suppressed, and, in old people particularly, the patient was apt to become lethargic or comatose. (p. 69.) His experience extended to 1000 cases, occurring in the lowest, filthiest, and most densely-populated part of the city. Of that number 43 died, and he states that, "when death occurred the patients seemed to sink into a state of lethargy or coma, from which they could be easily aroused, and would answer questions put to them with considerable correctness; and in this condition I have seen them remain for a couple of days." (p. 73.) These symptoms, it may be supposed, were probably dependent upon the cause pointed out by Dr. Henderson, and might have been benefited by treatment directed to the elimination of urea from the system.

Dr. Smith's paper contains some valuable facts of a statistical kind, in relation to this fever. Jaundice, which he believes did not interfere with the progress of the disease, occurred in 384 cases, petechiæ in 314, diarrhea or dysentery in 167, bronchitis in 132, and relapses in 112 cases. Pneumonia and pleuritis were only seen accompanying the relapse, and were present in 3 cases each. Cynanche laryngea occurred in 9 cases; epistaxis in 13. Of the 43 deaths, diarrhea was present in 10, jaundice in 9, vomiting in 4, diarrhea, jaundice, petechia, &c. in 12, dysentery in 2, and pneumonia in 1.

The last, and not the least interesting of the facts connected with the recent epidemic, was the occurrence, in a considerable number of cases, of a peculiar inflammatory affection of the eye, taking place during or after convalescence. The affection is described by Dr. Mackenzie under the name of *post-febrile ophthalmitis*. It appears to have occurred in greater proportion in Glasgow than in the cases treated in Edinburgh and elsewhere. In the Glasgow Eye Infirmary 36 cases occurred between the 8th of August and the 31st of October. It was characterized by amaurosis, or imperfect vision, affecting one or both eyes, and inflammatory action, extending from the retina to the other tunics of the eyeball. The symptoms are thus described by Dr. Mackenzie :

> "The characters of the disease appear to be, in the first instance, that of congestion, followed by inflammation of the internal parts of the eye, and especially of the retina, producing great imperfection of sight. This is succeeded by evident inflammation of the iris and sclerotica, the disease extends to the capsule of the lens and sometimes to the lining membrane of the cornea; there can be little doubt but that the choroid is also inflamed; while the conjunctiva remains in general but slightly affected. The part which the sclerotica takes in the disease is plain enough, from the intense injection of the blood-vessels which lie on its surface, and which, derived from the muscular and anterior ciliary arteries, are seen running in radii towards the cornea. The change of colour in the iris, the contracted state of the pupil, and the tags of adhesion between the edge of the pupil and the capsule of the lens, show the part which the iris takes in the disease. The internal membrane of the cornea, and the anterior crystalline capsule, especially the latter, are extremely muddy, showing their participation in the inflammation. The whole walls of the aqueous cell seem, in some cases, as if coated with a thin layer of lymph, of a yellowish green colour. The great deficiency of sight is not explicable from the mere muddiness of these parts, and is, besides, often the earliest symptom of the disease, showing an affection of the retina. At an early period the pupil is sometimes dilated, and does not become contracted till the inflammation embraces the iris. If not promptly combated by the appropriate remedies, the cornea and sclerotica become preternaturally flexible under the pressure of the finger, showing that the disease has extended to the vitreous body. In one case I found the cornea very flexible in the amaurotic stage, before there was any appearance of inflammation. The lacrymation is very considerable, and seems to be connected, not so much with the state of the conjunctiva, as with the pain in the interior of the eyeball. The severe pain in and round the eye, aggravated during the night, is exactly similar to what attends rheumatic and syphilitic ophthalmia, and may partly be ascribed to the pressure exercised upon the ciliary nerves within the eye by the inflamed parts, partly regarded as a direct neuralgic affection, such as we often meet with in the six branches of the fifth nerve which emerge from the orbit, when there is no evident inflammation. It is, in general, only after the iris and sclerotica have taken part in the disease that the patient complains of the ocular and circumorbital pain. So long as the disease is confined to the retina there is little or no pain. Hence, the patient is less alarmed than he should be by the mere dimness of sight, which, indeed, from only one eye being generally affected, may scarcely attract his attention. Even *photopsia*, in the early stage is not much complained of; in the last stage, *muscæ volitantes* form a constant symptom. Although, in by far the greater number of cases, all the textures of the eye suffer in this disease—on which account it may be designated an *ophthalmitis*—it sometimes happens that the inflammation is confined to one or two textures only."

These attacks occurred at various periods from three to sixteen weeks after the accession of the fever. In several instances it occurred about two weeks after convalescence from the relapse, but generally later.

The same disease, it would appear, occurred after the Dublin epidemic of 1826, and was described by Mr. Hewson, Dr. Reid, Dr. Jacob, and Mr. Wallace, confirming the views we have adopted of the identity of the epidemics.

The treatment pursued with the greatest amount of success by Dr.

Mackenzie was similar to that followed in cases of rheumatic and syphilitic iritis, and consisted principally of bloodletting, followed by calomel and opium, dilatation of the pupil with belladonna, and counter-irritation.

This affection appears to have been much more frequent in Glasgow than elsewhere, as it is not noticed by the other writers, with the exception of Dr. Cormack, who refers to only three cases, as having been seen by him. This apparent difference, however, may depend upon the circumstance of its occurring so long after the fever that, in most instances, the patients must have been dismissed from hospital previous to its development, and would probably apply to some dispensary for advice when the eye became affected. Dr. Cormack has transferred the whole of Dr. Mackenzie's interesting paper on the ophthalmitis to his pages, where it supplies what would otherwise have been a great desideratum. The rest of Dr. Cormack's work consists, in a great measure, of the reports of cases occurring in the wards of the Fever Hospital under his care, and although it bears some marks of haste, Dr. Cormack has rendered a service to the profession in recording at some length the features which struck him as distinguishing this fever.

THE TREATMENT of this epidemic appears to have been simple, and to have been directed mostly to the mitigation of sufferings, or to the subduing in some instances of local complications. Any attempts made to cut short the fever, or to accelerate the crisis, or even to prevent the relapses, seem to have failed. The disease, apparently, ran its course, and terminated at the usual period, and in the usual manner, by profuse sweating, whatever mode of treatment was adopted, equally as in those cases where the patients did not enjoy the benefit of medical treatment at all. Such cases were numerous; for, from the great prevalence of the epidemic, the hospitals, although in some instances greatly enlarged, were quite inadequate for the accommodation of all the applicants, and the dispensary physicians and pupils were equally unable to attend all the patients who sent for assistance at their own homes.

Dr. Smith appears to have directed his attention particularly to the possibility of preventing the fever from running through its usual course. His evidence on this subject is interesting, and exactly such as we should have expected :

"On the first appearance of the present epidemic amongst us, I embraced the opportunity of testing extensively the effects produced by emetics, diaphoretics, and other evacuants, in cutting short the progress of the disease. In upwards of 150 cases seen on or previous to the third day of attack, this mode of treatment was adopted; and, in addition, about fifty patients labouring under the symptoms of threatened relapse, were subjected to the same treatment as those in the incipient stage; but in all I have never seen a satisfactory arrestment of the fever: on the contrary, I have been led to believe, from what has fallen under my own observation, that this disease cannot be cut short by any means." (Op. cit., p. 73.)

The treatment adopted in the management of this fever is but briefly stated, and in some instances only incidentally alluded to, by the writers before us, with the exception of Dr. Cormack, who necessarily enters into the consideration of it at some length. Following his remarks, we shall refer to the opinions of the others as they happen to relate to the different points at issue.

Assuming the impossibility of cutting short the disease, Dr. Cormack sets out with the grand and indisputable generalization, that the great object, in reference to the prevailing epidemic, was "*to obviate the tendency to death.*" This fortunately was not great, and the physician, consequently, had the less to do. The states considered by Dr. Cormack as most apt to cause death, and therefore "to be anxiously looked for, and if possible, promptly corrected," are

> "1st. Congestion of the mucous membrane of the stomach and intestines, terminating in effusion of blood and subsequent destruction of large portions of this tissue.
>
> "2d. Congestion of one or more of the abdominal viscera, particularly of the liver and kidneys, disabling them from the performance of their secretive functions, thereby causing bodies to circulate with the blood, which ought to be separated from it, and which bodies we know act as poisons when not so eliminated from, or when directly introduced into the circulation.
>
> "3d. Debility and sinking." (p. 150.)

With regard to the first state, in two or three of the cases recorded by Dr. Cormack, debility and sinking, followed by death, appear to have been occasioned by hemorrhage from the stomach or bowels; and in those cases, exudation of dark-coloured blood, on and under the mucous membrane of the stomach and intestines, was found to a considerable extent upon examination after death.

As to the second cause of death, it refers principally to the development of urea in the blood, as pointed out by Dr. Henderson; but we have already sufficiently commented upon that point, and on the important relations which it bears to the treatment of this and, probably, of other fevers. In as far as the cause referred to (congestion of the abdominal viscera) is stated to have operated through the medium of the liver, we think there is every reason to believe, from the evidence before us, that the cases affected with jaundice were not more fatal than the cases not so affected.

The debility and tendency to sinking were produced, we think, most commonly by the occurrence of diarrhea, or by the profuse sweating which formed the most frequent crisis of the fever.

Bloodletting, both general and local, appears to have afforded great relief, in the experience of Dr. Alison and others, to the violent headach, and other uneasy feelings of the early stage of the fever; and could be had recourse to without those apprehensions of protracted weakness and exhaustion, which form the great obstacle to contend with in most cases of typhus fever. The tenderness in the epigastrium or hypochondriac regions, with indications of enlargement of the spleen, appear to have yielded readily to the application of leeches. Dr. Cormack rather inclines to discountenance bloodletting, convinced that the latter symptoms give way with equal readiness after the diligent use of warm fomentations, and the former after the use of purgatives and cold applications to the head. He thinks that the headachs return soon after the bloodletting, although temporarily relieved, and appears to dread the debility which may ensue upon the use of such a remedy.

Diaphoresis appears to have occurred spontaneously on the critical day, and to have been as profuse and as beneficial when no diaphoretics were used as when they were; and although they were pretty constantly employed, they did not appear, in any instance, to accelerate the crisis, but they afforded relief or satisfaction to the feelings of the patient.

The administration of opium, and the external application of blisters or sinapisms to the epigastrium, were found very serviceable in allaying one of the most troublesome symptoms—the vomiting. In some instances, Dr. Cormack tried creosote, in others hydrocyanic acid, for this symptom, and with marked success.

The application of cold to the head for the purpose of allaying headach, and the use of cold or of hot or tepid sponging of the surface of the body in allaying restlessness, were found by Dr. Cormack to be very useful and grateful to the patients. His remarks on the application of heat and moisture to the skin are worthy of being quoted:

> "Although of opinion, that all diaphoretic and sudorific drugs are of little advantage, and that violent sudorific doses are injurious, I am yet of opinion that I have very often seen the best possible results from other means employed for the purpose of diaphoresis and sweating, such as the wet blanket, the partial warm bath, and tepid sponging. The general warm bath is apt to produce exhaustion. I had three young patients affected with ardent fever and dry hacking cough, closely wrapped up in a blanket wrung out of hot water. Above this were placed several dry blankets. They sweated most copiously from ten to fourteen hours, and were then removed to a dry bed, where they all sweated again so freely as to require to have their linen changed. They emerged from this sudorific regimen, perfectly free from fever, cough, and pains, but excessively weak and languid. A child of four years old, when in the initial rigors of the relapse, was treated in the same way with equal benefit. I was led to try this plan from having recollected that Dalrymple, in the fever of Carthagena in 1740, and Jackson subsequently, in the yellow fever of the West Indies, had found that decided and immediate benefit had resulted from rolling the patients in blankets wrung out of hot salt and water. Had the supply of bedding been adequate, this method would have been in general use." (pp. 157-8.)

It is to be regretted that Dr. Cormack does not mention the day of the fever on which this hydropathic treatment was used, in the three young patients subjected to it, and that the remedy was not more extensively tried by him.

The administration of purgatives was found useful in mitigating the symptoms, and particularly in relieving the headaches. Croton oil was prized by Dr. Cormack, because, in spite of its activity, he did not find that it produced irritation; but that, on the contrary, it appeared to soothe the gastric irritability and nervous excitement, and to leave the patients little exhausted, even after its free and frequent action.

Mercurial purges appear to have been preferred by Dr. Craigie and others in the treatment of the jaundiced cases, but, after trying them extensively, Dr. Cormack concludes that the cases went on at least as well without them.

The rheumatic pains with which the patients were affected during convalescence, do not appear to have been very amenable to treatment. From the use of colchicum, Dr. Cormack does not think his patients experienced any benefit.

Astringents, chalk mixtures, acetate of lead and opium, acetate of lead and squills, &c., were found very serviceable in the cases complicated with diarrhea, dysentery, and bronchitis; and tonics, with the occasional administration of stimulants, in the debility which, in many cases, accompanied and followed the crisis.

The chlorate of soda was used, but without any decided evidence of its advantages.

The sulphate of quinine, both in small and large doses, frequently repeated, and other reputed anti-periodic remedies, were tried in a great many cases, with a view to prevent the occurrence of relapse; but, although Dr. Cormack conjectures that the former remedy modified or delayed the relapse, none of them appeared to have any effect in the prevention of its occurrence.

None of the writers of the various notices under review had any doubts regarding the contagious nature of this fever, and numerous facts are recorded by them, both in proof of this fact and of the different sources from which the cases affected with it, and those affected with typhus, originated. To these we have already alluded; but we cannot conclude these remarks, without referring to the evidence of Mr. Ramsay, inspector of lighting and cleaning in Edinburgh, as to the influence of lime-washings of the dwellings of the poor in arresting the progress of this epidemic. This is contained in a very interesting Report, prepared by Mr. Ramsay for the Commissioners of Police, on the means of promoting the health and cleanliness of the lower classes at the public expense. It contains some interesting statements regarding the cleansing and whitewashing of those localities where fever prevailed.

The number of cases of fever compiled from the books of the Royal Infirmary and Fever Board alone, as coming under the notice of those institutions during a period of eleven months and two days, was as follows:

In January,	there were	48 cases.
February	,,	75 ,,
March	,,	77 ,,
April	,,	91 ,,
May	,,	114 ,,
June	,,	161 ,,
July	,,	267 ,,
August	,,	376 ,,
September	,,	636 ,,
October	,,	489 ,,
November	,, (including 2d December)	674 ,,
	Making a total number of	3008 cases.

of which 566 only occurred during the first six months of the year, and 2442 during the last five months and two days.

The business of whitewashing the fever localities was commenced under Mr. Ramsay's active superintendence, on the 14th of September, and continued to the 30th of October, and less extensively up to the 14th of December, during which period, he states there were washed

"690 staircases,
1531 passages,
2120 apartments, exclusive of
1212 places cleaned previous to that date.

Making a total of 5553 different places, at a total expense of £77."

And the result of Mr. Ramsay's observations, in reference to the effect of those cleanings on the progress of the fever, was as follows:

> "The effect of these lime-washings on the epidemic has been matter of great interest to myself; and I have watched with the most anxious care to ascertain whether any new cases of fever occurred in dwellings previously subjected to purification; and I have pleasure in saying, that out of a great number of cases reported to me, with two exceptions, the whole turned out to be cases of relapse. That other cases must have occurred, there can be but little doubt; but notwithstanding of the pains I have taken to discover them, I have hitherto failed in doing so."

In concluding these remarks we cannot refrain from expressing our conviction that contributions similar to those which we have in the papers under review, describing the history and progress of this epidemic, as it has appeared in different parts of the empire, for we believe that it has traversed the island, would afford a series of facts of great value to medical science; and that a collected view and digest of the facts thus obtained might throw considerable light on the subject of fever. By a historical investigation we believe it might be shown that notices of such a fever as that by which we have been recently visited, could be traced back as far as the times of Hippocrates. That such an investigation, or such a general view of the history and progress of this and former epidemics of the same fever, would, at the present time, develop the source of those poisonous influences by which it or other fevers may be generated, we are not sanguine enough to anticipate; but we cannot doubt that it would contribute to give precision to our present vague and unsatisfactory views regarding the forms and varieties of continued and remittent fevers, and put us one step in advance in those investigations into their causes which it is most desirable for us to effect.

ART. XVI.

1. *The Fourth Annual Report of the Registrar-general of Births, Deaths, and Marriages in England,* 1842.
2. *The Fifth Annual Report of the Registrar-general of Births, Deaths, and Marriages in England,* 1843.
3. *An Essay on the best modes of representing accurately, by Statistical Returns, the Duration of Life, and the Pressure and Progress of the Causes of Mortality amongst different classes, &c.* By EDWIN CHADWICK, Esq. 8vo, pp. 42. (*Reprinted from the Quarterly Journal of the Statistical Society, April,* 1844.)

THE Fourth Annual Report of the Registrar-general contains the usual tables, showing the movement of the population, both absolutely, during the year ending June 30th, 1841, and comparatively, with reference to the preceding year. It appears, that the number of deaths was greater in 1840-1 than in the previous year; the mortality only slightly exceeded that of the antecedent year, but was nearly 6 per cent. (5·71) above that of the year 1838-9.

In the Appendix, by Mr. Farr, we have several interesting documents, displaying his usual industry and research. The first is an essay on the increase of population, fully corroborating those anti-Malthusian views of Mr. Alison, to which we have given (in promulgating them,) our cordial support. In about fifty years, the population of Great Britain will have doubled, and must be supplied with subsistence; but (to quote Mr. Farr's words) there will also be double the number of men to create subsistence for her families, to man her fleets, to defend her inviolate hearths, to work her mines and manufactories, to extend her commerce, to open new regions of colonization; and double the number of minds to discover new truths, to confer the benefits, and to enjoy the felicity of which human nature is susceptible. In this little essay, Mr. Farr, like a warm-hearted patriot, draws quite an arcadian picture of his country. There can be no harm done by inspiring cheerfulness and hope, but we see dark shadows, and sad stains on the picture, such as our author depicts not.

The next document is on the registration of the causes of death, and contains the statistical nosology placed in the hands of the district registrars for their guidance; to it is appended a useful alphabetical list of the diseases and their synonyms. Then follows an essay on nomenclature, including an analysis of morbid phenomena, highly creditable, we think, to Mr. Farr, for we know quite well, easy as they read, how difficult arrangements of this kind are. Numerous new terms are also recommended for adoption, and some of questionable value. It is proposed to state of a person poisoned by arsenic, that he died of *arsenicia*, or by tobacco, of *nicotinia*. People dying from cold are to be registered as dying of *psychria*, from of starvation, *pinia*. In like manner, Mr. Farr has accommodated the diseases arising from a specific animal poison, with new and not inconvenient names, excepting that they are new. The contagious matter of syphilis is to be called syphiline; of cowpox, vaccinine; glanders, equinine; measles, rubeoline, &c. "The existence of gangrenine, ergotine, tetanine, &c. may also be admitted." We need scarcely say that none of these names have much chance of being speedily "admitted" into medical glossology. One new term will, however, survive. A single word, as Mr. Farr observes, is required to replace the long periphrasis, "epidemic, endemic, and contagious diseases." This want he has well supplied by the terms *zymosis* and *zymotic*, from the Greek word *ζυμόω*, to ferment. The term is strikingly applicable, independently of the zymotic theory, to those diseases, in which a small portion of a contagious poison introduced into the system multiplies itself, and leavens the whole lump. We gladly call all epidemic, endemic, and contagious diseases, zymotic, and shall at once adopt the term.

The Fifth Annual Report is the commencement of a new series of abstracts, extending over the ordinary year ending December 31st, instead of a year ending 30th of June. This is therefore the report for 1841. The census having been taken in that year, and thus extensive data afforded for statistical estimates and comparisons, the Registrar-general has availed himself of that circumstance, and rendered this report more valuable than the preceding, by numerous additional tables. One of the most important of these additions is the essay on life-tables generally, containing a life-table for England, deduced from official returns. Life-tables, and diagrams illustrative of these, are given to exhibit the relative healthiness of three districts in London, and in Liverpool. A good deal of controversial discussion is introduced, and somebody written at very vigorously without mentioning names; but we suppose that Mr. Chadwick is the person referred to, and that the "fallacies" in the Appendix to his Supplementary Report on Interment, are denounced by the Registrar-general. We observe that in the essay by Mr. Chadwick, additional proofs are given of his doctrine, that the number of inhabitants living to one death does not express the average duration of life so accurately as the average age at death. Without entering into the merits of the controversy, we can easily imagine that some degree of irritation may be excited by this intrusion of Mr. Chadwick into the domain of the actuaries, whom it is quite possible that Mr. Chadwick may set right in some particulars.

Since writing the above, we have seen a paper by Mr. Neison, an actuary in an insurance office, controverting Mr. Chadwick's views. It is laid down by the author, firstly, that the average age at death is regulated by the distribution of the population as to age; and secondly, that this distribution is not uniform over the various periods of life in any two places, not even approximatively. The first proposition is a self-evident truth; the average age of the dying must necessarily have a fixed ratio to that of the living; if a large per cent. be young, the average age will be low, and vice versâ? No elaborate tables were required to prove this. With regard to the second proposition, we have to observe that Mr. Neison's tables, nay, his actual assertions, flatly contradict it. "While," he says, "in the counties of Essex, Suffolk, Norfolk, and Hereford, there is on an average 8·864 per cent. of the population above sixty years of age, in the towns of Liverpool, Sheffield, Manchester, Birmingham, Leeds, and Exeter, there is only 4·6 per cent. alive above the same age. These irregularities will be found to prevail in any other period of life that may be selected." It is manifest that what he calls irregularities, are, in fact, *uniform* results of the difference of health between town and rural life, as the following shows. Of persons aged from forty to sixty years, there were living in

	per cent.		per cent.
Exeter	13·28	Liverpool	14·87
Sheffield	15·50	Manchester	15·43
Birmingham	15·15	Leeds	15·23

The numbers for the four manufacturing towns, Sheffield, Birmingham, Manchester, and Leeds, are remarkably uniform, indeed nearly the same; the slight differences in Exeter and Liverpool depend, no doubt, on local circumstances.

The four agricultural counties exhibit a similar uniformity. Of persons aged from forty to sixty, there are in

	per cent.		per cent.
Devon	16·97	Essex	16·27
Norfolk	16·50	Suffolk	15·98

Can anything display the different effects of rural and town life, on longevity, more uniformly, more regularly, or more strikingly? Mr. Neison must have a singularly dull perception not to see that his own facts are as strongly corroborative of the views he attempts to controvert, as facts of the kind can possibly be. We might criticise other of Mr. Neison's deductions, as justly as the preceding, if they were really worth the criticism; but he has manifestly quite misunderstood the scope of Mr. Chadwick's arguments, and the nature of his facts.

It appears from the calculations in this, the Fifth Report, that the probable duration of life in England is 45½ years; the probable life of a male infant is 44, of a female 47 years. The expectation of life in England generally is 41 years; for males, 40 years, females, 42 years. We regret that we cannot find room for a most interesting table, exhibiting the expectation of life at different ages, and in the two sexes. Mr. Farr's Appendix in this Report surpasses his preceding contributions, and will add to his already high reputation.

We shall not follow our previous plan, and make copious extracts from the Report, as we trust the original is by this time in the hands of our readers; but devote the space thus saved to setting forth some of the advantages to society, and increments to science that these national records may afford, comparing them with those that *are* afforded.

One of the proposed advantages of the new system was the greater accuracy of registering the fact of birth, marriage, or death, and the more perfect prevention of fraud. It does not appear, however, that the act has been so successfully applied in this respect as it might be; nor, under the existing regulations, is it capable of that successful application. In registering a death, for instance, the person who testifies to have been present at the death, may *possibly* be the identical individual whose supposed death is registered: but we admit that this is not very likely to occur. The bill contains, in fact, no machinery whatever for identifying a corpse.

It must be admitted, also, that the new arrangements are not calculated to ascertain *the cause of death*, in a certain manner, but only approximatively. This is generally admitted by medical men; and they are naturally led to form this opinion by seeing so many difficulties in the way of ascertaining the real cause of death. From personal experience they know quite well that, in numerous instances, it is really impossible to state of what precise disease the individual died, or to name the disease at all. Then, again, there is often such a complication of disease in the same individual that four or five might be distinctly mentioned. Thus, a person may have chronic diarrhea, albuminuria, hypertrophy of the heart, anasarca, bronchitis, and paraplexy. Again, practitioners often treat symptoms, and will name a disease from a leading symptom, so that in the case of the individual just supposed, the cause of death might after all be entered as asthma, or orthopnea, or hydrothorax, according to the whim or theory of the medical attendant. But, in very many cases, no medical opinion whatever is given as to the cause of death The circular letter of the colleges was literally a dead letter; we should almost suspect, indeed, that it did more harm than good, as practitioners generally felt, that by acting on the instructions issued, they would acknowledge the authority of a body whose right was contested, and with which they had no interests in common. Consequently, in a certain proportion of cases, the nurse or friends, or the messenger sent to the registrar states the cause of death as it appears to them, and with what accuracy may be readily surmised. While, therefore, we admit that the present tables are infinitely superior to those that could be constructed before the passing of the act; and, also, that they are valid for many of the purposes to which they are applied by the Registrar-general, we can only regard them, in a great number of cases, as approximations, not positive results. Under the present system it is manifest that much time, labour, and zeal are being wasted, and that before this important measure can be put into really efficient operation, Mr. Chadwick's suggestion must be acted on, namely, to have an inspection of every corpse by a competent, and, of course, medical officer.

Supposing, however, this method be adopted, and more accurate returns secured, are the tabular arrangements of the Registrar-general such as to secure to medical science all the advantages to be extracted from these returns? Or do they even attain the objects for which they are drawn up? These objects are truly stated, and there is no want of liberal views, as will be seen by the subjoined extract:

> "Every person is naturally most interested in the district in which he resides; and the present form of publication will afford the scientific inquirer the means of framing tables of mortality; of calculating the mean duration of life in his own district, and comparing the results with those deduced from the facts registered in neighbouring districts, or in the whole kingdom. The inhabitants will thus learn, from authentic documents, the relative sanitary state of their neighbourhood; the causes of disease will be discovered, and suggestions which may lead to numerous improvements will be pressed upon the attention of the resident proprietors and authorities. The friendly societies will obtain from the local tables the means of adjusting their premiums equitably to the prevailing rate of sickness and mortality, which may be a fourth or a half higher in some districts than in others, and will be found to differ among the same classes, so as to affect materially the money-value of assurance, and the stability and prosperity of the societies. Materials of thought and guides of action will thus be afforded, on questions affecting health, life, and death, to scientific and benevolent persons in all parts of the kingdom." (Fourth Annual Report, p. 3.)

These remarks refer to forty-six pages of closely-printed abstracts of ages of deaths, in 324 districts, and to tables of the proportions of births, marriages, and deaths in England and Wales, arranged in counties. Now, what do these tables really teach us? Take a district with an agricultural and a town population, and they teach us, probably, that the mortality is equal to the mean mortality of England and Wales, a fact well enough known already; or take a civic population, as that of Liverpool, for example, and they show very clearly, (or at least, when illustrated by the Registrar-general's diagrams,) that the sanitary condition of Liverpool is lower than that of Surrey, or of the metropolis; but this fact we had learnt already by the simpler process of dividing the population by the annual deaths; the diagrams are mute as to the detailed information wanted. Will the Registrar-general say that the life-insurance societies are to frame their life-tables for Liverpool on these data, and demand a higher premium on the healthy lives in that wealthy emporium, than in London or Surrey? or that the benefit societies are to alter their rates in Liverpool? Manifestly not, because we know that the increased mortality is not among the life-insuring and provident classes, but among the immigrant Irish, (one third of the population of Liverpool is Irish,) and among the poor people living in the damp, unsewered cellars and courts of the town.

To place the Registrar-general's efforts in the proper light, they must be considered to be, what they are manifestly intended to be, namely, contributions to medical topography ; and, valuable as they are, so far as they go, it is manifest, that in this point of view they are extremely defective. This is no fault of the Registrar-general's ; but it *is* one, and one of such magnitude, as must render abortive his best efforts, until it is amended. In questions of this kind, it is obvious that meteorological changes, the nature of the climate and the soil, the composition of the air and the water, the food, habitations, and employments of the people, their social habits, morals, and even their race, (as in the example of Liverpool,) are all to be taken into account. Viewed in this light, the annual reports (with certain exceptions) supply no precise or available data. Let us take meteorological changes, the first of the subjects mentioned. During one complete year, there are a variety of meteorological phenomena ; at one time, hurricanes, at another meteors, at another, a succession of violent thunder-storms, at another, a comet, or shocks of earthquakes, or unnatural changes of temperature ; if we inquire what influence these had on the health or mortality of the people, and look to the Annual Reports for an answer, we may look in vain. There are not even the rough materials of an inquiry, excepting in the metropolitan weekly returns, and these are scarcely sufficient for the purpose in their present state. Nor can we learn accurately so simple a point as the influence of temperature or of the *seasons*, properly so called, on health. It is true, the Registrar-general professes to give this knowledge. Thermometric and barometric observations are detailed, and a long series of most elaborate tables drawn up, showing the births, marriages, and deaths in the four quarters of the year. Conclusions are deduced from these data as follows :

"The greatest number of marriages (36,542) occur in autumn, and the smallest number (25,174) in winter : the difference between these extremes is 11,368 ; and the four seasons stand in the following order :

	Winter.	*Summer.*	*Spring.*	*Autumn.*
	January, February, March.	July, August, September.	April, May, June.	October, November, December.
Average, 1839-41	25,174	29,502	31,559	36,542

. . . . The marriages in the four quarters are therefore the terms of a proportion." (Fifth Annual Report, p. v.)

Similar calculations and results are stated with respect to the births. We extract the following tables of seasonal mortality.

"If the mortality were uniformly at the same rate as in winter, 391,059 deaths would happen annually ; if at the same rate as in summer, 302,827 deaths would be registered. This exhibits in a striking light the fatal effects of cold ; and also of the crowding and privations to which a considerable part of the population is more exposed in cold than in warm weather. The average corrected number of deaths in the seasons would be :

Winter.	*Spring.*	*Summer.*	*Autumn.*
97,765	89,141	75,707	83,639

"By transposing the autumn and summer terms, the law is discovered which has regulated the mortality of the seasons. Thus :

	Winter.		*Spring.*		*Autumn.*		*Summer.*
	97,765	:	89,141	::	83,639	:	75,707
Differences		7629		4146		8333	

"The terms are in proportion: the product of the two extremes is nearly equal to the product of the middle terms; or the deaths in winter are to the deaths in spring as the deaths in autumn are to those in summer." (Fifth Annual Report, p. x.)

Now, on these statements we would remark, that they may be, and no doubt are, perfectly accurate as regards the four *quarters* of the year; but they cannot be received as even approximations to the relative health and mortality of the *seasons*. We would ask the Registrar-general how March can be called a winter month, when everybody knows that it is the opening month of spring? Or how can September be summer, when it is the veritable type of autumn, with its ripe fruit and corn? Or how is December an autumnal month, when all the world knows that Christmas-day is in mid-winter—the poetical personification of Yale being, indeed, an old man with a beard of long icicles? What, then, is the inference? So far as giving information on the pathological or physiological influence of the *seasons*, properly so called, all these laboriously-educed results are useless; the law of proportion a nullity; and the elaborate tables of births, marriages, and deaths in the four "quarters," and the three hundred and twenty-four districts, of no value.

We have had the deaths in the metropolis from four causes, selected from the weekly tables of mortality, and arranged according to the Registrar-general's plan, and according to the natural division of the year, as marked by the solstices and equinoxes. The months which make up the season of the Registrar-general may be seen *ante:* according to our arrangement, winter comprises the three last weeks in November, the whole of December and January, and the first week of February; spring includes three weeks in February, March, April, and one week in May; summer, the remainder of May, the whole of June and July, and one week in August; autumn occupies the rest of the year, to the first week in November, inclusive. This division, however, is not strictly accurate as to *temperature;* because the month of February is colder than November—the earth, in the latter month, being already warmed by the summer rays. The following results show the average annual differences per cent.—*plus* or *minus* the results of the tabulation on the principle of the Registrar-general. Each extends over three years, with their corresponding seasons, except the natural division, which comprehends two winters only.

	Smallpox.	*Measles.*	*Scarlatina.*	*Respiratory organs.*
Spring ..	28 per cent. plus.	6 per cent. minus.	11 per cent. minus.	14 per cent. plus.
Summer.	3 — plus.	22 — plus.	14 — minus.	5 — plus.
Autumn.	38 — minus.	30 — minus.	3 — plus.	16 — minus.
Winter ..	43 — plus.	61 — plus.	19 — minus.	3 — plus.

The table is thus read: in the metropolis, during each of the three springs of 1840-1-2, there were, on the average, 28 per cent. more deaths from smallpox than the Registrar-general's division of the seasons exhibits, 6 per cent. fewer deaths from measles, 11 per cent. fewer from scarlatina, and 14 per cent. more from diseases of the respiratory organs. In the autumns of the same three years, the deaths were not so numerous from smallpox as the Registrar-general would have us suppose, by 38 per cent.; nor from measles by 30 per cent.; nor from diseases of the respiratory organs, by 16 per cent.: but scarlatina was 3 per cent. more fatal. All these numbers, it must be remembered, are below the true difference, if *temperature* regulate the mortality, by so much as the three last weeks of November are warmer than the three last in February.

Let us pass to other points of inquiry. If the student of hygiene seeks to ascertain the influence of climate on the health and mortality, or of soil, employment, or any other of the influences before mentioned, he will derive no assistance from these Reports singly. He will never learn whether the inhabitants of the coast are more or less liable to any particular disease, or have longer or shorter lives than the inhabitants of hill-sides in the interior; nor can he compare the mortality of the latter, and their diseases, with the diseases and mortality of the people in the vales below them, or of those living on a diluvial plain, or of those on the carboniferous limestone, or on the red sandstone, &c.; because the same districts comprise an inland and a maritime, a hill-side and a valley population, &c. If he refer to the tables including the "mining" districts, to learn the most fatal diseases of colliers, he derives no specific information; for no means are given for analysis and classification of the deaths: and he knows well that the mill and the mine are generally next-door neighbours; while the employments of the factory operative and collier are too dissimilar to be ranged under the same general head.

In the Appendix to the Fifth Annual Report there is an interesting essay, by Mr. Farr, on the diseases of towns and of the open country. The mortality and diseases in five towns and seven counties, and of the metropolis and south-western division are compared. The deaths in the town districts, in the same time (four years,) out of the same amount of population, were 38 per cent. more numerous than in the country. The increase was in every class of diseases, but greatest in the zymotic class, and in that including affections of the respiratory organs.

In the present Report, Mr. Farr makes more use of algebraic formulæ than formerly. In discussing the causes of the increased mortality in towns, for example, he expresses his opinions in the form of an equation, as follows:

$$\text{"}\frac{\text{Income}}{\text{Drink} + \text{food} + \text{physic} + \text{clothing} + \text{firing} + \text{lodging} + \text{cleansing}} = r,$$

and putting n for income (in money), c for the aggregate cost (in money), of the seven necessaries, and L for the mean physiological duration of life, the equation is $\frac{n}{c} L = L'$; L' being the life in the particular circumstances. Assume the cost of a full supply of subsistence for labourers in the country to be £50 a year, that the income of the family is £50, and that he is in such favorable circumstances altogether, that he attains the natural mean term of life, the equation will be $\frac{50}{50} L = L$. But let the income be reduced to £40, and if the ratio be simple, the equation will be $\frac{40}{50} L = \cdot 8\, L$, or $\cdot 8\ L$ will be the mean duration of life of labourers in the altered circumstances." (p. 202.)

That our non-mathematical readers may understand the above (to them, probably) incomprehensible symbols, we add our explanation. By these Mr. Farr means to express his opinion, that a poor man's life is proportionate to his income. If he lives fifty years with £50 a year, deduct £10 from the cash, and ten years are deducted from the duration of his life; or, in the words of Mr. Farr, if the mean natural life = 56 years, it would be reduced to 45 years. Mr. Farr is of opinion that the ratio of mortality is as the 6th roots of the density, and gives some interesting examples. It would be an important principle if the mode of ascertaining it could be relied on, but the fallacy of such calculations is proverbial; free scope being given for assumptions, one may prove anything by figures. We are happy to see, however, that Mr. Farr very strenuously maintains that the "coefficients (the data assumed) can be determined only by careful *observation* and analysis—I mean *scientific observation*—by persons who understand the subject." Mr. Farr also suggests that the *expectation of life* should be employed to furnish exact results. In his hands, no doubt, the method would be efficient; but we apprehend there can be no valid reason for restricting the inquiry to one favorite mode. With proper corrections, the mortality amongst children might be made to exhibit the sanitary condition of a district or of a population, just as well as the expectation of life or the average age at death; or even the ratio of births might be used for hygienic calculations of this kind.

The registration of births and deaths is the registration of millions of facts in physiology and pathology, each individual fact having diversified relations. Moreover, these facts are so collected and registered (or might be), as to be capable of being tabulated, and so exhibit their diversified relations under every possible aspect. It is obvious, then, that this registration is a most important acquisition to medical science. It affords that long-wished-for desideratum, a record of multitudinous medical observations, and so similar in their essential points as to admit of comparison. In pursuance of our duty as medical journalists, we have already criticised, first, the registration of the facts, then their tabulation; we also thought it right to go a step further in pointing out one of the modes in which the defective registration might be improved, namely, by adopting Mr. Chadwick's plan: we will now suggest a method for the better tabulation of the facts.

As the registration of deaths is now conducted, the Registrar-general's efforts must necessarily be limited. One class of diseases, namely, the zymotic, may, upon the whole, be considered as registered with sufficient accuracy for scientific purposes. The deaths entered as from these diseases do not, indeed, include *all* of the class, but they include *none* of other classes. They do not include all, for perhaps one fourth of the deaths registered under other heads belong to the class. The late influenza-epidemic was not limited to the respiratory organs; it very often took the form of colitis or gastritis; sometimes it was ushered in with profuse pulmonary or intestinal hemorrhage; sometimes appeared as an apoplectic attack, and was no doubt often the cause of the "inflammations," the "pneumonia," &c. of the registers. The deaths of children registered from "convulsions" were, in many instances, deaths from one or other of the epidemics to which children are more particularly exposed. The errors in the registration of deaths from epidemics, however, will only affect the numbers dying of these diseases. As the date of each death is given, the means of estimating the influence of meteorological changes on their development and progress are in the hands of the Registrar-general, and the estimate ought to be made, as well as on deaths generally. By meteorological changes we mean something more than changes in temperature, as we have before stated. The influence of the moon on the earth and atmosphere at her various phases, of hurricanes, electric and telluro-magnetic changes, heavy rains, floods, the seasons, ought all to be inquired into and weighed. We would not even overlook important planetary conjunctions in the inquiry; and all the necessary tabulations should be so arranged that the secondary and indirect, as well as the immediate and direct results should be ascertained; and also that the effects of one week of striking meteorological changes, or even of one day, should be compared from year to year. Thus, for example, the influence of the electric 10th of August on births and deaths should be inquired into, and the effects compared from year to year. It is quite practicable to register the hour of death; and this should be done, that the effect of diurnal meteorological changes might be ascertained.

The topographical relations of the times and causes of death can only be ascertained by local inquiries; at least, the means in the hands of the Registrar-general are so imperfect that the results would not be equivalent to the necessary labour of tabulation. In October 1842 an attempt was made by him to ascertain the sanitary condition of various registry-districts of the metropolis, and the connexion of that condition with the cleanliness, supply of water, density of population, number of persons sleeping in the same room, occupations and condition of the population, nature of their food, supply of fire, and moral habits. Questions embracing these points were issued to the district registrars; but the questions are general, and, as might be expected by any one practically acquainted with these matters, the answers are general. Indeed, they could not be otherwise, for the registrars had no means of giving the information demanded; and even if they had had the means, being generally unscientific observers, their observations must have been unscientific too. Mr. Abrahams probably displays the greatest fitness for his duties—paradoxical as it may appear by—his laconic answers, "I don't know," and "I cannot tell."

Under the existing circumstances, it appears to us that the best plan for obtaining the requisite local data, would be to secure the appointment of medical superintendent registrars, with such general scientific knowledge as may enable them to carry out a general plan of inquiry, to mark out the topographical peculiarities of their district, and conduct generally a local tabulation. The latter object might be secured by having the deaths "posted up," to use a commercial phrase, under various headings by the deputy registrars. Tables for each sub-district would be thus formed. These tables would constitute the basis of more general tables, extending over the whole district, and might be transferred with the latter to the hands of a deputy registrar-general of several counties, or to the Registrar-general himself. By this division of labour, on the true inductive method, two or three important advantages would be gained. The labour itself would be rendered comparatively trifling; the results would be ascertained and communicated to the public month by month and year by year, instead of after a delay of one or two years; a perfect system of observing the relations of meteorological and topographical influences to disease and death would be commenced, and a corps of experienced men trained up to continue it with increasing efficacy and success. The additional expense would not be more than sixty or seventy per cent. on the present outlay, and would be returned a hundredfold. A tripled expenditure, however, if compared with the advantages the nation would derive from a really efficient plan of the kind laid down, would scarcely be worthy notice. Had our space permitted a full statement of the premises, we think we could have irrefragably proved that the progress of medical science, as well as of public hygiene, is mainly dependent on a proper working of the act for the registration of births, marriages, and deaths, and may be advanced by it in a constantly accelerating ratio.

Mr. Chadwick's essay is well worthy attentive perusal. The tables have all a meaning. Some of them must have cost an immense amount of labour. One or two illustrations of the practical difficulties in the way of the commonest conveniences and comforts of town life are given. In Paris the *porteurs d'eau* effectually oppose the introduction of an increased supply of water, and therewith of the means of cleanliness and health, into every house. Mr. Whitworth, of Manchester, invented a street-sweeping machine, and tried to get it introduced into the French capital, but he found the *chiffoniers'* interest to be impregnable. At New York the great obstacle to its adoption by the "profound statesmanlike men," as Mr. Chadwick quizzically terms the political leaders there, was, that it had NO VOTES; and would displace thirty or forty "independent ones."

Mr. Chadwick wishes statists would cooperate in some general plan. We refer him to an article in our current Number (art. xiv), for a suggestion of this kind; and conclude our present one with his reflections on the subject of hygiene in general, and the facts just stated:

> "The moral atmosphere under which a population is so situated is as offensive and depressing and pestilential as the physical atmosphere under which it suffers; and it is grievous to experience and melancholy to contemplate. But still there are facts of promise; we must hope and labour on; and some of the most beneficial labours of those who are fortunate in being placed above, and out of the reach of such influences, would be in the production of complete statistical returns, demonstrating, as they must do when complete, the enormous expense in money as well as in pain, sickness, and waste of life which would make it worth while to buy off on the most liberal terms every existing opposing interest to the most certain measures of human improvement.
>
> "If there could be intercommunication and simultaneous labours of statists in different places, based on local investigation, each might afford light to advance the labours of the other, and give to vital statistics a public estimation and use beyond any which they have at present obtained!" (p. 32.)

We shall not now despair of elevating the medical profession itself to as high an estimate as Mr. Chadwick desires for vital statistics. If our readers will always remember to act on the principle that the political aggrandisement of the profession depends upon its political usefulness; and that its political usefulness must be displayed by the application of medical science to social economy—the highest and noblest use of our high and noble science—such results to civilization will follow in Great Britain as have never yet been seen in any nation.

ART. XVII.

Précis Analytique sur le Cancer de l'Estomac et sur ses Rapports avec la Gastrite Chronique et les Gastralgies. Par le Docteur BARRAS. —*Paris*, 1842.

Analytical Sketch of Cancer of the Stomach, and of its Relations to Chronic Gastritis and Gastralgia. By Dr. BARRAS.—*Paris*, 1842. 8vo, pp. 134.

THE name of M. Barras has been long connected with the class of diseases to which the present small work refers, from his having published many years ago, a well known work upon Gastralgia. The particular aspect under which he considers the subject of gastric diseases in the pages before us is one of extreme interest, and his observations are evidently those of an observer who has studied the diseases he proposes to treat of.

The remarks made by the author on the morbid anatomy of cancerous disease of the stomach simply show that he accepts the current doctrines respecting the physical characters of that affection. The anatomical conditions of the diseased organ could not supply him with any direct aid in the immediate object of his work, that is, to establish the points serving, clinically, to distinguish cancerous, inflammatory, and nervous affections of the stomach. He observes, however, that few cases have ever been met with in which cancerous disease affected the entire organ, and comments upon the disproportionate frequency with which its orifices and lesser curvature suffer. On the other hand, gastric inflammation spreads over a wider surface than cancer, but a much more limited one than gastralgia. The neurosis may possibly originate in a single point, but, unless controlled at the outset, it gradually extends, implicates the entire organ, and even spreads to the liver and intestines.

M. Barras' experience (and that of every practical physician will here agree with his,) is, that cancer of the stomach is infinitely rarer than gastralgia. Within the last eighteen years he has observed at least five hundred cases of gastralgia, and not more than thirty of cancer. He considers chronic primary gastritis as rare as cancer, if not actually more so. We believe him here, too, to be correct; but he makes an almost ludicrous error (at the present day it can scarcely be styled otherwise,) in including the condition of the organ described by M. Louis as "softening with attenuation of the mucous membrane," among the number of its inflammations. Even M. Louis himself now knows that he mistook the effects of the gastric juice for a pathological condition.

Gastralgia M. Barras considers somewhat more common in females than males: cancer, on the contrary, he believes to occur much more frequently in men than women. This latter belief is certainly warranted by his own experience, for it appears that, of thirty persons in whom he ascertained the existence of the disease, twenty-six were men and four only women. It is obvious that could these numbers be regarded as significative of general experience, they would furnish an important element of diagnosis, but we have no hesitation in affirming that such is not the fact. The number of cases on which it rests is clearly insufficient to give weight to the statement.

Neurosis of the stomach is commonly met with in persons aged between fifteen and forty-five. Unlike M. Barras, we have, however, met with the affection before puberty; nor are we so certain of its never originating later than the forty-fifth year. Cancer of the stomach is principally observed between the thirtieth and sixtieth years of life. Chronic gastritis attacks both sexes indifferently, and, according to the author, is met with at all ages,—a proposition we are neither enabled to affirm nor deny.

Both gastralgia and cancer of the stomach are hereditary affections, according to M. Barras' opinion; and next to hereditary influence he considers "a constitution impaired by scrofulous, syphilitic, arthritic, rheumatismal, dartrous, or other affections implicating the economy generally," its most powerful cause. This latter proposition, if it mean anything, means simply that people of bad constitution have a stronger tendency than others to cancer of the stomach. Even thus understood, the point is mere matter of opinion, at least demonstrative evidences of its justness has never been tendered by any writer. But hear the author:

> "A circumstance *proving* the affinity of scrofula and cancer is, that tubercles of the lungs,—productions which though not perfectly identical with, are yet allied to *scirrhous tubercles*,—sometimes form in young persons born of parents cut off by cancer. Thus, two children and three grandchildren of a lady who died of uterine cancer became phthisical, and no cause could be assigned for the disease except the cancerous affection of their parent."

This is so utterly absurd, that we should certainly not have transferred the passage to our pages, but that the medical press teems with works in which etiological questions of the most serious character are settled in a similar spirit, and that we desire to use the opportunity of warning our readers against such illogical platitudes: *hæc tu Romane caveto.* Labours of another kind, and mental qualifications of another order from those undertaken by, or possessed by M. Barras, are required for the settlement of these high problems of pathology.

Assuming that cancer of the stomach is an hereditary disease, (we cannot help, as well as the writer, regarding it as such, though this proposition, too, is unproved,) he points to the non-hereditary character of gastralgia as constituting a ground of distinction between the affections.

When a disease of the stomach, says M. Barras, has been produced by immoderate hemorrhage, venesection carried to extremes, the abuse of other antiphlogistic remedies, and particularly of mucilaginous drinks, fasting, low diet, excessive coition, masturbation, &c., we may almost feel positive, that it is nervous in character. Nevertheless, these causes of debility, impoverishing, as they do, the blood, creating a proportional preponderance on the part of the lymphatic system, and so a kind of semi-scrofulous condition of the system, might, in the opinion of this particularly rigid reasoner, give rise to a predisposition to scirrhus. All this, we need scarcely say, is pure matter of hypothesis.

The writer refers to some cases of cancer of the stomach in which the causation of the disease was, according to him, evident. Two officers of rank, desirous of taking an active part in a campaign about to be entered upon, found that the ease of a long period of inaction had given such lusty proportions to their figures,—they had, in a word, grown so enormously fat, that the idea of their campaigning seemed an absurdity. They drank vinegar in abundance, thinned themselves down to respectable dimensions, and died of cancer of the stomach "in consequence."

At least so says M. Barras; but in the one case no post-mortem examination was instituted, and the other he only heard of from a brother officer of the victim of vinegar! Further, adds the writer, it is stated by M. de Rienzi, that the inhabitants of the Haonai islands being very commonly incommoded by unseemly accumulations of fat, drink *kava*, (the Haonai vinegar,) which thins them to the perfection of their desires; but the kava-drinkers soon suffer in their stomachs, and die in a state of marasmus. M. Barras thinks that they "probably die of cancer of that organ,"—a conclusion less judicious, perhaps, than convenient for his purpose.

Such being the points of distinction of the three affections connected with their presumed etiology, their comparative semeiology may be inquired into.

Cancer of the stomach is always an idiopathic malady; on the contrary, chronic gastritis is sometimes sympathetic of latent inflammation of the brain or its membranes; and gastralgia frequently thus dependent upon disease of the brain, medulla spinalis, the ganglia or plexuses of the abdominal nerves, the kidneys, bladder, or uterus.

Gastralgia and chronic gastritis, no matter how slight they may be, never run a completely latent course; scirrhus of the stomach may, on the contrary, exist for a length of time with all outward appearances of good health, and without local pain or disturbance of digestion.

The early symptoms of cancer of the stomach are well sketched in the following passage:

> "Pale tongue, or of natural colour; mouth clammy, and with mawkish or sometimes a bitter or acid taste; failure of appetite; laborious digestion, especially of solids; discomfort, uneasiness, and sensation of weight almost habitually in the region of the stomach, or dull and deep-seated pains in this region, increasing under pressure, felt when the stomach is empty, but most severe immediately after the ingestion of food; breath heavy and nauseous; eructations with disagreeable sour and caustic taste; great quantities of flatus. At a later period the epigastric pain is sometimes lancinating with occasional exacerbations, and gradually becomes continuous; the bowels grow more and more obstinately constipated, and nausea with slight (and at first rare) vomiting of watery, ropy, viscid, and glairy, sour or insipid matters occurs. Still later, a few mouthfuls of food are rejected after meals. The colour commences to change, and becomes pale, wan, and sallow."

Upon this alteration of colour of the skin, which gradually passes into the almost pathognomonic straw-coloured tint of cancer, the author lays very considerable stress;—and deservedly. We for our own part, believe firmly,—we have experienced the difficulty, and witnessed others labouring under it also,—that without it, the disease can scarcely, in no small number of cases, be distinguished satisfactorily from chronic gastritis. We of course mean so long as it has not advanced further than the stage indicated by the symptoms just enumerated. Yet there are, in well marked instances of chronic gastritis, occasionally observable a group of conditions extremely characteristic of this affection. Thus, the tongue is red, dry, and pinched; the patient suffers from thirst and sensation of heat in the stomach; the lower part of the thorax feels as if bound down firmly by a weight or ligature; the pain felt at the epigastrium is rather inconvenient than severe, and is scarcely ever present when the stomach is empty; vomitings occur at irregular periods, before or after meals; colic pains, and diarrhea supervene if, as is very frequently the fact, the inflammation spread to the intestine, slight fever with evening exacerbation sets in, and the lips, conjunctivæ, and cheeks acquire a violet tint, which is more marked during the progress of digestion, and of the febrile paroxysms than at other times.

> "In gastralgia the appetite is natural, impaired, or increased, perverted, depraved, capricious, fantastic, irregular; liquids are digested with greater difficulty than solids, whereas, the reverse is the case in cancer; the digestive process is sometimes easy, and is always affected in the end, in spite of the discomfort and suffering, which it frequently produces; the breath is free from bad smell, eructation of air free from disagreeable taste occurs frequently; pain at the epigastrium, often of greater severity than in cancerous disease, occurring in irregular paroxysms, shooting to the shoulders, and the walls of the chest, and diminishing instead of increasing under pressure, and on the ingestion of food; besides this there are curious indescribable sensations felt at the epigastrium, and singular pulsations."

It is to be remembered that pain may sometimes be altogether absent in gastralgia for a time—almost a contradiction in words. The description of neurosis of the stomach, which may be most readily confounded with cancer of the organ, is that principally characterized by vomiting of the ingesta. But in the gastralgic affection, the colour of the patient's skin undergoes no unhealthy change, and his strength and flesh do not give way.

The writer dwells emphatically upon the fact that cancer of the stomach is not attended with hypochondria. His own experience, coupled with the circumstance, that, of forty cases of cancer related in Chardel's treatise, not one furnishes an instance of hypochondriacal complication, fully justifies his statements on this point. It is, we perfectly agree with M. Barras, the nervous affection of the abdominal viscera which is allied with that distressing condition; insomuch that its existence will aid materially in excluding the idea of cancer from our diagnosis.

Chronic gastritis and gastralgia affect organs, more or less distant from that specially implicated, by *sympathy;* cancer does so more rarely, and by *absorption.*

In the second period of cancerous disease, the symptoms of the first become more marked; nausea and vomiting, above all, grow more frequent, especially when the disease is seated at the pylorus, and the solids vomited are sometimes mixed with, or followed by, blackish matters, one of the almost pathognomonic signs of the affection. Emaciation, debility, and languor increase rapidly; the colour becomes truly cancerous, as already referred to. Gurgling of fluids may now be detected by pressing the epigastrium, and local tension or an actual tumour discovered there by manual examination and percussion. The patient ceases to enjoy any intervals of ease,—his hours wear slowly on in unceasing suffering; nevertheless, the moral condition is so opposed to hypochondria, that sometimes patients have been known, up to the very day of death, to dwell on the chances of recovery. It is just, however, to add that the cancerous sufferer is generally morose, sour, and anxious about himself, but still he is not, correctly speaking, hypochondriacal. The pulse is feeble and slow, the skin cold rather than hot, in a word, the power of reaction to produce fever, seems wanting.

Nevertheless, during this stage of the disease, the only truly pathognomonic sign of its existence, is the discovery of a tumour at the epigastrium; and it is not until the third stage of the malady, at a period when it is hardly an exaggeration to call the patients moribund, that its diagnosis can, independently of the detection of tumour, be established beyond the reach of error. During the third stage, all strength is gone, the patient has frequent fainting-fits, his emaciation is carried to the extremest pitch; the features are decomposed, pinched, and cadaverous; the lower extremities frequently become anasarcous, the face bloated, the abdomen the seat of effusion; the frequency of vomiting increases, as likewise the quantity of matter vomited, and the patient expires, not very seldom, retaining his consciousness to the last, after having vomited an enormous quantity of the usual coffee-dreg looking matters.

M. Barras proceeds to the consideration of the irregularities occasionally presenting themselves in the course of cancer of the stomach. Among these are the extraordinary remissions sometimes observed, and of which we have ourselves met with two instances. Gastric cancer, as M. Barras says, may have advanced so far as to have produced black vomiting, and yet shall undergo such temporary suspension, that the outward appearances of health shall all for a time return. This is a most curious fact in pathology, but not more so than the equally well-established one, that phthisis will sometimes, (why or wherefore nobody knows, except that we know the fortunate occurrence is not the result of treatment,) stop short for a time in its progress. It will be well for cancerous persons to bear in mind that the most common cause of relapse of the symptoms in such cases as those referred to is excess at table. We regard, with M. Barras, the objection which may be urged to the view now taken of the nature of these cases; namely, that they are at first gastralgic, and subsequently become cancerous, as devoid of any degree of force. Gastralgia does *not* pass into cancer.

But, unfortunately for the facility of diagnosis, the three affections we have been considering may become associated. It is not very uncommon for chronic gastritis or gastralgia, to coexist with cancer of the stomach. Nothing but the deepest attention to the character and succession of the symptoms will in such cases detect the true nature of the malady. The reality of their occasional coexistence is proved by post-mortem examinations; as well as the reality of such a state as perfectly uncomplicated cancer.

M. Barras' observations on the treatment of cancer of the stomach are sufficiently judicious, but contain nothing positively new. He has particular confidence in the use of Vichy waters, with extract of conium taken internally, and also applied externally in the form of plaster. He correctly insists upon the importance of extreme attention to diet,—without this, in truth, medicine may be poured into the stomach without the remotest chance of benefiting the sufferer. Ioduret of potass, white oxide of antimony, *calomel*, acetate of potass, are the medicines which he thinks may be beneficially associated according, to circumstances, with the conium. Externally, carrot-poultices, mercurial plasters and frictions, or preparations of iodine or conium used in the same ways; local abstraction of blood, blisters, and baths; the application of emollients if there be evidence of inflammation in the neighbourhood of the scirrhous tumour, and narcotics internally if there be pain and signs of neurosis.

Art. XVIII.

Outlines of Pathology and Practice of Medicine. By William Pulteney Alison, m.d. f.r.s.e., Professor of the Practice of Medicine in the University of Edinburgh, &c. &c.—*London and Edinburgh*, 1843-4. 8vo, pp. 736.

On the appearance of the first and second parts of this work, we expressed a very high opinion of its merits,—an opinion confirmed by perusing it in its now complete form, in which it demands a more detailed notice than it has hitherto received from us. The work is, in truth, unique among the publications of the present day; for it is full of profound and original views of the philosophy of medicine, while it presents to the student an admirably simple and accurate exposition of the nature and treatment of diseases.

Our author's mode of introducing his subject is very uncommon, yet strikingly just and apposite.

> "As the object of physiology is to deliver the history and explanation of all the phenomena by which the living body is distinguished from the dead, so the object of pathology is to describe and explain all the phenomena by which the diseased states of the living body differ from the healthy; and we call all states of the living body diseased, in which there are such deviations from its natural condition, as cause suffering or inconvenience, or endanger life. A slight attention to this subject is sufficient to show, that there are many facts in regard to the operation of external causes on the human body, and the modes of diseased action assumed by its different organs, which could not possibly have been inferred from our knowledge of the structure and healthy action of parts, which are made known to us only by observation of the diseased conditions of the body themselves, and can only be properly generalized by an induction strictly confined to this department of knowledge. The fewer and more comprehensive these ultimate facts or laws in this department of nature, the more successful must the induction be regarded; and some of them have been already so far ascertained, as to enable us to treat the subject in some measure *synthetically*. The simplest exemplification of these ultimate facts in pathology, is to be found in cases of sudden death, and in the action of violent injuries, and we therefore premise a short account of different fatal injuries, to discussions on the pathology of the different diseases." (pp. 1, 2.)

The first chapter, on "Cases of sudden or violent death," is a remarkable specimen of powerful and highly-condensed medical reasoning, and, though it occupies only twenty-nine pages, it contains matter that might be expanded into a most useful treatise on the analogy of injuries and diseases. This subject, though almost entirely neglected, abounds with the most valuable materials for pathological reasoning and practical inference. A principal cause of its having been so little cultivated resides, doubtless, in that pernicious schism between physic and surgery which so long impeded the progress of both those branches of medicine, but which the advanced state of the general science has now, happily, almost abolished. There are many points in the practice of surgery which might long ago have both suggested and elucidated important questions in the treatment of disease. How long, for example, had the treatment of ophthalmia and gonorrhea made surgeons familiar with the use of stimulants and tonics in inflammation of mucous membranes when passing from the acute to the chronic form, before a parallel procedure found its way into the practice of physic? And yet the principle here involved is one of considerable moment, and suggests, as a subject of practical inquiry, whether inflammation of every texture may not occasionally assume an aspect under which the use of stimulants has a direct tendency to subdue the morbid action? Stimulants have, indeed, been employed by physicians time immemorial in inflammatory cases, but this only with a view of sustaining the vital powers when sinking,—a use of stimulants differing widely in principle from that which we have just represented as deducible from the practice of surgery, and which has only very recently begun to enter into the speculations of the physician. Again, the experience of surgeons in injuries of the head might long ago have suggested to physicians the important fact of the representation of cerebral disease by vomiting and other abdominal symptoms,—a fact of frequent occurrence, though not perhaps even yet so familiar as it ought to be to many practitioners in medicine. It would be easy to multiply instances in support of our argument. Instead, however, of so doing, we will attempt to analyse the first chapter of Dr. Alison's work, which affords an excellent specimen of his style of reasoning.

All causes of sudden or violent death operate either by *directly depressing or suspending the vital actions of the organs of circulation*, or by *obstructing the arterialization of the blood*, and thence *arresting the circulation at the lungs*. The first effect, or *death by syncope*, may take place in two ways: 1, by the action of a cause depressing the vital powers by which the blood is moved—as a concussion; 2, by the sudden or gradual abstraction of the vital stimulus, i. e. the blood. The second effect, or arrest of the pulmonary circulation, may also take place in two ways: 1, by injury of the nervous system arresting respiration by causing insensibility, and producing *death by coma*, or death beginning at the brain; 2, by directly impeding the access of air to the lungs, producing *death by asphyxia*, or death beginning at the lungs. But though death be always occasioned by one or other of these causes, the same cause may act, under different circumstances, sometimes chiefly in one way, and sometimes in the other. To these principles we can ascribe the known effects of the following kinds of injury:

I. Injuries affecting the *nervous system* impair, more or less, sensation, volition, and the mental phenomena; but they become dangerous to life only as they affect the circulation, either directly, or through the medium of respiration.

1. The most violent injuries affecting the nervous system suddenly arrest, or greatly impair the motion of the blood in all parts of the body, i. e. they produce a state of syncope or faintness. Such effect on the heart's actions and on the capillary circulation may result from injury of any part of the brain or spinal cord, if it extend to large portions of the nervous matter. This state of syncope forms the most characteristic part of the first symptoms of general concussion of the brain, as distinguished from more partial compression of it; and we learn from many examples that when concussion is general over the whole system, it is frequently fatal in this way, without producing any visible disorganization. It is not quite certain, in cases of general concussion, that the fatal depression of the circulating system takes place through the medium of the nervous system, and some have supposed that such injuries are fatal by a direct effect on the circulation in the smaller arteries, checking it so completely

as to oppose a resistance to the heart which it is unable to overcome; but since the effects of violent injuries, confined to the brain or spinal cord, are just similar to those of a general concussion,—since powerful mental causes, which certainly operate through the nervous system, have the same effect,—since partial, although violent, injury of other textures has no such effect,—and, lastly, since no such effect results from suddenly stopping the flow of blood in large arteries—it is highly probable that general concussion of the body does act on the vascular system through the intervention of the nervous, and that this is one of the facts included in the general physiological principle that the nervous system, although not necessarily concerned in the functions of organic life, is yet so connected with them, that, by certain changes in it, any of these functions may be variously altered or totally suspended. There is great variety as to the amount of injury which will produce this sedative effect on the circulation in different individuals of the human species, and as to the duration and termination of that effect, which sometimes abates quickly, and at others gradually increases till it is fatal. There is diversity also in the part of the circulating system chiefly affected by such injuries. In some cases the superficial circulation seems to be more reduced than in others where there is equal depression of the heart's action. In Chossat's experiments, the action of the heart was for some time little affected by certain injuries of the brain, which so checked the capillary circulation as to suspend secretion and calorification. Also, when the spinal cord, in the human subject, has been severely injured below the neck, the circulation in the capillaries has generally appeared, for some time, more affected than the action of the heart. There is likewise great variety in the effects of such injuries on the nervous system itself, which accompany or succeed the sedative effects on the circulation. Sometimes the coma is profound and long continued, sometimes it is slight and transient; sometimes there is much convulsion, at others little or none; in some cases the coma is succeeded by much headach, general or partial amentia or delirium, incessant nausea and vomiting, and, in other cases, by none of these. It is certain that all these various symptoms may exist independently of any perceptible change of structure in the nervous system. The sedative effect on the vascular, from injury of the nervous system, is seldom the immediate object of practice, because if it be not immediately fatal, it is usually followed by inflammation, against which the chief remedies must be directed: in a few cases, however, the danger from the first effects of the concussion may be averted by the cautious use of internal and external stimuli. It is important to observe that when injuries, affecting the nervous system, and through it the circulation, are not speedily fatal, but followed by inflammation and fever, the progress of these is frequently modified by the preceding or accompanying state of the system likewise consequent on the injuries, and the fever is apt to assume a typhoid, and the inflammation a gangrenous character. The relation of these facts to the true pathology of typhoid fever is still doubtful, but it is certain that in such states resulting from injury, the treatment adapted to typhoid fever is admissible, and sometimes decidedly beneficial.

2. Slighter, and especially more partial injuries of the brain, or upper part of the spinal cord, often produce death in a totally different way, namely by *coma*, the action of the heart remaining unimpaired, till the

defect of respiration, occasioned most probably by loss of sensibility, at length brings the blood to a stand in the capillaries of the lungs, and this puts a final stop to the whole circulation. In characteristic cases of this kind the circulation, and even the animal heat, not only continue till the last breath is drawn, but may even survive respiration for a short time. The most common injuries of the nervous system which cause death by coma are those in which there is partial compression of the nervous matter; but as death happens in the same way from disorganizations of the brain, which do not necessarily involve compression of its substance, and from the action of certain poisons which appears to have no connexion with compression, it is incorrect to speak generally of coma and its accompanying symptoms as indications of pressure on the brain. There is great variety as to the duration and degree of the insensibility which precedes the failure of respiration in such cases, and also as to other affections which may precede or attend that insensibility, as headach, delirium, somnolency, spasms, palsy, dilated or contracted pupil, preternaturally slow, or frequent, or irregular pulse, &c. Even the respiration itself is variously affected, it being sometimes hurried and imperfect, in others preternaturally slow and deep, previously to its final suppression.

The two preceding modes in which injuries of the nervous system may cause death, though perfectly distinct in some cases, are combined in others, as in those injuries of the head where insensibility and faintness from concussion immediately succeed the accident, but quickly abate, and are followed after an interval, by insensibility and a full pulse, and death by coma, which may then be confidently ascribed to compression of the brain by effused blood or serum. There are various causes, physical and mental, which affect the nervous system nearly in the same manner as a concussion: such as the sudden diminution of pressure to which the brain and spinal cord have been previously subjected, as instanced in the removal of depressed portions of the cranium, coagula of blood, or accumulations of serum;—in the rupture of a large blood-vessel within the head, implying a sudden determination of pressure on a part of the nervous matters, and attended at first with insensibility and feebleness of the circulation, followed by recovery of sense, rising of the pulse, and lastly the gradual accession of fatal coma from increased effusion;—in the production of syncope by the erect posture during bloodletting, and the removal of the fluid of ascites in the absence of artificial compression;—and in the syncope or diminished force of the heart's action occasioned by rising suddenly after long stooping. Another cause acting in a manner analogous to concussion of the brain is a violent blow on the abdomen, and especially on the epigastrium; and the concussion from a severe and extensive wound of any part of the abdomen is usually fatal in the same rapid way, independently of hemorrhage, and before there is time for inflammation to be established. The effects of cold water drunk when the system is heated and exhausted are referrible to the same principle. These facts illustrate the more gradual depression of the power of the heart, which is observed in the earlier stages of inflammation, and other acute diseases of the abdominal viscera. Violent injuries of various other parts of the body, especially in persons of a weakly habit, and where the nervous system is in a state of unnatural excitability, as from the abuse of opium or alcohol, may also act in the manner of a concussion, either causing sudden death, or so depressing the actions of the vascular system, as to give a typhoid type to the fever, and gangrenous tendency to the inflammation, which are to result.

In such cases, Dr. Alison thinks, it can hardly be doubted that the violence, or peculiar nature, of the sensation which attends the injury is the intervening link through which the action of the heart is affected, and, accordingly, various violent or overpowering sensations, intense pain, or the sudden transition from pain to ease, and certain mental emotions or passions, as joy, grief, anger, fear, existing in their utmost intensity, affect the circulation just as a concussion does, and sometimes with a fatal result.

II. The effect of very intense heat applied to a large surface of the body, as in an extensive burn, or to the whole body, as in a *coup de soleil*, is also quite similar to that of concussion. There is often insensibility, and, where immediate danger to life exists, there is always the characteristic depression of the heart's action. But intense heat of the sun, acting more gradually on a strong constitution not exhausted by fatigue, has often produced a state of insensibility, in which the pulse has been fuller than natural, and the vessels of the head turgid, and which has either been fatal in the way of coma, or has been relieved by copious evacuations, and the application of cold; and the same cause has often produced other diseased states connected with derangements of the cerebral circulation. In this case, the most injurious effect of the heat is evidently exerted on the vascular system exciting the heart, and probably expanding the blood in the vessels; and the brain suffers from increased compression, whereas in the former case the primary effect of the heat appears to be on the nervous system, the heart suffering from the violent impression made there.

III. It appears from experiments on animals, that lightning or other intense forms of electricity, likewise produce an effect similar to that of concussion, depressing or even extinguishing the action of the vascular system, and, at the same time, causing insensibility; but that the same agent, operating in a less intense degree produces insensibility without any immediate sedative effect on the circulation; that this state often appears to be connected with turgescence of the vessels of the head, and, probably, expansion of their contents; and that it may end in death by coma, or be followed by partial and more permanent injury of some of the nervous functions, or may be cured by depleting remedies.

IV. The effects of cold are very various according to circumstances, and depend not so much on the degree of cold that is applied to the body, nor even on the degree to which the body is actually cooled, as on the rapidity of the change, and probably, on the intensity of the sensation excited. It was found by Chossat that the temperature to which the bodies of animals killed by cold had been reduced before death varied considerably, and was always higher as the refrigeration had been more rapid, and therefore more injurious. The effect of cold on the living body has always been observed to be greater, as the sensation it excites is more intense and permanent, and therefore to be increased by all circumstances, either of the body exposed, or of the degree and mode of application of the cold, by which the intensity and duration of the sensation are increased. Cold, exerting its full effect on the body, is commonly stated to induce stupor, and death by coma. This effect, Dr. Alison remarks, has often been observed, and has sometimes been preceded by delirium, and attended by hemorrhage from the nostrils or ears, and has been found, on dissection, to be connected with considerable serous effusion in the head. It probably, therefore, depends in great measure on the flow of blood being much diminished to the surface and extremities, and proportionally increased to the brain. Persons recovering from this state of stupor by the assiduous application of heat, have continued comatose for hours after the circulation in the extremities has been restored. But in those who become comatose from cold, the heart's action is enfeebled, and it appears distinctly, from observation and experiment on man and animals, that the most intense cold may be fatal in the manner of a concussion; respiration continuing up to the moment when the heart's action ceases, the heart being found motionless, with arterial blood in its left cavities immediately after death, and artificial respiration being ineffectual in prolonging life. Cold always acts as a sedative on the capillary circulation on the surface of the body, and Dr. Edwards found that its repeated or long-continued application greatly diminished the power of subsequently generating heat. In some cases of frostbite, this effect is so powerful on the parts implicated as to put an entire stop to their vital action, even when the general system does not materially suffer; and the sedative effect on the vital actions of the frostbitten part is frequently such that the inflammation excited in a greater or less degree by the return of heat, is characterized by deficient reaction, and tends rapidly to gangrene. But these effects of cold on the vital actions of *individual parts* must be carefully distinguished from its powerful sedative operation on the *whole system;* because, in the first case, when the general circulation is strong, the chief danger is from the inflammation which ensues on the reestablishment of the circulation and natural heat of the part, and is to be moderated by causing their restoration to take place very gradually; whereas, when the vital power of the whole system has been depressed, there is no such risk of local injury from an elevation of temperature and external heat, and other stimuli may be more freely applied.

V. In regard to the action of *poisons*, Dr. Alison adverts to the following questions:

1. Is it essential to their action that they should be absorbed into the circulation, or may they act on the nerves of the parts to which they are directly applied, and affect sympathetically the organs more essential to life? This much-disputed question he decides—we think justly—in favour of both modes of operation, but concludes that the greater part of the effect of poisons which have been absorbed into the blood, is consequent on their direct application to the more important vital organs. He adds, that while the action of some poisons taken into the blood has been observed to be attended by a change in the sensible qualities of that fluid, and especially by diminution or loss of its coagulability, many other poisons act fully without altering its sensible properties, so that, where such alteration occurs, it is doubtful how far it may be connected with the action of the poisons on the living solids; the loss of coagulability in the blood also is so common in cases of sudden death,

that of itself it throws no light on the immediate cause of the fatal event. It is important to keep these considerations in mind when reasoning analogically from poisons to malignant diseases, since they show that, although the blood be changed by the agency of the cause of these diseases, it does not necessarily follow that such change is concerned in producing the most essential symptoms, or the fatal termination.

2. When a poison has been absorbed into the blood, is its action on the circulation itself to be ascribed to its direct contact with the heart and vessels, or, in a great measure, to an influence transmitted to them from the nervous system, which the poison must also necessarily pervade?

> "On this point," says Dr. Alison, "all that can be stated is, that we have clear evidence of the noxious effect of many poisons on moving solids to which they are directly applied (*e.g.* on those of the fibres of the heart or intestines with which they are laid in contact), or even on vegetables; but, nevertheless, as we have seen that the action of all parts of the circulating system in animals is subjected to an influence or control, from changes taking place in the nervous system, it is quite possible that the agency of poisons circulating in the blood on muscular organs, and especially on the circulation, may be in part consequent on the impression which they make on the brain and nerves. And the *order of the symptoms* in the case of some such poisons—as the oxalic acid—would seem to denote that the primary effect is on the brain and spinal cord, and that the heart suffers secondarily." (p. 21.)

3. The action of the different mortal poisons clearly exemplifies two of the modes of sudden death, namely, death by coma and death by syncope. Some poisons appear to have a peculiar action on the lungs, but the only case in which they act strictly by asphyxia is that where certain gases—as carbonic acid in large quantity—excite violent spasm at the glottis.

a. The narcotic poisons produce *coma*, and the immediate cause of death consequent on their action is arrest of the pulmonary circulation; but this ultimate effect, as in the instance of injuries of the brain, is preceded by various affections of the nervous system, as delirium, convulsions, vertigo, loss of different external senses, &c., or it may take place gradually, without any of these. *b.* Other poisons are evidently fatal by *syncope*, the unequivocal indications of which kind of death are, that the respiration continues as long as the action of the heart, and that the heart is therefore found, immediately after death, motionless, unexcitable by stimuli, and filled with venous blood on the right side and arterial on the left. It is thus that death is produced by the upas antiar, and by infusion of tobacco; other poisons, as hydrocyanic acid, digitalis, strychnine, oxalic acid, arsenic, preparations of antimony and baryta, various animal poisons, &c., appear, in full doses, to act chiefly by syncope, though they have also a more complex operation. *c.* Some vegetable and many mineral poisons excite inflammation of the gastro-enteric mucous membrane, or other textures, and the symptoms of such phlegmasiæ complicate those of the direct agency of the poisons on the nervous or vascular system, and are sometimes the principal cause of death. *d.* Some poisons, taken into the system, operate slowly, and their effects are regarded simply as diseases, sometimes not to be distinguished from others which may be excited by ordinary causes, as when arsenic produces epilepsy—lead, colic and palsy, &c.

VI. In a general way the modus operandi of fatal hemorrhage is too obvious to demand notice here: the following remark, however, is worthy of attention:

> "A more sudden and violent hemorrhage affects the nervous system much more speedily—just as we have already seen—than any other means of suddenly diminishing the pressure to which the brain had been subjected does; and the impression thus made in the brain *reacts on the heart after the manner of a concussion,* and causes its action to fail much sooner than it would have done merely by reason of the loss of blood. It is only in this way that we can explain the fact, that in bleeding from a large orifice, and in the erect posture, not only sensation and the other functions of the brain are sooner suspended, but *the heart's own actions fail,* with much less loss of blood than when the orifice is smaller, and the patient lies horizontally, so that the diminution of the pressure on the brain is less and more gradual." (pp. 26-7.)

Death (VII) by *fasting,* and (VIII) by *asphyxia,* need not detain us, as our author's remarks on them are brief, and illustrate no particular analogy that has not been already adverted to.

We wish that our space permitted us to present the reader with such an analysis of the three following chapters as we have made of the first. These chapters are on "Diseases in general—their fatal terminations, or spontaneous decline;" on "The remote causes of diseases in general, and the means of their prevention;" and on "The action of remedies in general, and the evidence of their efficacy." They constitute the first part of the work before us; and to this, as exhibiting a bold and masterly outline of the philosophy of practical medicine, we wish particularly to draw the attention of our readers.

Part II treats of "Febrile diseases," including, under that denomination, the various phlegmasiæ, idiopathic fevers, and the contagious exanthemata. In the first chapter the author enters at great length into the subject of inflammation in general—a subject which we have treated so fully of elsewhere, that it is unnecessary to enter upon it here. We cannot, however, avoid observing that Dr. Alison's theoretical views of inflammation, while they exhibit much of the close and subtle reasoning characteristic of his writings, present here and there instances of positive conclusions arrived at on what appears to us very insufficient grounds.

Part III embraces the consideration of "Chronic or non-febrile diseases." It is of equal excellence with the two preceding, and, like them, combines the philosophy with the practice of medicine in a manner surpassed by no work but Cullen's 'First Lines.' In the concluding chapter, on "Chronic diseases of the nervous system and organs of sense," the student will find a very valuable guide to the proper method of studying that obscure but highly interesting department of practical medicine. In conclusion, we beg to recommend Dr. Alison's work in the strongest terms to every medical student and to the whole profession. The subtlest reasoner on the science, and the simplest practitioner or student in the art of medicine, will each find in it materials admirably suited to his purpose. It is, in truth, no ordinary book, and the production of no ordinary man.

Art. XIX.

The Influence of Climate and other Agents on the Human Constitution, with reference to the Cause and Prevention of Disease among Seamen; with Observations on Fever in general, and an Account of the Epidemic Fever of Jamaica. By Robert Armstrong, m.d. f.r.s., Deputy-Inspector of Hospitals and Fleets, &c.—*London*, 1843. 8vo, pp. 208.

This work owes its origin to a circular order, "directing the principal medical officers of the naval hospitals at Portsmouth and Plymouth to deliver, during the summer months, a course of lectures to the medical officers serving in the ships-of-war in port and in the hospitals," which induced the author, in the expectation of being called upon to undertake the duties of lecturer at one of the naval hospitals, to draw up the observations now published, by way of preparation. They were originally written fifteen years ago, and are now published, with additional illustrations, from a belief that they may be useful to young medical officers entering the service, particularly to those ordered to the West Indies.

The work is highly creditable to the author, and well adapted for the great object kept in view in its composition,—"to create habits of thinking among the junior medical officers, to induce them to store their minds with useful information, to take advantage of the opportunities afforded them, of promoting the interests of science, and to apply to purposes of practical utility, the theoretical knowledge they had acquired in the medical schools."

A considerable part of the work is devoted to an examination of the effects of external agents, and more particularly changes of climate, occupation, and habits upon the health; and of the power possessed by the human constitution of adapting itself to such changes. Such a review contains, necessarily, in a work written for the purpose originally contemplated, much that is somewhat trite; but the illustration of the general principles adduced, and their application to the varied duties, in many varying circumstances, of a naval surgeon, are highly interesting and important.

There is much truth in the following observations:

"As one of the most important duties of the surgeon of a ship-of-war consists in the adoption of such means as are best calculated to preserve the health of the seamen intrusted to his care, it becomes an object of the utmost importance, that the means he recommends to his non-professional superior are founded on correct principles, and deduced from a knowledge of the exciting causes of disease. Seasoning to the climate, malaria, &c., are often used as certain cabalistical terms to explain everything, without any very precise ideas being attached to their nature, or mode of operation. The former term is generally understood to imply the power of resisting heat, as the result of habit, without any reference to those Physiological changes in the constitution which, when fully established, constitute that power of accommodation. When fever makes its appearance in a ship in harbour, malaria is the cause assigned, especially if a little mud, or a few patches of brushwood, are found on the shore, perhaps some miles distant. The seaman, on whom these influences are supposed to produce injurious effects seems to be regarded as a passive agent ready to receive impressions, whilst but little importance is attached to the various conditions under which he is placed, and the modifications produced in the physiological state of the functions of life." (p. viii.)

To the examination of those conditions and modifications, Dr. Armstrong devotes himself in such a manner as to impress his readers with the importance of looking for the obvious, predisposing, or exciting causes of disease, to which seamen are especially exposed, rather than rest satisfied with causes which are uncertain or fanciful. Such an examination leads him to important practical observations on the habits, diet, and clothing of seamen, on the fumigation and ventilation of ships, and on the means best adapted for escaping the exciting causes of disease, or avoiding the production of that susceptibility engendered by sudden changes of temperature and habits, which, in his belief, more surely occasions fever than any palludal poison. A very simple, and, as it appears to us, efficient method of maintaining a free circulation of air through the holds of ships, is suggested, on the same principles as Dr. Reid's, and having this advantage over the windsails in common use, that it requires no attention, and is attended with no inconvenience. This ventilation Dr. Armstrong proposes to effect by supporting combustion in the common fireplace, by air brought through pipes leading from the bottom of the holds, or storerooms, to the ashpit. "By this method of ventilation, every time the fire is lighted for cooking the provisions, a rapid circulation of air through the holds would be kept up by means of this simple apparatus, which requires no attention, is perfectly safe against fire, and out of the way in carrying on the duties of the ship." (p. 125.)

Dr. Armstrong examines, at considerable length, the theories of malaria or marsh miasmata, and furnishes, in addition to facts already known, many new ones, tending, as he conceives, to disprove the existence of any such agent as the cause of fever. The following are the conclusions at which he arrives:

> "All our reasonings, therefore, respecting the nature of these miasmata, are founded on an assumption; and consequently want that certainty which constitutes the only sure ground of belief. The existence of this material can only be regarded as an assertion—a species of evidence which is not admissible in the inductive sciences; and until proofs be brought forward of a very different kind from any hitherto adduced, we cannot acknowledge, without admitting contradictions and impossibilities, the existence of this vegeto-animal poison. The belief in such a material has an injurious effect, because it tends to suppress all observation and inquiry into the various causes of disease; whereas, by close observation and the correct application of well-known principles, we shall soon find that there are many powerful causes operating upon the bodies of men in warm climates, and which are fully sufficient to account for the ravages of disease, without the necessity of calling in the aid of aerial and unsubstantial phantoms." (p. 71.)

We cannot at all admit the truth of the last statement, although we believe that we are as yet in utter ignorance of the nature of the agent or agencies represented by the conventional term malaria, or marsh miasm. That there is *something* in many localities which acts as a poison on the human constitution, giving rise to fevers of an intermitting or remitting type, must, we think, be admitted by every physician of extensive experience who has practised in southern latitudes. We quarrel not about names or natures; but we maintain, boldly, that the *common* causes of disease, however "powerful," are inadequate to explain the occurrence of many fevers which we have ourselves witnessed between the tropics, as well as in Europe.

Dr. Armstrong's delineation of the symptoms of yellow fever is graphic, his description of the morbid appearances clear and precise, and his principles of treatment, in our opinion, judicious and sound. He disapproves of mercury, bleeds for excitement, and stimulates for depression; and we fear that all that has been discovered in relation to idiopathic fevers cannot lead to more than the adoption of those principles, and the experience of such observers as Dr. Armstrong to judicious rules for the time and mode of their application.

Dr. Armstrong's description of the morbid appearances in this fever does not differ materially from that of others. In his experience, alterations in the structure or appearance of the liver appear to have been less frequently observed than the descriptions of M. Louis would have led us to anticipate. Those which were observed, and the changes seen in the mucous membranes of the stomach and intestines, were frequently seen in the bodies of persons who died of other diseases.

> "In our numerous dissections in this hospital (Jamaica), the mucous membrane of the stomach, in those who have died of fever, phthisis, and many other diseases, presents the same appearance. I have more than once taken the prescription-ticket of a yellow fever case, with the appearances on dissection detailed, and found the description exactly apply to the stomach in phthisis—by omitting the words 'black vomit,' the description was copied verbatim." (p. 179.)

Although not disposed to assert that yellow fever never can become contagious, Dr. Armstrong believes that, under ordinary circumstances, "there are no grounds for supposing that this fever either originates from, or is propagated by, contagion." (p. 197.) He tried the effects of inoculation with the black matter vomited, on his own person, without any effect.

In conclusion, we commend the work to the attention of our young naval and military surgeons, as one calculated to be of great service to them in the discharge of their difficult and responsible duties on foreign stations.

ART. XX.

Special Anatomy and Histology. By WILLIAM E. HORNER, Professor of Anatomy in the University of Pennsylvania, &c. Sixth Edition. —*Philadelphia*, 1843. Two Volumes, 8vo, pp. 536, 548.

PROFESSOR HORNER is one of the few of the anatomists of America whose names are well known in Europe. His discoveries or improved descriptions of the tensor tarsi muscle, and of the longitudinal fibres of the rectum which turn under the lower margin of its internal sphincter, have gained him a deserved reputation for special investigations; and the work before us shows that he is well versed in the general knowledge of anatomy and physiology. The present is the sixth large edition published since 1826; and it is enlarged beyond all its predecessors, by the addition of extensive extracts, from the General Anatomy of Henle (translated through the medium of the Encyclopédie Anatomique), and other modern writers. It has the advantageous form of a union of descriptive anatomy with general anatomy, and a good deal of physiology; a form so agreeable, that we almost regret that the extent to which the sciences have been separately cultivated in England, should make it now almost impossible to combine them in one text-book.

Although we have not thoroughly *worked through* both these volumes, yet we have examined them sufficiently to say that the descriptive anatomy is, on the whole, accurate, and is clearly written in the American variety of our language; the general anatomy has most of the merits of the sources from which it is derived, though written with that kind of uncertainty which might be expected from one not practised in the actual investigation of the subject; and the physiology is chiefly of the old and easy kind. In none of its parts would the work be sufficiently minute for our more advanced students; yet, for a beginner, we think there is not, in the text-books of this country, one so good: it nearly accomplishes that which, for junior students, would be perfect, if it were possible, in a modernized edition of Bell's Anatomy.

The only parts that would interest our readers are the few original observations inserted here and there by the author, and of which some are annexed. Speaking of the form of the head, he quotes a recent letter from a missionary, the Rev. Mr. De Smet, who has spent some years among the Indians, on the west side of the Rocky Mountains.

> "The process of flattening the head exists among many of the tribes on the Columbia river. Among the Indians at the Cascades, and the Tschenouks at Fort-van Couver, I remarked several babes who were undergoing the barbarous process. They attach them to boards of about two feet in length, this sort of cradle is covered with a skin, with the hair outside; the child is stretched on it; its arms are tied close to the body with soft leather bandages; another skin is fastened to each extremity of the board and covers the child. A smooth strip of cedar bark, or of some other elastic wood, four or five inches broad, is fastened over the forehead of the babe, so tight that the eyes appear to start from their sockets. In this painful situation, I was told, they leave them for about a year; after which, the head has taken the form they wish to give it, and which they consider as a mark of distinction and of great beauty......The deformity disappears partly as they grow old......The Cascade Indians and Tschenouks are remarkable for their ingenuity in constructing convenient and beautiful canoes, nets, and wooden utensils; they are, in nowise, considered inferior to their round-head neighbours. Their constant intercourse with the whites has rendered them more vicious, poor, and indolent; they are much addicted to lying, stealing, and immorality." (Vol. i, p. 130.)

In the account of structure of tendons, the author says that he once applied the mode of treating ununited fractures by seton, to a case of rupture of the tendo Achilles, which, a long time after the accident, appeared unlikely to unite in the usual way. A seton of silk riband was introduced, and kept in its place for six weeks and a half: it produced considerable inflammation, but was followed by a perfect reunion of the ruptured ends of the tendon.

A case is mentioned (vol. ii, p. 278), in which the inferior cava, instead of passing at once into the right auricle, ascended on the dorsal vertebræ, in the place of the vena azygos, and, curving over the root of the right lung, united with the lower part of the superior cava. Another case is described (vol. i, p. 389), of a muscular male black subject, in whom the pectoralis minor and the sternal portion of the pectoralis major were absent; and another (vol. i, p. 432), in which the pronator carpi quadratus consisted of two triangular pieces, the bases of which were reversed.

ART. XXI.

Two Essays on the Diseases of the Spine: 1, On Angular Curvature of the Spine, and its Treatment; 2, On the Treatment of Lateral Curvature by Gravitation, Lateral Exercise, &c. By R. A. STAFFORD. —*London,* 1844. 8vo, pp. 92.

THE present treatise is "Part of the Jacksonian Prize of the Royal College of Surgeons, on the Injuries and Diseases of the Spine, with considerable alterations, corrections, and additions;" it consists, as may be seen from the title-page, of two parts, on each of which we shall make a few observations.

The essay on angular curvature of the spine occupies fifty-one pages, ten of which are devoted to cases; and in the remaining forty pages we have one of the best accounts of the disease that we have ever met with. An analysis of this excellent little essay would be quite out of place, as it does not contain anything new. Suffice it then to say that the etiology, pathology, and symptoms of the affection are detailed with clearness, precision, completeness, and brevity; while the directions for treatment are, in our opinion, extremely judicious. We know of no better or safer work on the subject to put into the hands of the student or junior practitioner. We forbear, however, from giving any extract from this portion of Mr. S.'s book, as its merit consists in the judicious use of known materials.

The second essay does not purport to be a complete essay on lateral curvatures of the spine; its title, indeed, indicates that it is merely intended to recommend a particular mode of treatment in this very refractory deformity; we have not, therefore, any right to complain that this essay is greatly less complete than the former one; for although many particulars, more especially connected with the pathology of the disease, are omitted, or imperfectly discussed; yet quite enough is said to illustrate and render intelligible the author's view respecting the treatment of the affection, to which we shall advert somewhat fully but yet briefly, inasmuch as Mr. Stafford, in the advertisement or introduction on the flyleaf, says "In lateral curvature I flatter myself I have simplified and offered an original method of treatment,—a method which at once takes these cases out of the hands of the charlatan and pretender, and places them in the power of the profession at large. I have only to add, that my experience has proved the utility of the plan adopted."

Mr. Stafford attributes lateral curvature of the spine: 1. To weakness of the spinal column and its appendages, (pp. 53-64.) 2. To the muscles of one side of the spine being brought into greater action than those of the other, whether from one limb being used more than its fellow, or from an habitually faulty carriage tending to deviate the trunk from the perpendicular. (pp. 53-8.) 3. As a cause secondary to either or both of the foregoing, to the centre of gravity being thrown on the edges of the vertebra, a circumstance which tends to aggravate the curvature, and on which, in the author's opinion, too little stress has been hitherto laid. (pp. 56-7.) Indeed, if we understand Mr. Stafford right, this secondary cause becomes ultimately by much the most important of all; muscular action, according to his views, can never do more than *commence* the curvature, which is subsequently aggravated by the unequal distribution of pressure on the vertebræ; for Mr. Stafford thus appeals to the condition of the muscles on the *concavity* of the curvature in evidence of the great influence of the secondary cause in question; those muscles, he says, "must necessarily be contracted *without having any power of action*, and thus the lateral movements of the vertebræ, as in other joints that are anchylosed from the same cause, is completely prevented. It would appear, then, when the curvature is completely formed, that the muscles which were first brought to pull the spine out of its centre of gravity, became rigid and contracted, while the opponent muscles on the convex side became stretched." (p. 57.) And again, "The muscles and the ligaments on the *concave* side of the curve are necessarily contracted, and, consequently, *are rendered incapable of performing their proper functions*, whilst those on the convex side are equally stretched beyond their natural tension, by which they are *also* in a greater or less degree *made useless*." (pp. 62-3.) If, then, the muscles on the *concave* side as well as on the convex side have no "power of action" and are "made useless," once the curvatare is established clearly, muscular action can be but an initial cause of the deformity, and must, after a certain period cease to operate.

Mr. Stafford's recommendations for the treatment of lateral curvature are as follows: we need scarcely premise that he does not hold out any hope of success in aggravated cases of very long standing. In the first instance the general health must be attended to; and then we must restore "the muscles and ligaments of the spine to their proper functions, which can only be done by bringing them into use." (p. 65.) If on inquiry the deformity seems referrible to "weakness of the spine alone, our chief care should be to relieve it of the weight it has to sustain, which can only be done by lying down," (p. 65;) but the recumbent position must alternate with as much daily moderate exercise as the patient can bear. If the habit of standing on one leg, or of using one shoulder more than the other, has caused a tendency to deformity, the habit must be corrected, and also the opposite limb should be called into frequent action, by means of some suitable game or exercise, and general gymnastics, appropriate to the particular case, should be enjoined. Such means may suffice in simple or commencing lateral curvature, but when the distortion is confirmed, then Mr. Stafford has derived the greatest benefit from *lateral exercise*. "It was from side to side that the curve first commenced; and consequently, to bring it back again to its natural state, that set of muscles ought to be more particularly acted on which would effect this object. The muscles and ligaments on the side of the spine are those which have chiefly lost their function, and to restore them, therefore, should be our chief aim." (p. 70.)

In order to perform this lateral exercise, Mr. Stafford has devised a machine, which consists of a semicircular wooden frame, resembling the platform or rocker of a hobby-horse. This frame lies on the ground on its convex surface, and the ends of a rope, which passes through two pulleys fixed in the ceiling, at a distance from each other equal to the length of the rocking-frame, are attached respectively to a bar at each end of the frame. "The patient stands upon this machine, taking hold of the rope by each hand, and then rocks himself or herself backwards and forwards (from side to side, rather,) by which both the lumbar and dorsal curve are acted on laterally." (p. 71.) Mr. Stafford has "hardly known an instance when it (lateral exercise) has not been of the greatest service." (p. 70.) But "lateral exercise, however, will not always recover a lateral curvature. The spine is ometimes so completely distorted and the vertebral column so entirely thrown out of the centre of gravity, that the muscles have lost their power. They are so stretched on the convex, and so contracted on the concave side of the curve, that they cannot act. In such cases lateral exercise will not alone be sufficient. More must be done—the spine itself must be elongated; and the best method of accomplishing this is by gravitation of the body. To effect this object I have invented a machine by which the patient can be raised up from the ground by the upper part of the body, while the lower part hangs suspended. Hence the lower part, by its own gravitation, and by additional weights being hung round the hips, gradually elongates the spinal column, until it becomes nearly if not quite, for the time being, straight. In this manner .... the muscles on the concave side are lengthened, while those on the convex are shortened, and allowed to contract, whereby they are both put into a more favorable position to pull back and retain the vertebræ in their situation." (p. 77.) "After the machine has been used I usually recommend the lateral exercise, as the muscles and ligaments are then in the best state to be strengthened." (p. 79.)

The machine here mentioned is called by Mr. Stafford the "spine elongator." We refer the reader to the original essay for a description of the apparatus; but numerous contrivances have been already proposed to effect the same object.

Several cases are detailed—no doubt quite accurately—in which the treatment we have described was adopted with perfect success. In one case, where "the curve of the spine was so great, that the spinous processes were actually hid under the right scapula or blade-bone," &c. (p. 81;) the spine ultimately "deviated so little from its natural course, that no one could ever have perceived that there had been a deformity." (p. 82.) In another similar case the patient, after two years of treatment, "had a beautiful figure." (p. 83.) Such success in such cases is, we must confess, new to us; and we could urge not a few theoretical objections to Mr. Stafford's practice, founded on his own views. We might, for example, say that, in confirmed lateral curvature, the muscles are, on Mr. Stafford's showing, inoperative in keeping up and increasing the curvature. How then can restoring their action remove a condition of things of which they have ceased to be the cause? and, least of all, how can "lateral exercise" accomplish our object? for it *equally* exercises the muscles on *both* sides of the spine, and restores their functions equally. But how can restoring the action of the muscles on the concave side of the curvature remedy the deformity? for it was those very muscles that originally produced the deformity, which their active contraction must tend to increase. We waive, however, this and other such speculations. Much as we respect theory in surgery, practical facts cannot be questioned; and we therefore leave Mr. Stafford's views and practice to be tested by experience, earnestly hoping that he may not have been misled by some of those unaccountable mistakes into which the most acute observers and conscientious men have fallen when advocating a favorite practice, and especially if that practice presents any novelty. This leads us to observe that Mr. Stafford's practice is new, so far as regards the point of lateral exercise and the machine he has devised for its performance at least; we certainly do not recollect any previous proposal to call the muscles of the spine into action in this particular direction. It just occurs to us to add that we are the less willing to theorize respecting Mr. Stafford's practice, as the condition of the muscles on the concave side of a lateral curvature of the spine is still an unsettled point in pathology; according to some they are in a state of active contraction: others maintain that they are passively contracted; while others say that they are atrophied, and all appeal both to clinical facts and to dissections in support of their views. We must, however, now conclude by recommending Mr. Stafford's Essay for the perusal of our readers.

ART. XXII.

A Practical Manual, containing a Description of the general Chemical and Microscopical Characters of the Blood, and Secretions of the Human Body, as well as of their Components, including both their Healthy and Diseased States; with the best methods of separating and estimating their Ingredients; also, a succinct account of the various concretions occasionally found in the body and forming calculi. By JOHN WILLIAM GRIFFITH, M.D. F.L.S. &c.—*London*, 1843. 12mo, pp. 62. With Two Plates.

THE plan and execution of this Manual appear to us to be alike excellent, so far as they have yet been carried out; but we have been grievously disappointed,—as we doubt not that some of its purchasers must have been,—in the limited performance of the promises set forth in the ample title-page. For it would scarcely be imagined from what there appears, that the little volume before us is solely devoted to the examination of the characters of the urine and its deposits in health and disease, and of those of vesical calculi; and that we must wait for future treatises, (of the appearances of which no definite promise is given,) before we can learn what we desire in regard to the blood and the other secretions. If this Manual had been announced as a description of the chemical and microscopical characters of the urine, &c., and as the first of a series of treatises intended to give a similar account of the other animal fluids, we should have received it with a hearty welcome. But we feel it our duty to put our readers in possession of its real scope, that they may not be disappointed in finding only a description of urine, when they were seeking for an account of blood, bile, or pus.

We have no hesitation, however, in strongly recommending this Manual as "very good as far as it goes." It will be found particularly useful to those who are working out the physiology and pathology of the urinary excretion; a department of inquiry, which, we venture to say, has not yet been pursued with that combination of previous acquaintance with the phenomena of health and disease, that philosophic acumen, and that practical skill, which the subject eminently requires. We believe that we are only now beginning to understand the true importance of the excreting processes, as *exponents* (so to speak) of the previous vital operations; and nothing but a full and complete examination of their products, and of the blood whence they are derived, can elucidate the numerous questions at issue. In this examination, so far as the urine is concerned, Dr. Griffith's Manual will prove a valuable guide; and we can therefore conscientiously give it our strong recommendation.

ART. XXIII.

1. *Nervous Diseases, arising from Liver and Stomach Complaints, Low Spirits, Indigestion, Gout, and Disorders produced by Tropical Climates; with Cases.* By G. R. ROWE, M.D. F.S.A. *Sixth Edition.—London*, 1843. 8vo, pp. 184.
2. *On some of the most Important Disorders of Women.* By G. R. ROWE, M.D. F.S.A.—*London*, 1844. 8vo, pp. 120.

THE title and recent publication of the second of these books naturally led us, in the exercise of our critical duties, to procure and read it; and its examination led to the perusal of the first work, as we found it stated in the new publication, that the doctrines inculcated in it would be found more fully exposed in the old. We were, moreover, curious to see the qualities of a medical book which, in these times, could command a sale of six editions. The result of our examination of both works is a judgment so unfavorable, that we should much regret the valuable time lost in the task, had we not been thereby enabled to warn our readers against the danger of committing the same mistake which we have done. In truth, the volumes, although bearing the semblance of professional treatises, are really written for the public; and they can only be regarded, by the impartial critic, as identical in their character and object, with the literary productions of those eminent scientific men whose benevolent purposes in regard to suffering humanity are set forth in the advertising columns of all our morning and Sunday papers. Dr. Rowe's books are, obviously, ADVERTISEMENTS calculated and destined to inform all and sundry whom it may concern, that at "CHIGWELL, ESSEX, and GOLDEN SQUARE, LONDON," (the place of date of the preface and dedication,) they will be most effectually cured of all their ails. The great body of both volumes consists of cases of the most popular diseases, written in a form intelligible to all, and portraying in glaring colours the horrors and dangers of sickness, and the potency of the therapeutics employed by the author to restore the all-prized blessings of health. In both books, out of the whole forty-nine or fifty cases, we think there is no one in which the patient is left uncured or not marvellously benefited.

We are the more decided in our opinion of the object of the author of these works, as we cannot believe that Dr. Rowe can be so ignorant or simple as not to know that they convey not one particle of information not already possessed by every medical tyro; and because he does not profess to teach the lay readers to cure themselves. Far from it: no opportunity is lost for pointing out the vast importance of professional discrimination and skill in the management of the diseases; and the invariably happy event of the author's own treatment tells silently, but with irresistible effect, that, although there may possibly be pools of healing elsewhere, the veritable Bethesda may with confidence be sought within the classical parallelogram of the Square of Gold, or amid the happy shades of Chigwell.

And yet, after all, Dr. Rowe's books are not positively bad, but only utterly useless. His practice is even, on the whole, good; and many of his doctrines are sound enough. He is, however, much too exclusive. The source of all his maladies lies in what he invariably calls the *prima via*, (on the classical principle, doubtless, of the alderman's daughter, who grumbled at the too numerous *omnibi* that passed her window;) and he strongly inculcates temperance, exercise, and the use of purgatives. But who knows not the value of these measures, and who does not prescribe them?

We said, not long since, in this Journal, that it was a comfort to well-educated and high-minded physicians to find that these popular charmers of the public almost invariably demonstrated, with their own hands, their unfitness for the ranks they had abandoned. The works before us form no exception to this general rule: they prove the total incapacity of the author to write his mother tongue; and justify the inference, that his scientific knowledge bears some proportion to his literary proficiency. Authors of this class seem to have an instinctive feeling of the value of their literary efforts, by the kind of critics they choose to be judged by. Accordingly, we find prefixed to one of the works before us no less than twelve huge pages of small type, blazoning the "*Opinions of the Public Press*," in a series of extracts of a commendatory nature, from the following high authorities: the Times, Morning Advertiser, Argus, Æra, Naval and Military Gazette, Essex Herald, Chelmsford Chronicle, Bucks Herald, Court Journal, Sunday Times, City Chronicle, Weekly Messenger, Indian Review, Cheltenham Chronicle, Essex Standard, Western Watchman, and London News!!! The only *medical* critics immortalized in this list are the Lancet and Medical Times. It is hardly necessary to mention to our readers the *means* whereby such a galaxy of popular glory can alone be created in these degenerate utilitarian days.

In justification of our assertion, that Dr. Rowe cannot write English, we give a few specimens, taken at random from his books: every page might supply similar evidence.

"........Temperate modes of life, and which must be regarded as the grand *coadjutors to the enjoyment of health, as well as to the annihilation of diseases.*" (Nervous Diseases, p. 12.) "The contractile state of the pupil of the eye.....; the increased secretion from the lacrymal glands, when any irritating substance is applied to the eyes, *are decided and merciful convictions* of that principle of self-preservation." (Ib. p. 19.) "Primitive and most *potent symptoms.*" (Ib. p. 27.) "The mind is *attended with great despondency.*" (Ib. p. 95.) "To assist the removal of *costiveness,* patients should be induced to visit a place *appropriated for that purpose* daily," &c. (Ib. p. 103.) "The foundation of his disease was laid by *an innate desire for spirits.*" (Ib. p. 113.) "During the nights and in the mornings she talked incoherently, but throughout the day she had intervals of *sensibility.*" (Ib. 122.) "The *liver* either becoming *hepatized* or obstructed," &c. (Ib. p. 153)

"From her first merging into womanhood, the pains and penalties inflicted upon her [woman] *the danger and modest solicitude peculiar to her sex alone.*" (Disorders of Women, p. 5.) "Yet how wantonly is it [health] sported with, *tampered,* and abused." (Ib. p. 6.) "....the increasing *emotions of the nervous system.*" (Ib. p. 11.) "....for all physiologists concur in the powerful dominion of that viscus [the uterus] over the *sympathies and various classes of animal life.*" (Ib. p. 23.) "....it is evident that *in proportion to its quantity* would the size of the fœtus *depend.*" (Ib. p. 27.) "....his must *prove as a powerful evidence.*" (Ib. p. 36.) "....the domestic habits of women tend much to produce inactivity and *generate muscular prostration.*" (Ib. p 42) "Can anything appear more *inconsistent than to witness* the well-protected feet of men," &c. (Ib., p. 52.) "Vicissitudes of climate and *transition of situation.*" (Ib. p. 55.) "But *every age has vied with each other.*" (Ib. p. 75.) "Enable that organ [the stomach] to *perform its natural powers.*" (Ib. p. 78.) &c. &c. &c. &c.

PART SECOND.

Bibliographical Notices.

Art. I.—*Scrofula, its Nature, Causes, and Treatment; and on the Prevention and Eradication of the Strumous Diathesis.* By W. Tyler Smith, m.b.—*London*, 1844. 8vo, pp. 172.

It is a great truth that there is nothing new under the sun; but although the amount ever made intelligible to us, of what was previously entirely unknown, be very small, the combinations of what is already known may be new, and may be more valuable than the products of actual discovery or invention. In the work before us we find neither new matter nor new combinations; and therefore we are constrained to say that, as it was not wanting, it should not have been published; it has contributed neither to the instruction nor to the amusement of mankind. Judging from the contents of the work itself, we must say that, though well written and evidently the production of a well-educated physician, it is the production of a man who has either not enjoyed advantages to enable him to speak with authority on the subject on which he treats, or, if he have, that most assuredly he has not profited by them. Too many books have been written on the subjects of scrofula to make it an easy task for any one to arrest attention to it; and most certainly, if this is ever accomplished, the task must be pursued in a way very different to that followed by Dr. Smith. We are, however, among those who, notwithstanding the numerous treatises on the subject, still consider it as a comparatively open field, and one which, if well cultivated, is capable of yielding abundant fruits: but the labourer must not fancy that he can reap such a harvest in his closet, even though surrounded by all the appliances that learning can minister.

We have some other faults besides those of omission wherewith to charge Dr. Smith. We do not, for instance, think it consistent with the dignity of a physician, when writing what should be a scientific work, to make it the vehicle of abuse, on insufficient grounds, of any political or economical system—even that of administering relief to the poor. It might have been well first to ascertain whether the children of the poor are, in reality, so much worse off under it, than when left to the unaided resources of their parents; whether, to use our author's own words, scrofula be "fearfully prevalent among the inmates of the present union workhouses;"—and whether "the New Poor Law be little better than a vast scheme for scrofulising the whole pauper population of Great Britain." Such statements are better suited to the columns of a newspaper than the pages of a scientific work.

We think there is not a little want of caution in expressing—as is here done—so very decided an opinion as to the ordinary curability of scrofula by medical means. If our author's opportunities of treating cases of scrofula had been many and great, we think his ideas and expressions in regard to this point would have been considerably modified. Sure we are that cases come every day into our hands, and into those of our most skilful medical friends, which drag on from year to year with only a small amount of amendment, in spite of every variety of treatment.

We think also there is some want of philosophy in the author's opinion, that the wide spread of scrofula in recent times is owing to the purification of our table salt! Really we did not conceive that so serious an evil lurked within those little baskets of salt. In our simplicity we had been accustomed fearlessly to prefer our own commodity to the browner representative of chloride of sodium used by our continental neighbours; and we confess to having heard that—in spite of the modicum of iodine said to be so important an element in the less pure compound—that scrofula is by no means an uncommon affliction in many European countries; we even fancy it has been stated that in some it is more rife than it is among ourselves. We are old enough to recollect the time when brown salt was in common use among ourselves: Where is the proof that we have more scrofula now than we had then?

In parting with the author of this book, we take the liberty of giving him one word of advice. We presume, though we know it not of certainty, that Dr. Smith is a young man; we are sure that he is not wanting in ability. His powers, however, for some years to come, will be turned to better account, for himself and for the profession, if he employs them in learning instead of teaching. There are many subjects which may be mastered without much experience, but scrofula is not among them.

ART. II.—*An Essay towards a Correct Theory of the Nervous System.* By J. HARRISON, M.D. &c.—*Philadelphia*, 1844. 8vo, pp. 292.

THE leading idea of this work originated in a passage in one of Dr. Stokes's lectures on medicine; in which it is suggested that the morbid condition of the nervous system in many of the neuroses, in which no appreciable change can be discovered, may be analogous to those Chemical phenomena, which are comprehended under the term *isomerism*. As in chemistry many bodies having the same constitution, (so far as the *number* of their component atoms is concerned,) possess totally different properties (resulting, it would seem from differences in the *arrangement* of their atoms,) so it may be imagined that a difference in the arrangement of the molecules of the component parts of the nerves, or their centres, may produce new modifications of their properties, without making any distinct change in their nature, or adding or abstracting a single organic molecule. This idea is expanded by Dr. Harrison with considerable acuteness and sagacity. He is evidently a man of great powers of observation and of reasoning; and we are disposed to consider many of his views as much more philosophical than speculations of this kind usually are. The author's clearness of thought and expression are well displayed in his examination of the electric theory of nervous action, and in his disquisition on life, which are contained in the Appendix. The chief objection we have to bring against the treatise, is its diffuseness. A large mass of well-known physiological facts is brought together, with the slightest possible connexion, often with none at all that we can discover; so that it is not always easy to discern the novelty of conception, which Dr. Harrison regards his views as possessing. We question whether the doctrine would be ever capable of *proof*, even with means of investigation far more refined than any which we at present possess; and we are inclined to believe, that it would be better to direct our attention to the sedulous inquiry into those alterations which we *can* appreciate. Many recent observations concur in leading to the belief, that a large proportion of those convulsive disorders of the nervous system, which are at present inexplicable by any known causes, are due to a disordered condition of the blood, resulting from an insufficient separation of the products of excretion, especially *urea;* and we would earnestly exhort those of our readers, who have attained sufficient proficiency in proximate organic analysis to conduct such inquiries, to test the condition of the blood and urine in all cases of obstinate convulsive diseases, which seem referrible to a *general* disordered state of the nervous system,—those especially which are commonly designated as the severer forms of hysteria.

ART. III.—*Tabulæ ad illustrandam Embryogenesin Hominis et Mammalium, tam naturalem quam abnormem.* Auctore W. VROLIK, Med. Doctore, &c. Fasciculus I.—*Amstelodami,* 1844.

Plates for the Illustration of the Development, both natural and abnormal, of Man and Mammalia. By W. VROLIK, M.D., Professor in the Athenæum at Amsterdam, &c. Fasciculus I, with Five Plates, Folio. —*Amsterdam, London, Dublin, &c.* 1844.

IN a former Number we mentioned Dr. Vrolik's Dutch work, on the Natural and Morbid Development of the Fœtus. The first volume alone had then appeared, as the first of a large work on the whole subject of morbid anatomy. Since that time, the second volume of the same work, published in 1842, has appeared. In it, the morbid anatomy of congenital defects is completed, in a manner which fully justifies the high opinion we expressed of the merits of the first volume, and which places the work beyond comparison with any other on the same subject. Our readers will be glad to hear that a German translation of the work is in course of preparation, through which its contents will be accessible to many more than they could be in their original language.

The work now before us, by the same author, though illustrative of that just mentioned, will yet be complete in itself. It will consist of about a hundred plates, to be published quarterly, most of them being from original drawings of the preparations in the author's, and the other rich museums of Holland. The plates included in this, the first fasciculus, illustrate the formation of the decidua and corpus luteum, the fœtal circulation, complete ectopia of the thoracic and abdominal viscera, with a cloacal condition of the pelvic organs, and a congenital hernia through the diaphragm; the intention being, in each fasciculus, to publish some of the illustrations of both the natural and the anormal states. They are all accurate, and are lithographed with a remarkable clearness and delicacy of touch, three of the five being by Mayer, by whom the author's beautiful plates of the Chimpansi were drawn. The descriptions, in Latin as well as in Dutch, are distinct and sufficiently detailed. The work will be too much "a great work" for us to recommend it to all our readers; but all who are interested in the subject should possess it, and so should all who admire "great works" of whatever kind.

ART. IV.—*On Superstitions connected with the History and Practice of Medicine and Surgery.* By T. J. PETTIGREW, F.R.S.—*London*, 1844. 8vo, pp. 198.

THIS work is a literary one, belonging to the "belles lettres," the "polite learning" of medicine. It contains the result of various and extensive reading on alchymy, astrology, talismans, amulets, charms, on the royal touch, and other similar superstitions, by which the body has been influenced, through the medium of the mind. It is the commonplace book of a literary physician with a running commentary, giving a unity and continuity to the mosaic. And although it has no directly practical aim, yet it is one of a class of books which we trust will not, in this practical age, be wholly superseded: for on such studies, in due subordination to more important ones, lead not alone to wider and less contracted views, (for "ignorance of all antiquity is a mutilation of the human mind,") but are essential to every profession which is considered a learned one. And the reputation for learning in our calling must mainly depend on the few whose tastes, industry, and early good education enable them to overcome the inducements to literary indolence, which the practice of medicine presents. On this account we hail such books as Mr. Pettigrew's. Whilst there is so much conversation on the position which medicine holds in this country, we cannot sufficiently remember that this depends mainly on each individual himself. No parliamentary enactments can raise into public estimation a body of unworthy men, or depress a class of superior men; and, unless each individual of our body assiduously cultivates his mind, and keeps himself at least on the level of the best educated classes, he himself must sink into a tradesman, and help to drag down the whole body. The habits of reading, which we get when students, should never be given up; it is one of those habits kept up, when once acquired, without much difficulty; but if not, not easily, if ever, regained. And unless a man, especially in the country, continually sharpens his own mind by collision with the minds of others of a higher grade than his own, and he can often only do that by means of books, he will gradually deteriorate.

Now we are on this subject, we cannot forbear quoting the following reminiscences from the writings of a man who, although actively employed in a laborious profession, was a most ardent student throughout his long life—the late Mr. Charles Butler. He says of himself:—

> "Very early rising,—a systematic division of his time,—abstinence from all company, and from all diversions not likely to amuse him highly,—from reading, writing, or even thinking on modern party politics,—and, above all, never permitting a bit or scrap of time to be unemployed,—have supplied him with an abundance of literary hours. His literary acquirements are principally owing to the rigid observance of four rules: to direct his attention to one literary object only at a time; to read the best book upon it; consulting others as little as possible,—where the subject was contentious, to read the best book on each side; to find out men of information, and when in their society, to listen, not to talk."

And he adds,

> "It is pleasing to him to reflect, that, though few have exceeded him in the love of literature, or pursued it with greater delight, it never seduced, or was suspected by his professional friends of seducing him, for one moment, from professional duty."

ART. V.—*Prize Essay on the Nature and Objects of Medical Science, and the Principles upon which its Study and Practice ought to be founded.* By PHILIP HENRY WILLIAMS, M.D.—*Edinburgh,* 1844. 8vo, pp. 52.

THE contents of this essay do not, for the most part, require any special notice. Nearly one half of the essay is occupied with an hypothesis which is foreign to the subject announced in the title-page. It is however, ingenious, and if expatiated upon, might possibly have made a better thesis than that of which it now forms a somewhat incongruous portion. Such hypotheses, though inadmissible in practical writings, are not to be objected to in an academical thesis, which affords fair ground for practical disquisition, for the exercise of ingenuity, or for the display of learning according to the inclination of the writer.

Dr. Williams is of opinion that the periodicity of intermittent fever may be accounted for on the supposition of the gradual decomposition of the morbific poison, and that febrifuge remedies cure the disease by acting as antiseptics on the poison. We extract the following curious passage:

"The hypothesis, however, that appears to the writer most satisfactory to account for the intermission is this, viz. that the malarious matter *when inhaled,* is in a *graduated* state of decomposition, and that, under the circumstances supposed, it will operate at distinctly graduated intervals. .... The *fact* of regular intervals occurring is one decided argument in favour of some graduation in the operation of their cause, vegetable matter. We know that decomposition is, beyond all question, a gradual process, and it is probable that a quantity of marshy air would contain matter in different degrees of decomposition. If this be granted, no argument is required to prove that, on the supposition of the most decomposed matter being the most poisonous, the operation of such a graduated substance would occur at pretty regular intervals, precisely the same *relative* decomposition being maintained between the different parts, throughout the whole operation. Thus if the dots . . . . . are supposed to represent the matter in its most poisonous state, and if $\begin{matrix} A & B & C & D & E \\ O & O & o & o & o \end{matrix}$ represent the remainder it is clear that, when E had assumed the dotted or poisonous forms, A B C D would be in the precise state in which B C D E were previously. It may be replied, 'then of course the operation would be *continuous.*' But it has been stated that the reaction following the operation of the first poisonous attack might fairly be supposed to arrest the whole process, and therefore E would remain in the state of E so long as the reaction continued; and, as soon as it ceased, E would proceed into the state of the dots indicating the poison, and D during that change, would assume the state of E, and so again be ready to pass into the dotted condition, when the next reaction ceased. And this leads to the argument in favour of the reaction causing an arrest of the poisonous development. It is this: if no reaction occurs, a *continuous* operation is the consequence; the malarious matter has full play upon the system, and comparatively little good can be done. We have in fact a case of continued or typhoid fever; the most decomposed matter is not eliminated, and a successive action is kept up on the nervous system, which effectually maintains its depression, and the depression of the circulation." (pp. 35-6.)

There is, to our mind, something rather Shandean and amusing in the mode of illustration here adopted by the author; but we much fear that some thick-headed people will not understand his meaning. Dr. Williams is inclined to similar views respecting the operation of animal poisons, in accounting for which, he advocates the cause of gradual decomposition of animal matter against that of the gradual development of ova. But we cannot further extend our notice of Dr. Williams's thesis. It is clever, and indicates some originality of mind. As it gained a gold medal from the University of Edinburgh, the author had no option but to print it in conformity with the rules of that body, otherwise its contents might perhaps scarcely be deemed of sufficient importance to render their publication desirable.

Art. VI.—*An Essay on the Tongue, in Functional Derangement of the Stomach and Bowels.* By E. Williams, m.b. — *London*, 1843. 8vo, pp. 120.

In this small volume Dr. Williams has endeavoured to trace the connexion between functional derangement of the stomach and bowels, and the appearance of the tongue. He has noted a large number of cases, placed them in tabular order, and drawn inferences and deductions. He has shown much industry, and no wish to generalize too hastily. There is, however, a want of clearness in his style so that his meaning is obscure. In the first page for instance: "An undue anxiety has been evinced to view this organ (the tongue) as an invariable monitor of functional derangement, or membranous disunion." What he intends by "membranous disunion," we cannot in the least imagine. Although fearing from the first that an author who wrote so loosely, would be found deficient in the first requisites of a teacher, we read on, as in duty bound; and were sorry to find throughout, something of the same difficulty at arriving at a distinct conception of many of the inferences and deductions. On this account we are prevented from giving our own impressions of them, as we do not feel certain it is the correct one. We trust that Dr. Williams will pursue the subject which is an important one, and digest his matter more completely. The want of clearness may be partly owing to the author being an unpractised writer, partly to the difficulty of the subject itself; but it also usually happens that where there is obscurity in expression, there is obscurity in thought.

Art. VII.—*The Diseases of Children, their Symptoms and Treatment. A Treatise intended for the use of the Student and Junior Practitioner.* By G. A. Rees, m.b. Second Edition.—*London*, 1844. 8vo, pp. 300.

We have here another specimen of those *Pseudo-second editions*, which we have already more than once exposed, and which we are determined to put down. This is *no second edition*, but the identical volume issued in 1841, with a new title-page and some ten new pages of introduction, which introduces nothing. We are compelled to come to the conclusion, therefore, that the announcement of a *second edition*, under such circumstances, is, to say the least, most improper and unjustifiable. The purchasers of the former edition are warned against laying out money on a duplicate; and Dr. Rees is requested to consider, whether he is not bound in honour to state this fact in his advertisements.

PART THIRD.

Original Reports and Memoirs.

REPORT ON THE INFLUENCE OF CIVILIZATION IN LESSENING THE NUMBER AND SEVERITY OF DISEASES.

By K. F. H. Marx, M.D.,
Professor in the University of Göttingen.*

It is a complaint often repeated, that our present epoch, although advancing in an economical and even in a mental point of view, is nevertheless retrograding in regard to the physical condition of man. It is said that the present race of mankind is feeble when compared with that of former times, and that diseases have increased, as well in their number as intensity. Many reasons seem to favour this opinion, especially the over-refinement of our social habits and enjoyments; the enlarged nomenclature of diseases, and the increased number of institutions for their cure.

This conclusion, however, although it may seem convincing at first sight, will appear utterly groundless if properly tested. It will therefore be our task to show, that, as civilization extends and increases, the sanitary condition of nations and individuals improves in the same ratio; that diseases are really on the decline, as well in number as intensity; and that every advancement in the path of knowledge and morality exercises a salutary reaction on the whole bodily condition of the human kind.

By means of advancing civilization, population not only goes on increasing, but even the duration of individual life becomes longer, and life itself less subject to the encroachment of disease. Plagues, which were considered by antiquity, and even more recently, as necessary evils and as trials of mankind, are now known only by name; and others, which were counted amongst the necessary consequences of human presumption, and as natural barriers to a state of happy existence on earth, are now easily accounted for and remedied. Several maladies which used to decimate mankind and destroy the organs of the senses and the beauty of the human form, are nearly extinguished. Others, which were considered as indispensable consequences of the progress of trades and professions, or even of the mere reunion of men in towns and cities, a more refined knowledge of hygiene has shown to be only adventitious intruders, and, as such, has successfully warred against them.

At first sight, we may readily believe that a free condition of life—one, as it is called, in accordance with nature—is the most advantageous to man, and that we ought to devote ourselves to agriculture, the pursuits of the huntsman or fisherman, if we wish to enjoy a degree of uninterrupted well-being. Every walk which we take in the open air, every sojourn in the country, every journey,—all of which so quickly and wonderfully refresh and soothe both mind and body,—seems to prove that the intimate intercourse with nature conceals the secret of health; and that every deviation from her paths is accompanied by disease and imperfection. Although this may be true, to a certain extent, we must not lose sight of the difference which exists between resorting to nature for a limited period and for recreation, and the constant following of rural pursuits as a means of subsistence. The peasant, the fisherman, the huntsman, can tell us of other concomitants and consequences of their respective callings, besides those of mere pleasure and enjoyment. The greater part of what nature yields must be extorted from her by exhausting toil, or obtained by a scarcely less exhausting patience. If the higher faculties of man are not properly excited and used, the whole mind will fall into a state of stupor and "mere oblivion." Perfect health is not possible without an adequate harmony of the bodily and mental faculties. An individual who possesses nothing but mere bodily health approaches very closely to the mere animal. Each individual man, as well as the whole race, has assuredly a very different vocation and one much more exalted than that of merely preserving health. And it will be our business to show that the tendency of civilization is to invigorate the physical man, while it promotes other endowments and creates new ones. The development of the higher faculties does not infer the manifestation of the dreaded evils. Only that mental development will be noxious which is effected without due consideration of the time requisite for its production, or of its legitimate amount, and the proper means of acquiring it. True civilization knows best how to keep within adequate bounds, and to follow that line of conduct which answers all reasonable demands.

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* This Report is taken from the second volume of the *Transactions of the Royal Society of Sciences of Göttingen*, published in 1844; it was read before the Society in May, 1843. The present translation having been made, in the first instance, by a German imperfectly acquainted with the idioms of the English language, it was found necessary to submit it to a complete revision; and although considerable pains have been taken to render the style more idiomatic, it will still be found *foreign* in many respects.

It has been thought proper to give the Report, as it stands in the original, without note or comment; but the intelligent English reader will not fail to remark more than one passage where the enthusiasm of the learned and amiable author has led him to employ his rose-colours rather too freely. Civilization has done much, doubtless; but it has not done all that the kind-hearted Professor hopes and believes.—Ed.

It is true, that the many senseless claims of society, the arbitrary dictates of conventionality and fashion, and other agencies called forth by the passions and by party strife, will often cause an interruption of health, either constant or transitory; but it is equally true that by means of the prudence, firmness, and calculated action which are the result of genuine civilization, the deleterious effects of those agencies will be either avoided or much lessened. At any rate, we cannot believe that these dark spots of civilization are sufficient to obscure the value of its brighter parts. The mental activity consequent on social cooperation, the numberless impulses which, engendered by objects of art, strike and vivify our senses, the endless impressions produced by reading, instruction, example, &c. excite, in a most important degree, the bodily functions also, and impart to them new power and energy, with a higher development of the intellectual faculties, an augmented strength of moral character, and increase of religious resignation: man becomes more capable of resisting the injurious influences of external nature; knowledge and a certain moral equipoise are found to be the most efficacious means for acquiring that pliancy and elasticity which enable us best to support the severest bodily pains. Accordingly, we believe that the robust pupil of untutored nature, deficient as he will be, in the case of severe illness, of every assistance of a higher character, will, speaking generally, be found to succumb much sooner to the same diseases than the delicate and even weakly offspring of civilization, to whom every advancement of science and art, the intellectual treasure of books, society, and conversation, will constantly minister fresh sources of life.

The reports of travellers who have sojourned for a long time amongst savage tribes are very discrepant, as far as their remarks on the sanitary condition of the people are concerned. While some speak only of a few maladies there observed, others acknowledge to have met with most of our principal ailments. But suppose even that a traveller has observed but few maladies in any place, it does not follow that they occur but rarely there. May the cause not rather lie in the inhumanity of the inhabitants, fostered by want and sanctioned by custom, and in the insufficiency of medical treatment? Those very diseases which most obtrude themselves on observation—those of a chronical or lingering character, which destroy the body by degrees, and against which art, with us, makes a determined stand—only yielding to them step by step, and even often succeeding in vanquishing them altogether—such diseases are scarcely to be met with among savages, because at the very first appearance of any such, the patient is carried off, either through neglect or want of proper management.

It is well known that fractures of the legs of animals seldom heal perfectly—a circumstance which is to be accounted for by the want of proper quiescence of the part: the animal will not lie down. The owners will not risk the trouble and expense of the perfect cure, (at best only problematical;) and the animal is put out of the way. Can this be adduced as a proof that fractures of bones are of rare occurrence among animals? Nations and tribes who have to defend themselves against enemies, and constantly to busy themselves about the procuring of their subsistence cannot pay the necessary attention to persons affected with chronic diseases. Mere kindness is soon exhausted; the impulse of self-preservation overcomes, in the long run, even the natural sentiments of near consanguinity, and a stupified indifference leaves the victim to its fate.

In a community where every one needing sustenance must necessarily seek for food, or earn food, those alienated in mind, who are unable to share in any work, but rather impede every useful undertaking, cannot be much taken care of. Lunatics are like persons in a state of trance, whose revivification can only result through a continuous personal self-denial. If such persons are left to their fate, under all sorts of privations, they must soon succumb; they cannot, therefore, so often meet our eye as with us, where everything is done for their preservation and comfort. We shall hardly, therefore, be tempted to regard the life of any nation or tribe, which approaches the so-called state of nature, as an object of envy to civilized man.

But, in order to place the true influence of civilization on the physical weal of man in the proper light, it is not enough to prove that the increase of disease caused by it and through it, is in some degree only apparent; we must also concede at once, that there is a great number of morbid causes contingent on civilization, which, although they are necessarily connected with it, still are capable of being mitigated, neutralized, or removed by it: as in the Grecian fable of the spear, the same thing both kills and cures. One of these causes—although one perhaps not yet fully recognized—is the remarkable fact, that while the populousness of most countries goes on continually increasing, the mortality of individuals, on the other hand, is continually decreasing. The longer lives of the present generation, compared with those who preceded it, and the diminished risk from the diseases of infancy and youth, have left more room for disease in the enlarged subsequent period of life.

A consequence, however, of increased civilization, scarcely to be avoided, is, that through the improvement of mechanical art, and the greater number of manufactories, the mass of working men who possess no fixed property (land), is also on the increase, by which the contrast between comfortable ease and hopeless misery becomes the more apparent. Is it not to be supposed that the pauperism of the working classes—which St. Simonists, Fourrierists, Socialists, and Communists have sought in vain to remedy—is a never-failing source of disease? So is it not to be supposed as well, that the use of ardent spirits, (once counted amongst drugs, and not used as a popular beverage before the beginning of the sixteenth century,) but now so much misapplied, may be alone sufficient to account for the greater number of sick, compared with former times? Although both these queries must be answered in the affirmative, yet it can be also proved, that along with these unavoidable drawbacks of a highly-developed civilization, the means to obviate them and to confine them in a narrower channel, also become manifest and multiplied.

One source of agencies leading to disease seems to lie in the very development of the human mind. The more varied and the more powerfully the mind is brought into action, the more numerous will be the occasions for the morbid misturning [disorder] of the higher parts of the organism. A proof of this may be found in the fact, that the lunatic asylums are everywhere either increased in number or enlarged. This seems to accord with a saying of the old Stagyrite, that men distinguished by talent, either as philosophers, statesmen, or poets, or in other arts, are prone to melancholy; which is corroborated by the statement of a recent Belgian political economist, that it is between the age of forty and fifty, when most of the great works of the human understanding are accomplished, that man is also most subject to mental alienation.

A more accurate investigation into the circumstances of the case, however, will teach us how uncertain and onesided such assertions are, leading to erroneous conclusions. Even the very logical meaning of words is here of great importance. When the ancients called a person melancholy, they did not mean, any more than we do, that the individual was diseased; but wished to express merely that stern retirement within one's self which men of genius feel at times, as if they were compelled by a higher and irresistible agency. Genius, the superior development of mind in any given direction, will scarcely ever of itself lead to disease; and if this should ever chance to be the case, it will be so often reported that it may grow at last into seeming importance by repetition. Men who outstrip their contemporaries by their actions or by other mental manifestations, are considered eccentric and even mad. It would be well if, in such cases, we could interrogate history as to particulars. If we hear that the poet of 'Gerusalemme Liberata' was a lunatic, it would be well to inquire whether this was really the case, and if so, how he was brought into this state. Richly-gifted minds, as well as those most obtuse, may become affected by psychical disease; but with the former it is considered a phenomenon, with the latter an every day's occurrence. It is certainly a very hasty conclusion, that the development of all or any individual mental quality can be the cause of mental disturbance or even disorganization. Not culture, but half-culture of the faculties leads to the lunatic asylum. The more numerous and the better the educational institutions of a country, the less the number of lunatics. The more all capabilities of man are brought into action, the more perfectly will all imperfections be overcome. The want of activity will much oftener cause mental disturbance than activity ever can. How seldom is it that men really learned, who work quietly and in measure, are mentally diseased! It is not the exertion of the mental faculties and the strenuous efforts for the achievement of the highest aims of existence which will disorder the higher mental powers; but rather passions and the accidents of fortune, against which elevation of the mind is the surest remedy. If, therefore, it has been stated by high authority, that the progress of so-called civilization increases the number of suicides, it is only the spurious, not the genuine civilization which is to be blamed. The latter will even teach us that the aim of life does not consist merely in earthly enjoyment, and that it is our bounden duty to bear heavy visitations with resignation.

Whether the number of lunatics is really on the increase, compared with former times, on account of greater civilization, cannot be ascertained, as no, or at least very incomplete, numerical accounts were kept at former periods. Even the most recent reports of lunatic asylums are not entirely to be depended upon. As little as the court fools of the middle ages were real fools, as little do all actually-reported lunatics belong to that category. Oxford, who discharged pistols at the queen, and is now confined in Bedlam, is certainly not a lunatic. Formerly such unfortunate persons were kept secluded in private houses, partly for the sake of keeping such family afflictions secret from the public gaze, partly in order to avoid any extraneous interference. Now they are generally sent to institutions of that kind. Asylums formerly were employed for safe keeping, now for cure. Insane persons, in past times, were only attended to at the end of their malady, or on the occurrence of some imminent danger from them; now proper treatment is applied at the very outset of the malady. For all these reasons, more spacious and additional buildings have become necessary for the accommodation of lunatics.

So far, therefore, from civilization being justly considered as a nursery of mental disease, it stands forth, as far as their cure is concerned, as the advocate and promoter of the purest humanity. Never has sympathy or the devoting forgetfulness of self burst forth into finer bloom than in many of the present institutions for mental alienation. The further we advance in the knowledge of maladies of this sort, the more forms thereof we become acquainted with. But it does not follow thence that such did not previously exist. On the contrary, some varieties of mental disease which existed in former times have already disappeared, or are on the point of disappearing. One species of lunacy, viz. *lykantrophy*, has really ceased. In the third and fourth centuries maniacs were seen prowling and howling about on graves, and other solitary places, like wolves; and they were rather numerous in several parts of Europe.

That form of innate imbecility called cretinism, which seemed hitherto to withstand any attempt of cure, has been alleviated and even cured now-a-days, by a successful combination of medical art and philanthropic expedients.

Animals were once called "the mutes of nature," out of compassion; the term implying that the organ of communicativeness was denied to them. It is only at a recent period that notice has been taken of those most unfortunate people to whom nature has denied the use of hearing, and thence prevented the use and development of their organs of speech. Philanthropists and instructors and medical men have united in promoting inquiries into the causes of dumbness and deafness, according to certain localities, and as well into their varieties, and have exerted themselves in making the lot of such persons as comfortable as possible. Formerly the deaf and dumb were a great burden on human society. Not taking into account those few who, by otherwise favorable circumstances, were permitted to attain some degree of moral and social independence, the remainder, if left to their own helplessness or the brutality or stupidity of those who commonly surrounded them, became objects, not only of compassion, but even of terror. How differently stand things now with this class of persons, brought up and educated as they are in public institutions! Here, by being instructed in reading and writing, the power of comprehending and mingling almost on equal terms with the remainder of society is opened to them; a substitute being supplied to the hitherto closed up and unyielding organs of speech and hearing! Be the causes of this abnormal state what they will, it is certain, that civilization has no share in their production, while it is from it alone that their only possible alleviation is proceeding.

The same may be said of all institutions for the reception of the blind, deformed, crippled, lame, &c., whom an increased knowledge and experience teach us to heal or to relieve. Evils of this kind have always existed, but in past times they were only productive of pain and care, both to the individual and to society at large.

In the same way as the *cure* of bodily ailments is attended to, so also pains are taken for their systematic *prevention;* and it is really not the fault of civilization, if the primordial germs of malady are continually sending forth fresh branches.

It would be easy to corroborate the foregoing assertions, *seriatim*, but this examination would lead us here too far. But the enumeration of some of the most important efforts against the spreading of maladies—efforts owing to the advanced state of civilization—will convince us that this decrease is not owing to any accidental epidemic influences, or to periodical cycles, but solely to the strenuous efforts of the human mind. It will show, at the same time, the constant manifestation of efforts either to limit the causes of maladies, or to eradicate them altogether, their progress being opposed at every step.

In turning our regards backwards, we shall find that each succeeding age, every advance in civilization, is marked by increased attention to the physical condition of children, and, consequently, by their decreased mortality. This attention begins even before they are born. How much more do we now (compared with former times) pay attention to the difference between the natural and artificial indications during the act of parturition, and thus by a well-calculated, timely aid, save the life both of mother and child. As the case may require, a healthy wet-nurse is procured, or the child brought up, at least, in an appropriate manner.

Much is done towards the preventing infanticide, whether intentional or that which may be caused by the want of proper knowledge. Orphans, or otherwise friendless children, are sent to the country to be nursed, not only for the purpose of avoiding their being placed in foundling-houses, but also for the reason, that in the first period of life the mortality of children is greater in towns than in the country.

Education tends now as much towards the developing of the physical as of the mental faculties; and if there be any hereditary disease, care is taken to check the elements of its growth by an adequate mode of life.

Our mode of dressing is more appropriate than formerly, and such garments which prevent, by their pressure, the free action of the organs, fall every day more into disuse. The less common use, or the better construction of stays, has done away with many a complaint. The same observation applies to our cropped and cleanly-kept hair. Dentition is scarcely ever mentioned amongst the diseases of infants, since the too warm keeping of the head has been abandoned.

The conviction of the necessity of dietetic and gymnastic invigoration of the body, as well for youth as those more advanced in age, is gaining ground every day. The means for accomplishing this desirable end, which heretofore was only within the reach of some classes of society, or was peculiar to certain races of men, are well-nigh becoming the common property of all nations.

Considering the important influence which the skin exercises on the most important functions of the body, the habits of cleanliness, which have now become a custom, contribute powerfully towards the preservation of health. It has been much aided by the universal use of soap. It is with perfect truth that cleanliness is considered one of the most valuable gifts of civilization.

With the increased knowledge of the conditions requisite for general welfare, the endeavours for maintaining these, and carrying them out, have kept equal pace. That weighty saying, "what maintains health, maintains wealth," is daily corroborated by manifold experience. Almost everywhere great efforts are made to keep the air pure and sweet, by the laying out of broad streets, forming sewers, and the removal of cemeteries from the vicinity of the habitations of men. Since the general peace of nations is now firmly established throughout Europe, high bastions may well cede their place to a free access of air, and narrow confining walls, and malarious ditches, be changed into public walks.

The supply of healthy food is so much facilitated, that diseases which formerly spread over vast tracts of land, on account of its deficiency, are now nearly unheard of. The scientific improvement of agriculture has made all sorts of soil available, and the amount of field produce has increased. The abundance of potatoes and fruit alone is sufficient to prevent scarcity, or real want.

The adulterations of nutritious substances, the glazing of culinary vessels in a manner deleterious to health, the imperfect tinning of kitchen or other economical utensils, are becoming every day more rare. Cases of accidental poisoning decrease every day in number, by the progressive destruction of venomous herbs, or by their character becoming better known. The sale of poisons is subjected to rigorous control, and the vending of patent medicines (*Olitäten krämer*) entirely prohibited [in some countries]. If a case of poisoning occurs, efficacious antidotes, the result of experiment and research, are at hand. From the high degree of development which chemical art has attained, the ascertaining of any poison introduced into the body is a task so sure and easy, that all premeditated poisoning is easily ascertained, and its repetition made more difficult.

Active and efficient relief of the poor is carried everywhere into effect. The distributing of warm clothes and fuel in the winter season, the numerous societies for providing convalescents with adequate food, the institutions for destitute infant children—all branches of the one charitable feeling towards the poor—contribute powerfully towards the preservation of life and health in the lower classes. The establishment of poor-colonies (home-colonies), tried lately in several countries, bids fair for a permanent improvement of our social state, partly by relieving the community from an onerous surplus of population, partly by the increased happiness of thousands of needy people, and partly by bringing into cultivation tracts of land, some of which would otherwise remain a constant source of deleterious effluvia.

Those arrangements whereby prisons become not merely places of punishment, but of reformation, are constantly more and more extending themselves; consequently a great number of people are not only physically but morally preserved. The merits or demerits of the several methods of treating prisoners are by no means yet perfectly ascertained. Still, the time is not distant, when those philanthropists interested in this weighty object, will agree on some final plan proved to be the best. If the fact, that the American penitentiary system is often followed by aberration of mind should become confirmed, it will, of course, prevent its adoption.

The generally milder character of punishments of the present age, cannot fail to preserve the health of those who have subjected themselves to such treatment. It is only from tradition, as it were, that we know in civilized states of organic defects or mutilations having followed brutal punishments.

In the military service, also, a humane treatment is gradually superseding the old strenuous severity. It is not merely attention to cleanliness and sound food, but also a humane treatment, which can prevent disease of the common soldier; so much so, that the saying is not far from truth, that "a good commanding officer has generally a healthy troop." To the establishment and the improvement of military and field hospitals, thousands owe the preservation of life and limb.

Since care has been taken that not too many persons are crowded on board of ship, that the crew are provided with a sufficient supply of linen, and that instead of the old air-infecting ballast, blocks or rather tanks of iron (containing the supply of water) are had recourse to, the salubrity of vessels has made astonishing progress.

The special investigation of the diseases of artists and artisans has greatly tended to ascertain the hidden sources of their ailments, as well as the ways and means for removing them. How many owe to the drawing-oven of D'Arcet, or the safety-lamp of Humphry Davy, their preservation from lingering disease, or even death! Many operations which formerly, from the unnatural position and exhausting toil required in them, destroyed the health of the workman, are now accomplished by means of machinery.

The fact that the mortality is greater in manufacturing countries and districts, than with people who live in the country, seems to be partly based on the circumstance that the sources of employment do not flow steadily in the former, and that not seldom the flood-tide of enjoyment is followed by the protracted ebb of privation. The beneficial effects of savings'-banks, every day more and more admitted, will eventually succeed in obviating this inconvenience.

Even the nursing of the sick in hospitals and private dwellings, inasmuch as this now demands as an indispensable requirement, the proper separation of the patients, free circulation of air and cleanliness, has thereby elevated itself into an important branch of the sanitary art. Equally beneficial in that respect, as well as productive of general good, and indispensable for obtaining scientific results, was the establishment of separate institutions for sick children, for incurables, for those affected with skin diseases (*Krätzige*), syphilis, epilepsy, &c., together with separate wards for convalescents, especially those labouring under contagious and mental diseases.

The preservation of those who had received severe injuries, as well as the asphixiated, did not remain dependent merely on the sympathising feelings and the skill of individuals; public solicitude exerted itself in this behalf. Prizes were given to those who had saved the life of others, as well as to those composing the best essays on the means of doing so; instructions also for treating such cases, intelligible to common persons, were distributed, and societies were formed for their relief, which took the exclusive appellation of "humane."

Contagious diseases are repressed in the same proportion as the authorities carry out preventive measures: all suspected articles undergo several processes of purification, such as exposure to air, washing, calefaction, and often they are even burnt altogether. The discovery of the preparations of chlorine has afforded a strong means for checking putrefaction and infection.

A more careful study of veterinary medicine has taught us what animal contagions can be communicated to man: and the knowledge of these facts points out the necessary care, and the means which secure man against them. The often repeated muster of dogs, the speedy removal of all which are suspected, and the taxes which have been laid upon them, with the view of leading towards their decrease,—all these expedients have been so efficacious, that for several years past no case of real hydrophobia has come under our observation.

The eradication of endemic agencies has been much advanced by medical topography. It points out the influence which the soil and climate in general exercise on the inhabitants of various localities. Wherever man penetrates with his peaceful arts and occupations, and his free institutions, bogs, dense forests, and with them the concomitant cold and damp, will vanish. But in the same ratio also in which the commercial and political importance of a country are declining, and industry and population diminished, the malaria, and especially the marsh-miasma, will increase.

The great share which scientific discussions on sanitary subjects has had in the preservation of health is not to be passed over in silence. In the same category comes the publication of good popular works, now issuing from the press in several countries of Europe, especially in England, where they are composed by first-rate men, and sold at most moderate prices. The opposition to prejudices of every kind, be it by oral discourse or by the public prints, gains every day more ground. The more we find that public health is encroached upon by prejudices and abuses, which are based either in ignorance or selfishness, the more unrelaxed ought to be our opposition. As long as the opinion prevailed that the burial in churches and chapels, under the immediate protection of saints and martyrs, led to salvation, the faithful were doomed to inhale the vapours of graves.

That the burning of widows is no necessary proof of affection has been duly acknowledged by the English in India, and they have prohibited it, as well for reasons of humanity as of sound sense.

There were times when either all or some diseases were considered as punishments or visitations sent by God, to which man had to submit without resistance. These false notions have, however, long passed away; or if such a dogma of predestination is still in force in some countries, it is to be hoped that it will soon vanish before the light of reason. As in this case from *religious* scruples, so in the case of vaccination arguments founded on a basis of *humanity* were advanced against that most beneficial measure: it was then considered improper to convey to the human body an animal virus, and thus to exchange the dangerous human varioli for the innocuous form of the cow virus. Nowadays such things are smiled at.

From a misunderstood delicacy in difficult deliveries, the right moment for relieving both mother and child was formerly neglected; but now, since the repugnance of the female sex against male interference has been overcome, and proper aid being now called in at the right period, many a woman and her offspring are saved.

Many habits and customs, connected with sanitary laws, do not assume their legitimate and natural bearing but by slow degrees. The practice of keeping the patient warm, especially in febrile exanthematic diseases, in scarlatina, and miliary fevers, and even in measles, was pushed to such an extreme, that it became a true calamity. This air-repugnance, if we may so term it, is now vanquished; nurseries and sick rooms are ventilated; no one now forbears to carry children into the fresh air, to wash invalids with cold water, or even to pour it over them; and if the new system of hydropathy has carried its precepts too far, still even this lapse into extremes is on that side where danger is least, and the return into the right path nearest at hand.

As the spread of genuine civilization is capable of stopping the increase of disease, so is it also with the advance of morality. The shutting up, for instance, of gambling houses, stops also a considerable source of morbific agencies.

Philanthropic societies accomplish, by their wide-spread activity, not only the high aims of civil and moral improvement, but also increase health and prolong life. Who ever can mistake the blissful effects of temperance societies, even conceding that their activity is much crippled in many countries? The command which the man of cultivated mind can exercise over himself, by the means of principle and firm will, must be impressed upon coarser minds by example, at times even by a sort of monastic vow. Temperance, however, is the chief basis of all physical prosperity, the foundation and preservative of every human blessing. If modern time can succeed in erecting everywhere temples to that divinity, it will have fulfilled a high vocation. The reason why more *men* die in towns is, because their life is, in the main, more disorderly.

Life-insurance companies, and all such arrangements whereby that which we have acquired or saved can be deposited for the time of want, advance, in so far, the physical welfare, as they protect us against the harassing and corroding vicissitudes of life.

If the fact be true, that the prosperity of the lower classes is increasing, it will not fail to exercise its influence also on their general physical well-being. Many a child of the poor, which looks well at first, dwindles away gradually, verifying the opinion, that in proportion as misery decreases, more infants are reared up to adult life. The more roomy dwellings, better protected against cold and wet, as well as better clothing and food, will keep off many a bodily ailment, especially where infection is to be apprehended. How many diseases are to be met with in the hovels of misery and privation! Blindness is most common with the poorest classes, as it chiefly originates in neglect and want of proper management.

The habit of being much in the open air, and the consequent hardening of the body, contributes much towards the preservation and prolongation of life; but if this condition be accompanied with severe labour, it will lose a great deal of its benign influence. Hence it comes, that the so-called simple states of life do not prove themselves so beneficial in this respect, as those of more civilized nations. In like manner, we know, that more women die during the time of their pregnancy in the country than in the towns. It may be, that this is at times caused by the want of proper help, but still more we think it is owing to the necessity women are there under of doing hard work at a period which demands great quiet. Considered, however, generally, we know there is less mortality in the country than in large towns, and that some sorts of maladies are rarer there; but the cause of the difference is not to be sought for so much in the production of noxious airs, consequent on the concentration of so many people, but rather in the fact that all agencies tending to the disturbance of health and life are there more copious. The more, however, these agencies are acknowledged as such, the greater will be the care of all well-informed men, and of philanthropists and the public authorities, to diminish them.

The mortality amongst the higher classes, compared with the lower, is not only less, because the former live in plenty, and the others in want, but also because the former are accustomed to cleanliness and temperance, are less excited by low passions, and less subject to sudden changes of their circumstances. It is remarkable that in England, where no doubt there is the greatest amount of general material well-being, the average duration of human life is also the greatest, viz. thirty-eight years, whilst in Russia it is only twenty-one. The rich, therefore, do not only live better, but longer than the poor.

But even the less opulent now possesses more means to restore his impaired health. It is not the least of the merits of medicine, that more efficacious and cheaper remedies are used now than at a former period. Before the Peruvian bark was discovered, how long had an artisan (exposed to the inclemency of the weather, and other noxious influences,) to suffer from intermittent fever and its consequences, and how quickly is he now restored to his occupation, more especially since the discovery of the alkaloids! Formerly, if an artisan had contracted a lameness of his arms by inhaling the vapour of lead, he remained for a long time a burthen to his family, and under the most favorable circumstances was obliged to consume, during his forced inaction, what he might have saved at a previous period. Now, by the aid of strychnine, and sulphur-baths, the surgeon speedily dismisses him, freed from pain and spasm, to his former occupation: wherefore it is that the present higher degree of medical skill in the use of simpler and surer modes of cure, the result of general instruction and an improved civilization, constitutes an important item among the causes of the decrease of maladies. Medical arrangements in general have acquired, almost everywhere, a reputable degree of development. For instance, natural and artificial baths are within the reach of every one who is in want of them. In consequence of improved communications, efficient drugs are now brought from all quarters of the globe at a moderate rate; others have been discovered by the exertions of our chemists. On this account, the number of incurable or painful chronic diseases is constantly more confined.

Through means of improved diagnostics, as well as of improved treatment both in medicine and surgery, more individuals of all ages are now preserved than at any former period. Even at the beginning of the present century the inflammations of the cerebral membrane, of the lungs, and of the intestinal canal of children, were for the most part mistaken, and therefore terminated unfavorably. The mode of cure of syphilis and its consequences was formerly as deleterious for the patient as the disease itself. Aneurism, which formerly was mostly fatal, is now successfully removed under the hands of skilful operators, by means of the ligature of large branches of arteries. Distortions of the limbs, which formerly—for instance, club-foot—impeded their use, are now removed by early and skilful operation. Squinting, which prevented many from improving their condition, is now as easily cured by a simple incision; so also is stammering, by systematic exercises in the mode of speaking. How many blind persons were once considered incurable, to whom sight is now restored in a moment!

It may not be urged as an argument against all we have hitherto stated, that new names of diseases are every day starting up,—proving, as is alleged, their increase. But names are not entities. As little as a botanist increases the botanical riches of a country by making new species and genera from mere varieties, as little does a nosologist, who transforms mere symptoms into forms of disease, prove the distinct existence of such forms. Fortunately, the number of diseases has not increased in reality, but only on paper; it is merely the modes of classification, and not the ailments themselves, which have been multiplied.

It often depends merely on the ideas received at college, or on certain fashionable notions of the faculty, whether or not some diseases are considered as epidemic and far spread. Thus, at one time we hear everywhere of affections of the heart, at another of inflammations of the abdomen, of the dorsal spine, &c.; whence it might appear as if heavy visitations were afflicting mankind in an extraordinary degree. Soon, however, this notion will be found illusory, and everything will return to the usual course of disease and recovery.

But even supposing that any particular disease be observed more frequently, where is the proof that it has really become more frequent? Among savage nations, a great many of the sickly offspring die in the first years of life; among civilized people, on the other hand, they are mostly preserved, and only at an advanced age fall a prey to natural or accidental death. Can it, then, be justly said that a disease has increased, merely because a greater number of persons perish by it from the circumstance of their attaining a higher age? Civilization has only the power of diminishing or preventing the tendencies to disease; it cannot confer on man physical immortality! Considering the greater amount of vital powers and vital excitement among civilized people, the victory over the many dangers which threaten them, is the more glorious.

As a proof of the justice of the foregoing statements, we might adduce the historical or statistical data of almost every disease. We select, in the first instance, some of the principal.

Consumption. It has often been asserted, that pulmonary phthisis—the great destroyer of the human kind, and that, too, in the very prime of life, when mind and body are full grown and developed—has increased in these times, compared with former times: but this position we cannot admit. Comparative tables, which alone can lead to the solution of such a question, do not yet exist, even in a form which may be deemed approximatively correct, and cannot, therefore, be depended upon. One of the most recent authors, (Sir James Clark,) who has given a statement of those who have died in London from that disease, from the year 1700 to 1821, says: "The opinion entertained by some authors, that phthisis has increased since the year 1750, is based on the error, that its relative fatality has been placed in comparison with that of all other diseases, instead of comparing its absolute fatality with the general amount of population. That relative increase is not occasioned by the increase of phthisis, but by the decrease of other morbid affections: the causes which have had such a beneficial influence on the mortality arising from other diseases, have passed away without producing any influence on phthisis." Also, if we compare the statistical tables for a number of years, compiled in countries where they have been made out with care, (for instance, in Würtemburg,) we shall find, not an increase, but a decrease of this disease. Since the year 1787 a smaller number have died annually of phthisis at Stuttgart. During five years only *three* individuals have been exempted on that account from military service. In some districts there was not one person affected with this disease.

The ancients speak often of phthisis, and mention its hereditary and even contagious characters. But just as we do not confine the term consumption to the pulmonary affection, they also used the word phthisis in a wider acceptation. Some countries are little favorable for the development of this disease, owing to their position and endemic circumstances; for instance, Egypt: and it was stated, even very recently, that the disease scarcely occurs there at all. It has also been stated, that in countries where aqueous exhalations caused intermittent fevers, phthisis is little known.

Admitting that, under similar external circumstances, phthisis makes its appearance equally among civilized and savage nations, the mortality among the latter will doubtless be much greater; as we know that, without a very careful regimen of life and appropriate professional aid, it will be soon accompanied by inflammation, and then degenerate into florid consumption, or will be soon followed by a softening of the pulmonary tissue, and all the symptoms of tubercular decline.

Since the deleterious influence of such trades as affect the lungs—as, for instance, that of grinders, brushmakers, &c.—and since the evil effects of mercurial vapours, and the consequences of the too great use of mercurial ointment have been properly understood, and means taken against them, phthisis has been often prevented. And as medical investigation has been so strongly directed to the nature and cause of the deposition of tuberculous matter in the lungs, we may still entertain a hope, that that morbid almost inorganic mass may be checked in its growth, and be made capable of absorption and excretion.

In conclusion, we ought to observe, that the deductions derived from tables of mortality (although they are at present the only data to go by,) are to be accepted with caution; because, when we find it stated that a patient died of pectoral disease, whether chronic catarrh, a spitting of blood, &c., it remains still doubtful whether the disease was phthisis or not.

Scrofula. This malady, which especially affects young persons, may certainly be considered rather on the decrease,than otherwise, as, by the present improved circumstances relating to the dwellings of man, the elements of its formation become less numerous. By the greater attention paid to the cleanliness of the skin, and to the mucous membrane of the intestines, which is much facilitated by the greater cheapness of digestible food, the disease does not manifest itself so easily, or if it does so, it is checked before it has attained any degree of development, by abstaining from food containing gluten, (Hänkmehl,) by exercise in the open air, by dwelling in a pure atmosphere, avoiding of damp habitations, and by the use of cool baths.

The old physicians have described this disease very well, but their way of treating it was not always the most judicious: they considered it a mere external one, and endeavoured to bring the swellings to suppuration. Out of all the numerous anti-scrofulous modes of cure and eulogised remedies presumed specific, recourse has finally been had to diet—certainly a great improvement.

Rickets. This disease, formerly described by the Arabians under the name of hunchback, and arising from fever, as observed, during the sixteenth century, in

Holland and Switzerland, and which was so well described from observations made in England in the seventeenth century, that it received in Germany the name of "English disease," becomes less prevalent every year, from the same causes as have been stated under the head of Scrofula. The so-called *double limbs* (doppelten Glieder) and other distortions, now appear in less number; and if they are more talked of, and even especial institutions established for their cure, it is to be recollected, that the increased number of journals calls forth many detailed descriptions of subjects which formerly were passed over in silence. Besides, the results of former methods are now regarded as insufficient: it is not only health we now seek for; beauty and strength are desiderated in addition.

Syphilis. Among the most awful diseases which have long afflicted mankind, and still continue to do so to a certain degree, syphilis occupies a prominent place. I shall not enter here upon the question, whether it was known to antiquity, and whether it also occurs in animals; suffice it to say, that when it made its first appearance, some centuries back, it soon spread with great rapidity, and was followed by a number of accessory maladies, unlike any other ailment to which flesh is heir: since then, its baneful sway has successively extended over the whole inhabited globe. At its first advent, the guilty, as well as the innocent, were seized by it: the harmless babe—nay, the unborn embryo—was not secure from its poisoning influences. All the remedies which the then rude and dark ages taxed for checking it, proved useless, or even increased the evil; and thus it seemed, that this ominous ailment—affecting as it did the very sources of propagation and increase—even menaced the existence of the whole race. The disease has not been extirpated: it still exists, but civilization and science have much curtailed its extent, its intensity, and its consequences. In the same ratio as the knowledge of its essential relations to the organism and the remedies employed against it have increased, so also has the morality of states and nations improved; and it would be easy to prove how both these causes have contributed towards the checking of its progress, as well as mitigating its symptoms and complications; and how it has finally become restricted within rather narrow limits. It cannot be denied, that that very remedy which was considered, for a long time, indispensable and unavoidable, was not less detrimental to health than the disease itself. But we now know, full well, that the untoward symptoms alluded to were not so much to be attributed to the use as to the abuse of this remedy; so much so, that the conviction of our being able entirely to dispense with it is becoming every day more general. As syphilis is much milder in warm climates, (for instance, in Egypt,) so much so as to be curable by nature, it was imagined that energetic therapeutic measures were required in our northern latitudes. But in proportion as it is acknowledged that the affections of the cutis and the mucous membranes comprehend the chief therapeutic indications, and that a radical cure can be effected without the use of mercury, the character of the disease has become more simple, and the organism less subject to secondary ailments.

The better knowledge of syphilitic affections, together with that of the disease called Marsch-krankheit in Holstein, the radesyge and spetälska in Scandinavia, the sibbens in Scotland, the skerlievo on the shores of the Adriatic, the Krimmean disease (krimmschen-krankheit) on those of the Black Sea, the Asturian rose, the eruptive disease of Aleppo, the yaws and pians, makes us often believe in an affinity between syphilis and lepra, or, at least, doubt of the pure specific character of the former. This disease, therefore, formerly so terrible and mysterious, has now come within the range of ordinary maladies, being not only amenable to art, but vincible by art in every respect. It is with some degree of confidence that the philanthropist may expect its gradual decrease and disappearance, at a period not far off; if those authorities to whom devolve the superintendence and promotion of the general health, as well as the maintaining of public morals, will not relax in their exertions.

In like manner as the above three malignant and most extended plagues of mankind are not only not ascribable to the progress of civilization, but are, on the contrary, combated by it with all possible might, so also is the case with almost all other ailments; inasmuch as the representatives of civilization (we mean science, art, and morals,) ever oppose them, either in an open or indirect manner. For the sake of placing this matter in a clear light, it will suffice shortly to illustrate some more prominent forms from the different departments of pathology.

Nervous diseases. The civilization of the ancients had attained such a high pitch, that we view the remnants of their architecture and sculpture—we regard their poets and historians with astonishment and admiration, as the monuments of some finer world, now passed away! The economy of their public and private life comprehended such a chequered tissue of manifold arts and enjoyments, that the reproaches made against our own age, stigmatizing it as one of too much refinement, are perhaps unfounded. But even supposing that modern civilization does not possess the simple grandeur of ancient thought, and has merely adopted the superfluous wants, the appetites and passions of the olden time, or only combined these with the views and notions of a newer order of things, still it is striking that, in the department of *nervous diseases*, no new forms of these anomalies of over-excitation are to be met with. Nay, the circle of nervous diseases has not only not become wider, compared with former times, but has been rather restricted. This will appear evident when we compare the writings and traditions of antiquity with the records of the present time.

Nostalgia. Even within the recollection of practitioners still living—a period which, compared with history, is obviously of no account—a dangerous nervous disease, the *nostalgia*, has nearly ceased. It is well known how much this formerly prevailed, especially among the natives of mountainous countries; and how very seldom is it now even heard of! Into the heretofore lonesome valleys of the Highlands a more lively traffic has found its way; the inhabitants have entered into communion with the whole world; and the accelerated communication, by means of stage-coaches, steam-vessels, and railroads, has taken away from the stranger in a foreign land the depressing idea of secluded loneliness and cold isolation; letters from one's home now arrive with astonishing celerity, as if by pigeon-post; the mere idea that the decision of a moment may realize the possibility of being transported, as on the wings of the wind, to the place of our anxious desires, banishes every feeling of hopeless despair.

Hysteria and hypochondriasis. The easy modes of travelling now prevalent, and the constant custom of availing ourselves thereof, are among the chief reasons that hypochondriasis and hysteria are now less frequent than formerly. Travelling is one of the chief preventives of these maladies; not merely from the necessary bodily exercise connected with it, but from the enforced change of the mode of life, from the necessity of entering into foreign feelings, and the unavoidable impulse of new and distracting impressions. In proportion, also, as with less trouble and expense than heretofore, that abode can be chosen which is most suitable both to mind and body, the duration of life is also increased. The increase of strength which the whole constitution obtains, at times, by a change of climate often surpasses belief. The deeper organic anomalies existing in hypochondria and hysteria can also be now the easier remedied, as the organs which are thereby affected—especially those which are connected with the sympathetic nerve—are much better understood than they were formerly; and, consequently, better remedies can be adopted against them. It is proper, moreover, to observe, that whoever should conclude from the infrequency of the terms hysteria and hypochondriasis among ancient authors, that the diseases themselves were proportionally rare, would commit a great error; as, according to the theories of those times, several other appellations have been applied to these diseases; for instance, *dry cholera, the spleen*, &c.

Chorea Sti. Viti. This is one of those nervous diseases which certainly was known formerly, but was comprised with others under other appellations. As it mostly appears in the so-called period of development, its decrease is to be expected with the more certainty as medicine is constantly studying the changes of the human body, and education becoming every day more circumspect. Indeed, cases of the disease are now rather rare.

The dancing mania. This disease was a consequence of the ties of public and private life having been so much loosened, in consequence of the ravages of the *black death.* Many persons were then seized, as if by some contagious plague, with a demoniacal furor to stroll in loose bands through the land, and to dance until they suffered complete exhaustion, which was followed by feigned or real spasmodic affection. With the exception of those who were carried away by the impulse of imitation, the subjects of this delusion were mostly persons who had long lived on public charity; and they persisted until authority had regained its lost influence, and social life assumed a degree of reasonable quiet. In a well-regulated state of society, like the present, the repetition of such an epidemic is scarcely possible. It is true that our present time has exhibited something similar in the *preaching mania* of Smäland, in Sweden; but this strange excitement was soon assuaged by the cooperation of all sensible persons.

Catalepsy (*Starrsucht*). This disease has become rather a subject of history than of nosology, since the doubt of its existence as a real disease has grown into conviction.

Raphania (Kriebel-krankheit), which, on account of its striking symptoms, has been named also "spasmodic tragedy," is only to be met with in districts where no measures are taken against the growth of ergot in corn. The simplest remedy is to dry the land by draining. In some countries the government issues public notifications how to guard against the bad effects of the corn, which might have been spoiled in wet seasons by this poisonous fungus.

Paralysis. Palsy, more especially of the lower extremities, is a disease so frequently observed in modern times, that it cannot be denied, that it has numerically increased. As it is, however, very probable that the recent great political revolutions, with their concomitant excitement and disasters which befel so many persons, as well as the hardships of war, have a great share in its increase, we may presume, that the disease will again decrease in frequency during a lasting peace and a consolidated social condition. This, certainly, will be much assisted by the recent discoveries as to the function of the dorsal nerves, as well as the knowledge of the effect of some vegetable alkaloids.

Neuralgia. Nervous pains in general, and especially tic douloureux and angina pectoris, have been much noticed since the beginning of this century. Still it is difficult to say anything positive about the alleged increase of this disease, as its existence previously is not properly elucidated. The Arabian physicians speak often of painful spasms of the face; and one of them even recommended to apply derivatives near the origin of the nerves. Several of the great men of antiquity are said to have suffered from the angina pectoris.

Hydrophobia. This disease is now not known in many countries but by hearsay, whilst in others it still claims its share of victims. As art is here without avail when the disease has become somewhat developed, every care is to be taken to prevent it. The modes of doing this are various, and have proved efficacious in so many instances, that we may cherish the hope, that if generally resorted to and thoroughly carried out, they will finally eradicate the evil. Considering the fact, that house-dogs are those which usually run mad, several governments have imposed a tax, with the view of diminishing their number. At Argos a feast was celebrated in ancient times, in the dog-days, called *kynaphontis*, at which a number of dogs were publicly killed. And it is to this expedient that we shall be obliged to resort if we do not like to be taxed, or if taxation does not prevent the keeping of too many useless dogs.

Delirium tremens. It is probable that this affection has only become a distinct form of disease since the mode of distilling ardent spirits has been so much improved, that large quantities can be had at a moderate price—a circumstance which so much encourages intemperate habits. This, however, has reached its climax. The general disgust, temperance associations, as well as the improved therapeutic indication, to remove the morbid state of the patient by inducing sleep, are all directed towards the same aim.

The gilders' malady (Zittern der Vergolder), which renders invalid so many persons obliged to inhale mercurial vapours, and which often ends in complete paralysis, has lost much of its awful character by the knowledge of the salubrious effects of chalybeates, taken either internally or applied in the shape of baths. The newly-invented method, by which almost all metals can be gilt by galvanic process in a substantial and solid manner, will probably lead to a perfect abandoning of the pernicious quicksilver.

Colica pictorum. This disease is also a marked proof of the present progress of medicine. Persons affected with this disease are now generally cured by an appropriate alterative method. It was chiefly owing to the practice of the Romans, to inspissate the juice of grapes in leaden vessels, and to preserve their wine by the admixture of this syrup, that they were so much subject to colic. And it is on account of lead being now more cautiously used for the domestic purposes of glazing and tinning vessels, as well as in the fabrication of oxide of lead, and even for medicinal purposes, that we are to attribute the diminution of the causes and, consequently, of the effects of poisoning by lead. And the same happy result would be seen in the case of painters, if they would resolve to use the oxide of zinc instead of that of lead.

Congestions. As often as the unavoidable evils which extended and refined civilization has brought on are spoken of, the congestive diseases are always mentioned; for instance, the disordinate tendency of blood towards the head, chest, and abdomen, caused by unnatural positions, peculiar movements of the body in working in secluded rooms, also by improper garments and too stimulant food or medicine. It is more especially the hemorrhoids which are counted amongst these artificial diseases. Although it may be the case that the mucous membranes and the vessels of the digestive organs are more affected with us, on account of our social enjoyments, than they are in a natural state; still, even in the latter state, the disease is to be met with, as a self-aid of nature, which thus endeavours to produce a derivation from more noble organs. They terminate seldom fatally, and are mostly the consequence of an inappropriate and irregular mode of life, coupled with the neglect to remove the disposition and to repress the early growth of symptoms. The hemorrhoids have not what may be called any morbid seed in the human body which must necessarily develop itself; it lies mostly in the free will and conduct of the individual, whether he shall be thus affected or not.

Inflammation. Inflammatory irritations and real inflammations have mostly on that account increased, as well numerically as in intensity, because civilization has extended from the milder Asiatic and South-European climate to more northern latitudes. So soon as man, with enlarged social and physical wants, comes in contact with a more severe climate, it follows of course that he must experience the full influence of its inimical agencies. Where change of temperature takes place suddenly, and sharp gales frequently displace more genial breezes; where a humid atmosphere and thick and heavy fogs prevent the access of the milder rays of the sun—more occasion will be afforded for the formation of rheumatism, measles, and catarrh. But even in this case it belongs to a state of civilization, with its many resources,—such as appropriate clothing, warm dwellings, due exercise, increased strength of constitution, and an invigorating mental activity,—to wage successful war with the influences of the climate, and even to achieve more than the rude son of nature, merged in his stupid inactivity, can do, viz., just to manage to subsist on the extremest limits of the habitable earth.

Ophthalmia. The Egyptian inflammation of the eyes, which has become so prevalent in the present century, is nothing else but an intensive catarrhal inflammation, the discharged phlegm being of a corroding and, if the disease has reached its height, of a contagious character. The ancients were acquainted with it, and its spreading is not to be apprehended, if its contagious character is properly taken notice of.

Croup. This disease, which only a few years past, was the terror of all parents, when viewed in reference to its peculiar intensity and frequency, may be regarded as a produce of recent times—a sad result of our social condition and the physical education of our youth. The employment of emetics early in the disease has proved how it can be checked with perfect success; and, consequently, it can no longer be considered as one of the ominous satellites of our times, whatever may be its origin or nature.

The men of civilized nations, as they feed better, are more subject to pure inflammation than those of uncivilized, because they are more plethoric and excitable, and, in the main, undergo greater toils and perils.

Fever. Fevers, both inflammatory and nervous, have lost much of their dangerous and fatal characters, since we have advanced more in our knowledge of them, and since their prevention has been more attended to; those already existing have been treated with more skill, and those which threatened to be contagious have been skilfully prevented. The worst forms of fever are not everywhere the same; some countries are entirely free from them, whilst others are ravaged by them most severely. In the latter case the merit of checking them is so much the greater. It is said that Egypt and India can neither generate nor propagate typhus. How different is the case with Ireland! But it is in this last instance shown most clearly what marvels the exertions of medical men can accomplish when they are assisted by the authorities, and even by the people themselves. It is difficult to ascertain why certain periods and countries remain free from this disease, while others are severely suffering under its affliction. Civilization certainly has no share in its production, but strenuously assists in checking it. The ancients speak only of malignant nervous fevers. The slow nervous fever was well known to the Arabs, who treated it with cooling medicines. Afterwards, this form of disease was but little noticed; and even medical men of our times assert that it did not make its appearance before the close of the last century. The petechial typhus, accompanied by typhomania, has much decreased, in comparison with former times. The English sweating-fever, which did much mischief in the fifteenth and sixteenth centuries, has long ago ceased. The Hungarian fever, which made its appearance in the shape of a nervous putrid fever, and in the camps, and which was accompanied by the most violent colics, and spread contagiously, is now only known historically.

The plague. The Oriental plague, which, under the name of *black death*, spread, in the fourteenth century, over the inhabited globe, like some winged demon, and made those horrid ravages in Vienna and London in the middle of the seventeenth century, and destroyed in the eighteenth nearly one third of the inhabitants of the country of Brandenburg, and in Dantzig nearly one half, and which, even in our days, is constantly penetrating into Europe wherever isolating measures are not properly kept up,—is one of the most striking examples of the victory of civilization over barbarism. Whilst Mahomedan nations look on indifferently at the uninterrupted spreading of this plague amongst them—its destroying fire, scarcely put under, anon reviving from its ashes—whilst they consider it as an unavoidable destiny, when their nearest relatives die away in rapid succession—whilst they, in the height of their carelessness, retain the objects used by their late friends, and treat the malady with imaginary remedies, such as mummies, bezoar, and amulets—whilst all this is done among the Turks, the Christian nations, on the other hand, seek, by isolation, cleanliness, a strict diet, and a scientific treatment, to guard against it; to establish quarantines, which combine the greatest attention to health with the comfort of the traveller, as far as it can be attended to safely; so much so, that a most lively traffic and communication, as well by land as sea, is entertained, without that one single life is endangered. It is even owing to civilization, with its unremitting advice and interference, that in the very seat of the plague the tenets of fatalism are no more attended to, but that the natives listen to the precepts of a sound medical policy. The hope of philanthropists, that the time when even those barbarous nations will enjoy the blessings of civilization, by being freed from the plague, is not distant from its accomplishment.

The yellow fever. The yellow fever has even visited of late the shores of the European continent, and left behind the salutary admonition, that only great care and rigorous isolation will prevent its return. If the observation should be confirmed, that the germ of this disease is mostly generated on board slave-ships, by the effluvia of the Negroes crowded together, the general prohibition of the slave-trade ought to be the more urged, as an affair of humanity and of science, on which medicine ought to insist on its own account.

Ague. Intermittent fevers (of which it may be said, that they are more than any other disease, rooted in the soil,) are gradually losing ground and disappearing. If the attempts to regain extensive marshes for cultivation and traffic are persevered in—if the draining of stagnant waters, and the prevention of inundations in the neighbourhood of the sea, &c. are duly pursued—if, in districts where rice or hemp is cultivated, the deterioration of the air is prevented by the planting of trees—if the cleansing of sewers and the filling up of moats are attended to—if this care is extended to the procuring of pure water for drinking, good food, and cleanly dwellings—marsh-fevers will cease, even although they had obtained the right of denizenship, and seemed to have become endemic. The history of the economical improvements on the surface of the soil affords the best proofs of the truth of our assertion. The number of diseases and deaths gradually decreases; and those once dreaded localities—as if the abode of some fabled dragon—are even sought after on account of their salubrity.

Dysentery. This disease, which was formerly so severe an affliction, and spread over large tracts of country, has progressively undergone a remarkable decrease, as well in the numbers of the subjects affected, as in the intensity of the affection. There scarcely remains a doubt that this improvement does not so much depend on epidemic influences, as on the promulgation of better dietetic rules, the production of more wholesome food, and the more general regard now paid to adequate nourishment, clothing, residence, warming, &c.; and, in the case of the disease being threatened, to the adoption of more effectual preventive measure.

Cholera. The Asiatic cholera—the strange produce of climatorial and national causes in the East Indies—has lately fallen, like a new disease, on Europe. But certainly its sudden invasion and gradual disappearance alike demonstrated the existence and the benefits of a higher state of civilization. It was only under the shelter of transient political and warlike turmoils, that it was ever possible for the disease to transgress certain limits, which, otherwise, it could have never done. The means resorted to by one of the German governments to protect its territory from the approaching plague will remain memorable in the history of its European migration. The resolution to combat the approach of the invading enemy, step by step, was the result of the examination of extensive data, officially investigated. Here, as in every case where we do not yield to illusion, we succeeded, in numberless cases, by a rigid, complete, and consequential carrying out of the sanitary regulations, and especially by the isolation of the infected, and the careful cleansing of the dwellings, to suppress the evil in its very germ, and to check its further spreading. At the period of this danger there were also manifested such an amount of sympathetic feelings, such care for the support of the poor and suffering, such attention of the authorities to the cleansing of dwellings, as well as such an assiduous cooperation of all medical men towards effecting everything which human art and experience afforded towards the eradication of the disease, that we at length succeeded to vanquish it step by step, and finally to expel it utterly from Europe. As, in this case, root and seed seem to be destroyed, it is to be hoped that the acquired knowledge of its true character will prevent its again returning to us from its far-distant original abode.

The chimney-sweeper's cancer, which existed especially in England, has been expunged, as it were by act of parliament, from the series of pathological phenomena. The mode of cleansing of very narrow chimneys by boys had occasioned this disease; and the new regulation, to clean such by the machines only, removed both cause and effect.

Scurvy. The sea-scurvy is one of the incontrovertible proofs that advancing civilization is capable, by the searching out of the right means, and their careful application, to take away from even the severest ailments their strength, nay, to destroy their very existence. The orders of the English Admiralty, to supply all vessels, which undertake long voyages, with a sufficient quantity of lemon-juice, made it possible for them to approach even the South and North Poles without a single case of this once so dreaded malady. In the government vessels, where the prescribed cleanliness and good food are strictly attended to, there has, of late, no case of scurvy been heard of; not so, however, in the vessels of private individuals, who neglect, on account of a sordid economy, to keep a medical man on board, and to provide the necessary store of lemon-juice and fresh provisions. It is in vain that we now look in the naval hospitals for this form of cachexy: whoever would now know anything of it must look back into books, or in situations where the precepts of civilization are not attended to.

Lepra. This was the most dreadful among the popular maladies of antiquity, and attacked, under different forms, even the higher classes: with us it has almost vanished, leaving scarce a trace behind. In former times, thousands of leper-houses served for the reception of those who suffered from the most severe forms of this most contagious disease; now it is a rare occurrence, whenever a slight modification of it makes its appearance anywhere. It was principally by means of the knights returning from the East during the Crusades, that this disease was spread in Europe; but the development of our civilization has driven it once more back to the countries where the low state of medical art, the indolence of the people, superstition, and ignorance may yet probably for a long time afford an appropriate shelter.

Smallpox. It is yet doubtful whether the smallpox was known to the ancients; it is too certain, however, that this disease was considered for many centuries as one of the unavoidable calamities of mankind: it has nearly disappeared since the discovery and introduction of the vaccine. Of all the blessings which have ever been bestowed by man on man, none has proved greater and more important than this. Up to the epoch of its discovery, about a twelfth part of the population of the globe had been either destroyed, or deprived of their health, or disfigured by this plague. To such ravages this inestimable process put a sudden stop, as if by the wand of a magician. The occasional appearance of the real or modified smallpox since then, cannot be placed in comparison with what took place before. Even at the time when it was believed that a single and incomplete inoculation was a sufficient guarantee, and every other precaution was neglected, the mortality, compared with former times, had extremely decreased. And now, in all countries where vaccination has been placed under official control, where care is taken that a sufficient supply of efficient virus is provided, and where revaccination is duly attended to, the smallpox has ceased to exist as a mortal disease. Vaccination is a splendid proof of the advantages of civilization, the most precious result of human thought and research, the fairest reward for devoted attention paid to the delicate and, at times, almost incomprehensible hints and indications of nature. The health and life of millions are preserved through its agency, without any sacrifice of pain or privation. This discovery, like the armed Minerva from the head of Jove, sprung at once, perfect and efficient, from the studies and experiments of its great discoverer. Right speedily it accomplished its triumphal march through the world, reaching even the most distant Indians, from whom there is still a letter in existence, addressed to the discoverer, in which they thank him for that gift of the Great Spirit. Thus it results, that the undeniable blessings of civilization reach even the countries of the remotest savage, and make him more ready to receive the other advantages of civilized life, which may be less obvious to his rude perceptions.

To conclude: Do we need any more enumeration of diseases, in support of the fact, that civilization not only does not increase them, but diminishes and partially eliminates them? Almost every one of the innumerable ailments to which flesh is heir, affords, when thoroughly investigated in its causes and relations, a new proof of this consoling truth. In the same proportion as arts, sciences, morals, and refinement advance, so also are the means multiplied whereby human life is strengthened and protected, as well against internal as external foes. True knowledge and true welfare march hand in hand together. As in the Grecian

Mythos of old, light and healing are the same. Phœbus Apollo who illuminated the world, was also its helper; to him was sung pæans, the hymns of salvation. What previous ages clothed in mythical garb, has become truth in the lapse of centuries. The nearer man arrives at the full consciousness and development of his powers, the more surely will he also attain the full harmony of corporeal existence. It can be therefore asserted, with perfect consistency, that knowledge is not only power, but even health. The approach to knowledge is now forbidden to none; the printing-press and the schools afford to every one participation in the the highest goods of mankind. And Medicine has not kept behind the other promoters of humanity. As its glorious aim has always been to eradicate diseases or to abate their violence, to assist the suffering, to strengthen the healthy, so has it also endeavoured more and more to make its truths the common patrimony of mankind, and the undeniable evidence of civilization.

REPORT ON THE CHANGES IN THE BLOOD IN INFLAMMATION, AND ON THE NATURE OF THE HEALING PROCESS.

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CHANGES IN THE BLOOD IN INFLAMMATION.

I. CHANGE IN THE STAGNANT BLOOD.

§ 1. The red corpuscles stagnant within the vessels cease to be distinguishable individually, and appear as if fused together into a uniform red mass; interspersed throughout which (the frog being the subject of observation) the nuclei of the red corpuscles are still to be seen on careful examination.

§ 2. This change in the stagnant blood appears to have been first particularly referred to by Dr. Hastings. In the natural state of the blood, he observes, globules can be distinctly seen; but after inflammation has commenced the globular structure disappears, and the blood becomes redder.[*]

II. EXPLANATION OF THE CHANGE IN THE STAGNANT BLOOD.

§ 3. The explanation of this change of the accumulated and stagnant blood-corpuscles which Emmert gives is this: The matter contained in the interior of the red corpuscles escapes with or without destruction of their walls, becomes mixed with the plasma still present in very small quantity, and thus forms a homogeneous looking mass, inclosing the nuclei which remain behind. That the substance contained within the blood-corpuscles escapes from them, is inferred partly from the disappearance of the blood-corpuscles, partly from the small quantity of plasma present, seeing that this although coloured would by no means be alone sufficient to form the homogeneous mass. Moreover that the colouring matter escapes from the red corpuscles is distinctly proved by the circumstance to be afterwards mentioned, that it exudes through the walls of the vessels, and tinges of a red colour the surrounding cellular tissue. Whether the wall of the blood-corpuscle remains during this process or is destroyed can scarcely be determined by direct observation. Still it does not appear that at the commencement of the change at least destruction takes place. It would rather seem that the disappearance of the previous form of the blood-corpuscles is owing to a transudation of their contents, and a coincident contraction of their walls. And this, because as will be mentioned when considering resolution of inflammation, it may happen that the blood mass which has but recently undergone the change is reconverted into blood-corpuscles, and passes again into circulation.*

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[*] A Treatise on Inflammation of the Mucous Membrane of the Lungs: to which is prefixed an experimental inquiry respecting the contractile power of the blood-vessels, and the nature of inflammation. London, 1820, p. 95. Dr. Hastings's explanation of these alterations came as close to the truth as the state of knowledge of the blood and physiology in general at the time permitted. And it is to be remarked that Dr. Hastings points out a relation between these alterations in the blood and the debilitated action of the vessels, stating the occurrence of the change in the blood on its being received from healthy into debilitated vessels; and, *vice versâ*, its gradual reassumption of its natural appearance when, on the occurrence of resolution, the blood passed from these debilitated vessels into a large venous trunk, whose action was unimpaired. Though Dr. Hastings's work was published more than twenty years ago, it would still have done honour to the science of the present day, if it had now only appeared for the first time.

§ 4. The reader who has already considered what the author of this Report has said in his 'Observations on the Blood,' and in his Report on the Nature of Inflammation, will readily detect the insufficiency of this explanation offered by Emmert, and will at once perceive that the apparent fusion of the red corpuscles is of the same nature as that which occurs in buffy blood, or in healthy blood, treated by the reagents already spoken of.

§ 5. The change in the appearance of the stagnant blood under consideration supervenes on stagnation more or less quickly. It may usually be observed to implicate the blood in the vessels first obstructed, and from thence to spread to the blood in the vessels more lately affected.

§ 6. This circumstance seems to show that the change is owing to some condition coming into operation subsequently to the stagnation. The apparent fusion of the red corpuscles being evidently of the nature above referred to, viz. owing to a change in their endosmotic state, to their having become somewhat distended, the condition on which it depends, it is to be inferred, is a change in the fluids by which the red corpuscles are surrounded.

§ 7. The change in the endosmotic state of the red corpuscles, and the change in the fluids by which the red corpuscles are surrounded, Henle endeavours to explain thus: "At a certain height of the inflammation absorption ceases, and the capillaries containing the stagnant blood-corpuscles are soon surrounded by a fluid which is thinner than blood plasma, and cannot but change the blood-corpuscles in the manner above mentioned. It is as if the blood-corpuscles having been separated from the plasma by filtration were treated with diluted serum." (Bericht, p. 129.)

§ 8. Undiluted serum has not, as is well known, the effect of causing much distension of the red corpuscles by endosmose, and when sufficiently diluted to cause much distension, it at the same time prevents the aggregation of the red corpuscles or disperses it if it has already taken place. (§ 107.)

§ 9. The distension and apparent fusion of the red corpuscles stagnant within the vessels being like what are observed in buffy blood out of the body, and the same as may be artificially produced by an increase of viscid matter and diminution of salts in the plasma, the author of this Report believes that the change is owing to the action of a plasma inspissated as regards its protein constituents, but deprived of a portion of its salts by exudation of serum. In fact, he believes the apparent fusion of the red corpuscles which supervenes on their stagnation within the vessels to be owing to the action of the plasma in that very condition to which, it was shown in the former Report, (§ 40,) Henle attributed the first stagnation of the red corpuscles, but, as the author endeavoured to show, erroneously. Instead of the first stagnation, Henle's arguments find their application, in explanation of the changes in the blood under consideration and that with full force.

§ 10. In the course of the endosmotic changes just described, while the red corpuscles receive into their interior a portion of the surrounding and altered plasma, they give out a part of their contents, especially the colouring matter. This passing out from the vessels with the exuded fluid, tinges the adjacent parenchyma.

§ 11. To this transudation of colouring matter from the blood in the vessels to the adjacent parenchyma, as also to the endosmotic changes in the red corpuscles, the variations in the tint of the redness of an inflamed part appear to be owing.

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* Beiträge zur Pathologie und Therapie, Erstes Heft, p. 84. Bern, 1842.

§ 12. When stagnation has not continued long before resolution takes place, the red corpuscles though distended and apparently fused together appear to remain entire, and on being restored to the circulation, resume their former collapsed and isolated state. But if stagnation has already existed for any length of time before resolution occurs, and especially if some other event than resolution supervenes, it is very probable that some of the red corpuscles burst and dissolve. See § 89.

III. CHANGES IN THE GENERAL MASS OF BLOOD.

§ 13. Inflammation may arise without any previous change in the general mass of blood, and if limited in extent, may run its course without the supervention of any such change. As, however, in inflammation of any severity and extent, changes in the general mass of blood early show themselves, and as these changes when they occur, exert an important influence by reaction on the inflammation and its events, they here require some consideration.

§ 14. The most characteristic and important change which the general mass of blood in inflammation presents, is an increase in the quantity of fibrin, and a decrease in that of the red corpuscles. And in connexion with this change in respect of composition, there is the remarkable increase in the tendency of the red corpuscles to aggregate into rolls, when the blood is drawn from the body.

§ 15. In consequence of the decrease in the quantity of red corpuscles, blood drawn in a well-marked case of inflammation appears paler, thinner, and more fluid than natural. So much more fluid is it, that if a prick be made in the finger of the patient, the blood, instead of oozing slowly out in the form of a drop, flows out quickly, and spreads on the surface of the skin.

§ 16. In consequence of the increase in quantity of the fibrin and of the increased tendency of the red corpuscles, diminished in quantity, to aggregate together, blood drawn in inflammation presents the buffy coat, and that by the following process as formerly explained in this Review: "The minute process leading to the separation of the liquor sanguinis from the red corpuscles, the visible condition for the formation of the buffy coat, consists in an exaltation both of the rapidity and closeness with which the red corpuscles naturally aggregate into rolls, and these again into a sponge-work, thus squeezing out the liquor sanguinis from among the corpuscles, and allowing the *greater specific gravity of the latter to come more fully into play*, whereby the liquor sanguinis, which in such cases is in relatively greater quantity, collects at the top, and coagulating, gives rise to the buffy coat."*

§ 17. As excess of fibrin constitutes so striking a character of the condition of the blood under consideration, the late Dr. Simon of Berlin, distinguished it by *the name of hyperinosis*, (from ὑπερ and ἴς, ἶνος, flesh.)†

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* For details on this head the reader is referred to the author's 'Observations,' in this Review, for Oct. 1842, and to a short paper in the Edin. Med. and Surg. Journal, for Oct. 1843. Before leaving the subject, however, the author of this Report begs to say that Dr. Williams, (Principles of Medicine, § 205, p. 96,) in representing and objecting to his views, as laid down in the 'Observations' just referred to, has altogether fallen into error. To guard against the erroneous supposition to which the observation of a thin stratum of blood under the microscope is apt to lead, viz. that the red corpuscles run less closely together into a mass in buffy than in healthy blood, the author of this Report thought it necessary, in his 'Observations,' to devote a long paragraph, p. 594. This Dr. Williams has entirely overlooked, and adduces the erroneous supposition in question as an objection to the author's comparison to a contracted sponge. Dr. W. moreover speaks as if the author had not taken into consideration the specific gravity of the red corpuscles, whereas it is distinctly referred to, not only in the extract above given but in two or three other places. It is curious to find how differently from the English author a foreigner represents the author's statements on this subject: "Wharton Jones," says Henle, (Bericht, &c. p. 120,) "legt besondern Werth auf das vermehrte specifische Gewicht des Blutkuchens. Indem nämlich die Säulen der Blutkörperchen zu einem viel dichteren Netzwerk sich zusammenfügen als im normalen Blut, sind die von Plasma, erfüllten Räume relativ kleiner und die ganze Masse wird schwerer."

† Besides inflammations, hyperinosis occurs in some other diseases, e. g. phthisis pulmonalis, and also physiologically in pregnant women.

An opposite condition of the blood, viz. diminution in the quantity of fibrin, the quantity of red corpuscles remaining the same, or the quantity of fibrin remaining the same, an increase in the quantity of red corpuscles, occurs in fevers, hemorrhagic diseases, plethora. To this Simon has given the name of *hypinosis*, from ὑπο and ἴς, ἶνος.

§ 18. The natural quantity of fibrin and red corpuscles in the 1000 parts of blood is :—

According to Simon.		*According to Lecanu, and adopted by Andral and Gavarret.*	
Fibrin	2·1	Fibrin	3·000
Red corpuscles	110·000	Red corpuscles	127·000

§ 19. The fibrin may in hyperinosis of the blood increase, according to Simon, to four times, according to Andral and Gavarret, to three and a half times the above quantities respectively admitted by them as natural. The red corpuscles, on the other hand, may diminish, according to Simon to one third, according to Andral and Gavarret, to one sixth of their natural quantity.

§ 20. The remarkable inverse proportion in the quantity of fibrin and red corpuscles in hyperinosis of the blood, is well illustrated by the two following tables, drawn out by Simon, the first from his own analyses, the second from those of Andral and Gavarret.*

Taking the quantity of the solid constituents of the blood at 100, the relation in respect of quantity between the fibrin and red corpuscles stands thus:

	Fibrin.	*Red corpuscles.*
Healthy blood	1	53
Morbid blood	1·4	43
,,	1·6	40
,,	1·7	40
,,	2·0	42
,,	2·0	39
,,	2·1	36
,,	3·0	28
,,	6·0	22

	Fibrin.	*Red corpuscles.*
Healthy blood	1·5	61
Morbid blood	2·5	60
,,	3·2	57
,,	4·1	57
,,	4·2	54
,,	4·8	52
,,	5·0	50

§ 21. In the 'Observations' on the Blood in this Review, for Oct., 1842, it was remarked that though blood on which a buffy coat afterwards forms, appears thinner than natural, this is not owing to any increased tenuity of the coagulable lymph, or

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* The opposite inverse proportion in the quantity of fibrin and red corpuscles in *hypinosis* of the blood is illustrated by the following table reduced by Simon from the analyses of Andral and Gavarret. In the first series the proportion of fibrin and blood-corpuscles is that in the 100 parts of solid constituents of the blood. The second series is the same, only the quantity of fibrin is constantly given at 1·5, and the proportion of blood-corpuscles reckoned accordingly. By this means the increase of the latter is rendered more striking.

	First Series.			*Second Series.*	
	Fibrin.	Red corps.		Fibrin.	Red corps.
Healthy blood	1·5	61	Healthy blood	1·5	61
Morbid blood	1·5	64	Morbid blood	1·5	64
,,	1·5	65	,,	1·5	65
,,	1·3	60	,,	1·5	69
,,	1·1	53	,,	1·5	72
,,	1·2	59	,,	1·5	74
,,	1·1	60	,,	1·5	81
,,	1·0	60	,,	1·5	90
,,	0·9	60	,,	1·5	90
,,	1·0	61	,,	1·5	91
,,	1·0	64	,,	1·5	96
,,	0·9	63	,,	1·5	105
,,	0·5	60	,,	1·5	180

more properly speaking, liquor sanguinis, but to a diminution in the number of red corpuscles. This circumstance of the liquor sanguinis not being attenuated in blood in the state of hyperinosis, though, in consequence of the diminution in the quantity of the red corpuscles, the quantity of solid constituents in the aggregate of the blood is diminished, and the blood as a whole therefore more watery, Henle has illustrated in a striking manner from the data furnished by the analyses of Andral and Gavarret, and of Simon.

22. Excluding from consideration the red corpuscles, and comparing the plasma of the blood in inflammation with that of the blood in health, the quantity of water is in the former case less than in the latter. Besides this diminution in the quantity of water in the plasma of the blood in inflammation, it appears that there is frequently a diminution in the quantity of salts, whilst the quantity of fat, albumen, and fibrin, is increased; the quantity of the latter in general to a greater amount than that of the former.

§ 23. This actual diminution in the quantity of water in the blood in inflammation, Henle thinks is produced by the exudation of serum, and, as was shown in the last Report, it is the inspissation of the plasma resulting therefrom, which he thinks is the condition which immediately determines the agglomeration of the slowly flowing corpuscles, and their subsequent stagnation.

§ 24. The objection, however, was raised to this view, that stagnation precedes the exudation of serum. And in the present Report, it has been shown, (§ 9,) to be probable that it is the changes which take place in the blood subsequently to its first stagnation, which are produced by the plasma inspissated, as regards its protein constituents, but deprived of a portion of its salts.

§ 25. Hitherto the protein compound coagulable spontaneously, found in buffy blood, has been called fibrin, but according to Mulder's recent researches,[*] it would appear that the buffy coat does not consist of true fibrin, but is composed of a *binoxide* of protein, which is insoluble in boiling water, and a *tritoxide* which is soluble. These oxides Mulder comprehends under the name of *oxyprotein.*

§ 26. Is the quantity of colourless corpuscles of the blood changed in inflammation in the stage under consideration? That the quantity is increased is very generally maintained principally on the following grounds:

1st. The accumulation of colourless corpuscles within the vessels of an inflamed part.

2d. The large quantity of colourless corpuscles contained in the liquor sanguinis, which in blood drawn in inflammation, rises to the top to form the buffy coat.

3d. Mr. Gulliver's observations in the 'Philosophical Magazine,' for September, 1838, have been especially adduced by Dr. Carpenter, as being in his opinion at least, satisfactory proofs that "the usual amount of colourless corpuscles in the blood is very much increased at the time when an increase of fibrin is taking place, that is in the inflammatory condition."[†]

§ 27. In regard to the accumulation of colourless corpuscles within the vessels of an inflamed part, it was shown in the Report on Inflammation, § 35, in the last Number of this Review, not to be any indication of an increase in the quantity of colourless corpuscles in the general mass of blood. It is in fact no more an indication of this than the occurrence of a crowd in the street before one's window, can be reckoned an indication that the number of inhabitants of London is amazingly increased.

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[*] Ueber die Oxydationsproducte des Proteins im thierischen Organismus; in Wöhler and Liebig's Annalen der Chemie und Pharmacie, Band xlvii. Heidelberg, 1843.

[†] Report on 'Cells,' in this Review, for Jan. 1843. M. Gendrin has been referred to also as an authority that the colourless corpuscles are much increased in number in inflammation, but no reference is made to the work where M. Gendrin has made a statement to such an effect. The author of this Report has not been able to find it in the 'Histoire des Inflammations.' Indeed when M. G. wrote this work (1826) he did not know of the existence of colourless corpuscles farther probably than this, viz. that the corpuscles which, in the frog, he took for pus-corpuscles, into which he supposed the red corpuscles had become converted, were most probably colourless corpuscles.

§ 28. On the large quantity of colourless corpuscles in the buffy coat,[*] considered as an indication of an increase in their quantity in the general mass of blood, it was remarked in the observations on the Blood, in this Review for October, 1842, that without denying that the colourless corpuscles may be more numerous in buffy than in healthy blood, it is probable for the reasons given that they are actually not so very much so as might at first have been supposed. And this especially as in microscopical examinations of newly-drawn blood, which afterwards became buffed, any very marked increase over what occurs in health, in the number of colourless corpuscles, was not observed.

§ 29. As to the proofs derived from Mr. Gulliver's observations, it is to be remarked: Mr. Gulliver's object in his paper appears to have been simply to establish the fact of the existence in the blood he examined of corpuscles, which he considered pus-globules; not at the time being aware of the existence naturally of the colourless corpuscles of the blood. His expressions do not point to any great numbers. Had healthy blood been treated in the same way, colourless corpuscles would also have been "detected," "numerous colourless corpuscles would have been observed," "many would have been seen," "several would have been found," &c., and certainly healthy blood examined without any preparation will not "rarely' but *always* present colourless corpuscles.

§ 30. But supposing that the corpuscles referred to by Mr. Gulliver, had been *all* colourless corpuscles, (which, however, Mr. Gulliver does not seem to admit,) and that had their quantity, instead of being vaguely indicated, been accurately determined, and had they been found by comparison with the quantity of colourless corpuscles in healthy blood actually increased, as Mr. Gulliver's subsequently published statements warrant the supposition they would have been found to be,[†] do Mr. Gulliver's cases of 1838, and his subsequently published ones, come under the head of inflammation proper, at present under consideration, the stage of inflammation at which an increase of fibrin, hyperinosis, is taking place? No: they were cases for the most part of suppurative inflammation, under which the subjects of them, with two or three exceptions, sank. It is also to be remarked that, with these exceptions, it was only after death that the blood was examined.[‡]

§ 31. The inference from what has been now said is this: In cases of recent inflammation, i. e. when hyperinosis is taking place, it has not yet been shown that there is any very great change in the quantity of colourless corpuscles, but when the inflammation has already existed some time, and as Mr. Gulliver says, especially when attended by suppuration, there may be a marked increase in the quantity of colourless corpuscles.

IV. EXPLANATION OF THE CHANGES IN THE GENERAL MASS OF BLOOD.

§ 32. *Source of the fibrin in hyperinosis of the blood.* Considering that it is in the blood that fibrin first appears in any considerable quantity, and endowed with the strong and peculiar tendency to become organized, and considering that the red corpuscles are principally if not wholly composed of protein matter, the author of this Report suggested in his 'Observations on the Blood,' already often referred to, that the more peculiar object of the elaboration supposed to be performed by the red corpuscles in their capacity of glandular cells might be the conversion of one protein compound into another—albumen into fibrin—a less into a more highly organizable proximate principle.

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[*] The buffy coat in anæmia is not here referred to; though this may contain a very large quantity of colourless corpuscles, it contains very little fibrin.

[†] In his Appendix to Gerber's General Anatomy (1840), Mr. Gulliver speaks of the pus-globules found in the above experiments and cases thus: In the inflammatory diseases, especially when attended by suppuration, whitish globules, which I have elsewhere described as those of pus, may be found in unusual numbers in the blood, (p. 20.) And again, in the Philosophical Magazine for Sept. 1842, in a paper on the pus-like globules of the blood, Mr. Gulliver says: "In some of my earlier observations these two varieties of globules (colourless corpuscles of the blood and pus-globules) were doubtless confounded; and their similarity is often so close that it may well be questioned whether there is any essential difference between them in many cases, although it is difficult to avoid attributing to the effects of disease the unusual abundance of pus-like globules in the blood of patients labouring under numerous inflammatory distempers."

[‡] Andral (Essai d'Hématologie Pathologique, p. 113 et seq.) gives three similar cases.

§ 33. He was further corroborated in this view by reflecting on the remarkable inverse relation, as regards quantity, between the fibrin and red corpuscles of the blood in disease; for, according to the theory of secretion, on the principle of which the view depends for its foundation, the secreting cells are, with their contents, resolved into the secreted fluid; and therefore, as the secreted fluid increases, the secreting cells must decrease in quantity, to be replaced by newly-formed ones.

§ 34. The inference in regard to the source of the fibrin in hyperinosis of the blood from all this was obvious, viz. that the augmentation of fibrin is at the expense of the red corpuscles which have disappeared, these in consequence of increased action, having been more quickly and in greater quantity resolved into fibrin than in health, and that in a ratio disproportionate to the production of new red corpuscles.

§ 35. This view of the production of fibrin the author of this Report enunciated as his own, in his Observations on the Blood, in this Review, for October, 1842, and in his Report on Inflammation, in the last Number, he claimed it, protesting against the award of it which had been made to Professors Wagner and Henle, by Drs. Carpenter and Williams. Since then the late Dr. Simon's 'Physiologische und pathologische Anthropochemie' having come into his hands, he finds that Dr. Simon had already enunciated a similar view.*

§ 36. After adducing various arguments in support of the opinion, that fibrin is formed from the red corpuscles, Dr. Simon (p. 68) goes on to say: The formation of fibrin by metamorphosis of the red corpuscles is, however, shown much more clearly—I might say convincingly—from my researches on the blood. These show that the quantity of fibrin is, with rare exceptions, always in a pretty definite inverse proportion to the quantity of red corpuscles—the more red corpuscles blood contains, the less fibrin—or, the more fibrin, the fewer red corpuscles. Admitting that the fibrin is formed from the red corpuscles, this phenomenon is simply explained; as when there is a greater consumption of red corpuscles, the quantity of fibrin in the plasma must increase.

§ 37. Which of the component parts of the red corpuscles, he then goes on to ask, is that which is most likely expended in the production of fibrin—that most important constituent of the plasma? It can scarcely be *globulin*, he answers, as it amounts to from four to ten per cent. of the blood; and, being a protein compound, is chemically so closely allied to fibrin, that, admitting a conversion of globulin into fibrin, we should expect to have a much greater quantity of fibrin in the blood than is found in it. It is, he continues, still less likely to be the *hematin* of the red corpuscles which is expended in the production of fibrin. The object of the hematin rather appears to be, to promote and maintain the independent metamorphosis of the red corpuscles, by its great affinity for oxygen and by its own proper metamorphosis, and not to form a product further serving for nourishment to the plasma.† There still remain the envelopes and nuclei of the red corpuscles. The envelopes are still too little known; but the nuclei have, in fact, a very close chemical resemblance to fibrin, so that their conversion into it (perhaps whilst oxygen is taken in and carbon given out) may be easily conceived.

§ 38. "The nuclei," Simon still continuing, "of the red corpuscles in the early stage of development are large and distinct, but as development advances they become smaller, and at last disappear—wholly, according to C. H. Schultz and Henle, in those corpuscles which are in their last stage. It is thus seen that the metamorphosis of the nuclei is by no means sudden, but goes on *pari passu* with the development of the red corpuscles. Burdach, R. Wagner, and Valentin are of opinion, that the red corpuscles, as long as they are circulating in the living body, have no nuclei, and that nuclei form only at the moment when the red corpuscles are removed from the circulation. R. Wagner found that the mere contact with the atmospherical air determined the formation of the nuclei. According to this, the nuclei resemble chemically the fibrin of the plasma still more; and if it be unequivocally the case that in a red corpuscle in which the nucleus has, in consequence of advanced development, already become dissolved, its reappearance is observed when the red corpuscle has been withdrawn from the body, then, in my opinion, the conversion of the nucleus into fibrin would be sufficiently proved; as we know that no other constituent of the blood possesses the so characteristic property of being held in solution in living blood, and of being deposited as an insoluble mass by dead blood."

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* Dr. S.'s Physiologische und pathologische Anthropochemie, is dated Berlin, April, 1842; but he mentions in the preface that he had enunciated the view in 1840, at the meeting of naturalists at Erlangen.

† The hematin he supposes is ultimately metamorphosed into hemaphæin or brown colouring matter of the plasma. This brown colouring matter of the plasma is continually being removed along with the urinary secretion, it being in fact the colouring matter of the urine also.

§ 39. To this opinion of Simon, that fibrin is derived from nuclei of the red corpuscles, an objection at once presents itself in the circumstance that there are *no nuclei* in the red corpuscles of man and the mammifera. But, waiving this objection, and admitting, with Simon, that nuclei do exist in the red corpuscles in their early stage of development, but that they soon disappear, being resolved into fluid fibrin; and that this fibrin is retained within the corpuscles and set free to be mingled with the plasma only when the red corpuscles themselves become dissolved. Admitting all this, it is quite clear that the red corpuscles of the blood of man and the mammifera, in the state in which they are usually found, should consist of fluid fibrin, in addition to their globulin, hematin, and envelopes.

§ 40. In support of the likelihood of this, Simon, as above seen, adduces the allegation which has been made by Burdach, Wagner, and Valentin, that the red corpuscles of the frog's blood, for example, present, while circulating within the body, no nuclei, and that the appearance of nuclei in the red corpuscles out of the body is owing to the fibrin contained in their interior coagulating.

§ 41. Supposing this allegation well founded as regards frogs' blood, there should, on the same principle, be found a nucleus in the red corpuscles of the blood of man and the mammifera after being drawn from the body, if there be fluid fibrin in their interior while circulating within the body; but as no nucleus presents itself, the conclusion is obvious, that there is no fluid fibrin in the interior of the red corpuscles while circulating within the body.

§ 42. But the allegation, that there is no nucleus in the red corpuscles of frogs' blood while circulating within the body, is not well founded. A nucleus does exist, although not distinctly seen, for the reason already given in the former Report. (§ 5.)

§ 43. The only fact tending to show that fibrin already exists in the red corpuscles, is one stated by Dr. Maitland, (an Experimental Essay on the Physiology of the Blood; London, 1838;) viz. that more fibrin is obtained by washing the coagulum of blood than by whipping. Maitland, believing that a nucleus exists and that it consists of fibrin, inferred from this observation, that the excess of fibrin was owing to the nuclei of the red corpuscles being retained within the coagulum. But, as already seen, there can be here, as regards human blood, no question of a nucleus. The additional quantity of fibrin must have been derived from fluid fibrin contained within the red corpuscles. But it may be doubted whether the excess in the product obtained by washing the coagulum over that obtained by whipping, were really fibrin. Did it not consist of the red corpuscles deprived, by washing, of their colouring matter—therefore of globulin and envelopes—retained in the meshes of the coagulated fibrin of the plasma?

§ 44. All that has now been argued appears to lead to the conclusion, that, in the red corpuscles of human blood, there is no fibrin, or at least no fibrin separate and distinct from the globulin, as long as the red corpuscles exist as red corpuscles. If fibrin have its source in the red corpuscles, therefore, that source must be the globulin, hematin, or envelopes. The hematin and envelopes, however, may be dismissed from consideration, for the reasons adduced by Simon.

§ 45. In regard to the globulin, Simon adduces two different kinds of arguments against the likelihood of its being the source of the fibrin in hyperinosis of the blood. His first argument is, that the quantity of fibrin produced is too disproportionate to that of the red corpuscles which have disappeared. His second argument is, that there is a more likely way in which the globulin is disposed of, viz. that it is the immediate source of urea, uric acid, and bilin.

§ 46. It would be out of place here to enter upon any consideration of the second argument: it is enough to remark, that it does not appear to carry much force with it.

§ 47. As to the first argument, it is to be replied, that, though the quantity of fibrin is, in the normal state of the blood, small, and the quantity of red corpuscles very large, it is only what might be expected, seeing that the red corpuscles may be viewed as a store whence fibrin is constantly being drawn, in a certain quantity, but which, on an emergency, might yield more than that quantity. There is no reason to suppose that, under ordinary circumstances, more red corpuscles disappear than may be actually accounted for by the quantity of fibrin which is produced. As fibrin is constantly being abstracted for the purposes of nutrition and secretion, the quantity produced must not be estimated by the quantity found at any one time in the blood. In inflammation, however, it is different, and the difficulty raised by Simon more striking; for here there is a sudden disappearance of a large quantity of red corpuscles, and a substitution of but comparatively a small quantity of fibrin, without there being any reason to suppose that an increased expenditure of fibrin has taken place in nutrition, secretion, &c. but rather on the contrary.

§ 48. If, however, the quantity of red corpuscles stagnant in the inflamed part, and the quantity of fibrin which may have been withdrawn from the blood by exudation be taken into consideration, the difficulty is lessened, if it does not vanish, especially when it is remembered that, in the production of the fibrin, a larger quantity of corpuscles must be expended than there is of fibrin produced, as it is the globulin only of the corpuscles which is supposed to be converted into fibrin, not the envelopes —in regard to the chemical nature of which, as already said, little is known. (§ 37.)

§ 49. Simon finds no chemical difficulty in supposing that a portion of globulin may be converted into albumen. There appears to be as little chemical difficulty in the supposition that globulin is converted into fibrin, seeing that all are equally protein compounds. But, as to the conversion of globulin into albumen, it is to be remarked, that it would be a retrograde instead of a progressive metamorphosis.

§ 50. It has been above mentioned (§ 25,) that, according to Mulder, it is not true fibrin but a *binoxide* and a *tritoxide* of protein, together comprehended under the name of *oxyprotein*, of which the buffy coat and of course the spontaneously coagulable matter of the plasma of the blood in hyperinosis are composed.

§ 51. Oxyprotein, Mulder affirms, is not derived from the albumen of the blood, but results from the oxydation of the fibrin by absorption of oxygen in respiration.

§ 52. Mulder here says nothing of the red corpuscles, nor of the source of the increase in the quantity of fibrin, which, by oxydation, should yield the excess of oxyprotein which exists in the blood in inflammation, over the quantity of fibrin and oxyprotein which exist in the blood in the natural state. But it is easy to supply this omission, on the principle contended for in this Report, in regard to the production of fibrin or oxyprotein, by supposing either that the red corpuscles are first resolved into fibrin, and that this fibrin, by absorbing oxygen, is, in conformity with Mulder's views, converted into oxyprotein; or that the red corpuscles absorbing oxygen, their globulin is at once resolved into oxyprotein.

§ 53. The hypothesis, that the fibrin of the blood is the result of the secreting action of the red corpuscles, was objected to by Dr. Carpenter in his Report on the Origin and Functions of Cells, above referred to. His principal argument against it is founded on the very facts which were adduced and relied on by the author of this Report as being most strongly in its favour—facts which, in the preceding pages, have been shown to have been previously made use of by the late Dr. Simon *on the same principle*, in support of nearly the same view.

§ 54. The facts referred to are: the diminution in the quantity of red corpuscles which is found to accompany hyperinosis of the blood, and the opposite inverse relation in the quantity of red corpuscles and fibrin in hypinosis of the blood.

§ 55. In employing the fact of the inverse relation in quantity between the red corpuscles and the fibrin in hyperinosis and hypinosis of the blood as an argument against the view, that the fibrin of the blood is the result of the secreting action of the red corpuscles, Dr. Carpenter entirely loses sight of the principle of the doctrine of secretion which he gives an account of a few pages further on in the same Report.

§ 56. But, besides the fact of the inverse relation in quantity between the red corpuscles and the fibrin, Dr. Carpenter adduces "another important fact having the same bearing," viz. the observation made by Remak, that "when an animal is bled largely and repeatedly, the quantity of red corpuscles in its blood is greatly diminished, whilst the proportion of the colourless corpuscles is increased." And this observation of Remak he couples with the circumstance, that it has been ascertained by Andral's investigations that the quantity of fibrin is but little affected by bleeding. The inference which Dr. Carpenter draws from all this is, that the white, rather than the red corpuscles are the agents of the elaboration of fibrin.

§ 57. It is quite true, not only that, when the same individual has been subjected to several bloodlettings, the red corpuscles become greatly diminished in number after each successive one, but also that the fibrin is increased in quantity. This is shown, in a very striking manner, by the following table from Andral and Gavarret:

Fibrin.	*R. Corps.*	*Fibrin.*	*R. Corps.*	*Fibrin.*	*R. Corps.*	*Fibrin.*	*R. Corps.*
6·3	130	6·1	123	4·0	111	5·6	133
7·7	106	7·2	120	5·5	107	5·5	131
8·2	112	7·8	112	6·5	101	9·1	128
9·3	103	10·2	101	9·0	83	9·4	102*

§ 58. This diminution in the quantity of red corpuscles and increase in the quantity of fibrin after bloodletting is, however, still quite in accordance with the view, that the fibrin results from the solution of the red corpuscles. And though the proportion of colourless corpuscles was found by Remak† increased, his cases afford no ground for Dr. Carpenter's inference, that the white rather than the red corpuscles are the agents of the elaboration of fibrin, seeing that in these very cases of Remak the quantity of fibrin was diminished—a circumstance which Dr. Carpenter does not mention. Remak's cases, in fact, which Dr. Carpenter couples with Andral's cases of moderate bleeding, belong to the category of the cases mentioned by Andral, in which, when bleeding has been carried beyond certain limits, both the red corpuscles and the fibrin are found diminished—a state of the blood to which Simon gives the generic name of *spanæmia*, (αἷμα and σπανος, or σπάνιος, i. e. poverty of the blood.)

§ 59. As, in employing the fact of the inverse relation in quantity between the red corpuscles and the fibrin in hyperinosis and hypinosis of the blood, as an argument against the view, that the fibrin of the blood is the result of the secreting action of the red corpuscles, Dr. Carpenter entirely loses sight of the principle of the doctrine of secretion, on which the view is founded, so, when he appeals to an alleged increase of colourless corpuscles occurring along with an increase in the quantity of fibrin in the blood, as an argument in favour of the view he proposes, he equally loses sight of the principle of the doctrine of secretion.

§ 60. But suppose for a moment that his line of argument were just: let the facts which he alleges in support of it be examined. In the first place, it is obvious that, according to Dr. Carpenter's principle, there ought to be much fibrin in all cases in which it is alleged there exists, either normally or abnormally, a large quantity of colourless corpuscles. Do Dr. Carpenter's *facts* show this?

§ 61. Dr. Carpenter appeals first to the observation of Remak, that the colourless corpuscles were increased in number in the horse, after being largely and repeatedly bled. But it has been above seen that, in referring to this observation, Dr. Carpenter overlooks the circumstance, that Remak found the proportion of fibrin diminished; whilst, in referring to Andral to show that the quantity of fibrin is increased after bloodletting, he also overlooks the circumstance, that Andral says the same as Remak, that, after copious bleeding, the fibrin is diminished in quantity as well as the red corpuscles.

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* Beyond certain limits, however, it was found by Andral that both red corpuscles and fibrin diminish.

† Medizinische Zeitung d. Ver. f. Heilk. in Preussen, 1841. N. vi, 7 Juli, p. 127.

§ 62. Dr. Carpenter next adduces the blood of the invertebrata as a fluid in which the elaboration of albumen into fibrin must be taking place, for the nutrition of the tissues, as in the higher animals; although there appears to be a total absence of any red corpuscles resembling those of the blood of the vertebrata: the globules contained in the fluid bearing, as suggested by Wagner, a much stronger resemblance to the corpuscles of the chyle.

In regard to this, it is to be remarked that, considering how very imperfect the knowledge of the blood of the invertebrata is as yet, any analogical argument drawn from it in the present case is of too loose a nature to merit the slightest attention.

§ 63. Dr. Carpenter further says, on the authority of Mr. Gulliver, that in very young embryos of vertebrata the white globules are nearly as numerous as the red particles; but omits to show the coexistence of fibrin in a correspondingly large quantity.

§ 64. Dr. Carpenter continues, "We should expect to find in the blood of the fœtus, up to the time of birth, a large proportion of white corpuscles; and this circumstance has been noticed by Dr. Barry, (and communicated to me by him,) with regard to the blood of the umbilical vessels of the placenta, which he had the opportunity of examining very shortly after its expulsion."

§ 65. Here again Dr. Carpenter makes no attempt to show the coexistence of a corresponding quantity of fibrin. But what are the facts of the case? Denis, in his chemical examinations, found the blood of the umbilical artery richer in red blood-corpuscles than the blood of the mother. Thus, in the blood drawn from a vein of the mother, there were of blood-corpuscles 139·9 per 1000; whilst, in the blood from the umbilical artery, there were of blood-corpuscles 222·0 per 1000! The quantity of blood-corpuscles in a dog three months old, was 97·0 per 1000; whilst, in a puppy one day old, it was 165·0 per 1000. As to the microscopical appearances presented by the blood of the umbilical vessels, in a case in which these vessels were carefully secured for the purpose of examining the blood, the author of this Report found the red corpuscles in great abundance—the colourless corpuscles in no preponderance.

§ 66. As to the quantity of fibrin in the blood of the fœtus and young animal, it is, according to Denis, small, in comparison with that of the red corpuscles; whilst in the mother it was as 2·4 to 139·9, it was in the umbilical artery as 2·2 to 222·0. Again, whilst in the dog three months old it was as 2·4 to 97·0, it was in the puppy one day old as 2·0 to 165·0!

§ 67. Dr. Carpenter continuing, says, "the presence of colourless corpuscles, sometimes to a large amount, in the blood of reptiles has long been known." True, but what is the amount of fibrin in the blood of reptiles?

§ 68. The existence of fibrin in chyle, though in small quantity, and when coagulated, much less consistent and firm than in blood, might be adduced in favour of the view, that the colourless corpuscles elaborate fibrin, seeing that there exist in the chyle colourless corpuscles and no red ones.

§ 69. In answer to this, it is to be remarked, in the first place, that the chyle, when it presents any marked quantity of fibrin, has been already mixed with the contents of lymphatics, and these are known to contain fibrin. In the second place, any fibrin which the chyle may present while within the lacteals in the early part of their course, may be supposed to be derived from the blood circulating in such large quantity in the villi during absorption. The fibrin presented by the chyle in the lacteals after they have passed through the glands, besides being derived from admixture of lymph from anastomosing lymphatics, the author of this Report agrees with Simon in supposing it to be derived from the liquor sanguinis of the blood circulating in the vessels of the glands. That the fibrin of the chyle, says Simon, (p. 245,) is formed from the constituents of the food, is not very probable.

§ 70. In the second place, according to Dr. Carpenter's principle, there should be a large quantity of colourless corpuscles where there is much fibrin. But as he has failed to show an increase of fibrin in the cases which he adduced, more or less correctly, as examples of the blood abounding in colourless corpuscles, so from what was above said (§§ 26-31), it will have been seen that here also he fails in proving an increase of colourless corpuscles along with an increase of fibrin.

§ 71. Lastly, Dr. Carpenter refers to the circumstance stated by Wagner, that the number of colourless corpuscles was found to be greater in the blood of well-fed frogs just caught in the summer season, than in those that had been long kept without food, and in those examined during winter.

§ 72. This, however, is nothing to the purpose. In the first case, the blood being in its healthy state, has a due proportion of fibrin, red corpuscles, and colourless corpuscles. The colourless corpuscles in proportion to the quantity of food taken, and the activity in the development of red corpuscles; the latter in proportion to the quantity of fibrin. In the second case, it is to be remarked, that the frogs were in a state spanæmia, or poverty of blood: the red corpuscles were few in number, in consequence of their gradual expenditure in the formation of fibrin, on the one hand, and their non-reproduction, on the other. The colourless corpuscles were few in number; those transformed into red corpuscles not having been duly replaced, in consequence of the want of food. The fibrin was in small quantity, having been expended in nutrition, and not duly replaced, in consequence of the now diminished quantity of red corpuscles. The non-reproduction of these again being owing to the fewness of the colourless corpuscles.*

§ 73. Dr. Carpenter's facts have thus been found not to warrant the inferences he would draw from them, even admitting the justness of his line of argument. But this line of argument has itself been shown to be contrary to the principle of the doctrine of secretion, which even Dr. Carpenter himself advocates.

§ 74. But how stands the relation between the colourless corpuscles and the fibrin of the blood?

In the first place, as to chemical nature: Nothing is yet exactly known of the chemical nature of the colourless corpuscles of the blood.

In the second place, as to quantity: In healthy blood the quantity of colourless corpuscles may be stated to be, on an average, half a part in the 1000 parts of blood,† whilst that of fibrin is, as has been seen, according to Lecanu's average, three parts in the 1000. In inflammation at the time of hyperinosis of the blood, it remains to be shown that there is any increase in the quantity of colourless corpuscles; but suppose there is, how much higher a proportion does it bear to the increased quantity of fibrin than the half to three of the healthy state?

§ 75. In fact, it may be said that the colourless corpuscles and fibrin, instead of being in a direct are in an inverse proportion to each other. This inverse relation, however, it is to be remembered, is not of the same sliding-scale kind as that between the red corpuscles and the fibrin, and therefore not to be viewed as an argument for the production of fibrin from the colourless corpuscles on the principle of the doctrine of secretion above recognized as the correct one.

§ 76. Thus, if the red corpuscles be found small in number when the fibrin is in large quantity, this is owing to the former having been reduced below their usual quantity, and the latter, *pari passu*, increased above its usual quantity. Whereas, if the colourless corpuscles be found few in number in proportion to the quantity of fibrin, this is no more than what already exists, even before any change in the quantity of fibrin has occurred.

§ 77. When, on the other hand, the colourless corpuscles are found in great number at a time when the fibrin is in small quantity, it will be found that the red corpuscles are at the same time in small quantity—that the blood is in the state of spanæmia; and that the relation which the large quantity of colourless corpuscles has, is not like that which the red corpuscles have with the small quantity of fibrin in hypinosis; but the relation is with the diminution in the quantity of red corpuscles themselves.

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* Total privation of food, in the case of frogs, it may be remarked, is no more than defective supply of food in the mammifera. In these latter, the result of total privation may be compared to an acute disease. In dogs and horses kept for some days without food, the blood presents a great increase of fibrin and a corresponding diminution of red corpuscles. This Andral and Gavarret explain by the inflammation of the stomach which is found to have occurred.

† From an average of ten observations and enumerations made by Mr. T. H. Huxley, a distinguished pupil of the class of Anatomy and Physiology at the Charing-Cross hospital.

§ 78. The increase in the number of colourless corpuscles appears to be to yield a supply of red ones, in order to make good the loss of those which have disappeared. In fact, according to Remak's observations on the blood of the horse, as also Gulliver's, the red corpuscles are reproduced by being formed within the colourless ones, and become free by their solution. However this may be, the observation of Remak regarding the increase of the colourless corpuscles after repeated bleeding, which Dr. Carpenter quotes so partially, as above seen, to favour the view that fibrin is formed from the colourless corpuscles, Remak adduces to show that the red corpuscles are reproduced from the colourless ones.

§ 79. Lastly, when there exists along with an increase in the quantity of fibrin and a decrease in the quantity of red corpuscles, an increase in the quantity of colourless corpuscles, it will be found that the increase in the quantity of colourless corpuscles has taken place subsequently to the time when hyperinosis occurred; when the increase in the quantity of fibrin having been at the expense of the red corpuscles, the increase in quantity of the colourless is for the reproduction of the red corpuscles.

§ 80. Thus, then, if the red are reproduced from the colourless corpuscles, it is quite correct, in one sense, to say that the fibrin is derived from the colourless corpuscles. The rapidity with which red corpuscles disappear at the time fibrin is increased in quantity, or, as the author of this Report would say, are resolved into fibrin, and the comparative slowness with which they are reproduced from the colourless corpuscles will account for the relation between the fibrin and the colourless corpuscles not being of the same sliding-scale kind as that between the fibrin and red corpuscles.

§ 81. *Intimate nature of the process leading to hyperinosis of the blood in inflammation.* Considering it as having been by the preceding arguments, rendered very probable that the red corpuscles of the blood are the source of fibrin in the natural state, and of oxyprotein in hyperinosis of the blood, and that, of the constituents of the red corpuscles, it is the *globulin* which is converted into the fibrin and oxyprotein, an attempt may now be made to trace the intimate nature of the process leading to hyperinosis of the blood in inflammation.

§ 82. According to Mulder, the protein oxides always occur in the blood, being the matters employed in nutrition and secretion; but in inflammation they exist in a much greater quantity than in the healthy state. They are formed by oxidation of fibrin in the lungs; hence he says that in inflammation, in which the quantity is so much increased, there goes on a more active process of oxidation, this being the process leading to hyperinosis of the blood.

§ 83. That the process leading to hyperinosis of the blood is one of increased oxidation, is chemically true; but if what has been above said (§ 52) be well founded, there is along with the chemical process an increased activity of organic development. This is the view taken by Simon and the author of this Report.

§ 84. The author of this Report, in his Observations on the Blood, in this Review, for October, 1842, whilst he remarked, in opposition to Liebig's view of the red corpuscles being carriers of oxygen, that there is no reason to suppose that the liquor sanguinis less readily absorbs and is less a carrier of oxygen than the red corpuscles—a view which is the same as that advocated by Mulder, from his experiments, viz. that the fibrin oxydized in the lungs is the principal if not the only carrier of oxygen of the air—also remarked, that the absorption of oxygen by the red corpuscles might be looked upon as accessory to some peculiar function performed by them, rather than as being solely for the purpose of distributing the oxygen to the different parts of the system. That function the author of this Report considers to be the formation of fibrin, as above seen.

§ 85. Under the influence of the oxygen taken in during respiration, the red corpuscles run through their stages of organic development, and at last become dissolved. In this process, as in other cases of cell development, chemical changes occur—the globulin is converted into fibrin, the hematin into hemaphæin.

§ 86. In inflammation there is reason to believe that this process of organic development of the red corpuscles, in consequence of increased absorption of oxygen, takes place with increased activity. The red corpuscles, as already said, become dissolved in increased quantity, the globulin being converted into fibrin to be afterwards oxidated, or perhaps at once converted into oxyprotein.

The expenditure of oxyprotein in nutrition or in exudation at the inflamed part does not keep pace with its production by solution of the red corpuscles—hence its increased quantity; and the reproduction of the red corpuscles does not keep pace with their solution—hence their diminished quantity.

§ 87. *What are the circumstances in inflammation which determine the changes in the blood just described?* That the production of hyperinosis stands in some direct connexion with the local inflammatory process, and is not the consequence of supervening febrile excitement, may be inferred from the fact ascertained by Andral and Gavarret, that in the pyrexiæ there is hypinosis instead of hyperinosis, but should a local inflammation supervene in the course of a fever, that hyperinosis takes place.

§ 88. According to this, instead of inflammatory fever being a cause of the change in the blood, the change in the blood might perhaps be viewed as the cause of inflammatory fever.* To this it has been objected, that there are cases—cynanche tonsillaris, for example—in which fever distinctly precedes any local symptoms; but is not this premonitory fever, altogether different from that which supervenes after the local inflammation has developed itself?

§ 89. Supppose that the process of hyperinosis goes on, in part at least, in the vessels of the inflamed part, it might be said that the ordinary nutritive process being suspended, the oxygen of the stagnant blood, instead of acting on the tissues, acts on the red corpuscles themselves, and determines their conversion into oxyprotein. This is received into the circulating mass—perhaps in part exuded—and the place of the red corpuscles which were resolved into it is supplied by others stagnating; the cause of the inflammation continuing in operation.

§ 90. On this hypothesis the increased heat of the inflamed part seems to be readily explicable, especially when it is considered that in the red corpuscles there is an increased and constant supply of fuel†

TERMINATIONS OR EVENTS OF INFLAMMATION.

Inflammation terminates either in mortification or in the healing process.

I. NATURE OF MORTIFICATION.

§ 91. Mortification is the death of the part affected, together with the blood stagnant in it, and the matter which may have been exuded.

§ 92. The proximate cause or the condition on which mortification immediately depends, is complete stoppage of the circulation; either from all the vessels of the part being the seat of the inflammatory stagnation, or sometimes together with pressure exerted by the exuded matter, the operation of the pressure being favoured by the mechanical conformation of the part.

§ 93. The dead part being the same as a foreign body, the organization tends to throw it off. This separation is effected, as Dr. Billing remarks, by softening of the dead part from decomposition. It not being the object of this Report to enter further into the subject of mortification, the reader's attention is now requested to the *healing process.*

II. NATURE OF THE HEALING PROCESS.

§ 94. In the course of the healing process, epiphenomena may occur, which, on account of their prominence and practical importance, have been spoken of as separate and distinct terminations or events; but the inflammation may not terminate on their supervention, and it is obvious that in the abstract they are merely species of the healing process and this though their tendency may sometimes be to interrupt rather than promote the cure.

§ 95. With this explanation, *resolution*, *adhesion*, *suppuration*, and *granulation*, are to be looked upon as different species of the healing process, in which inflammation may terminate. Only it is to be remarked in regard to resolution, that it being the immediate and direct process by which the healing of inflammation takes place, it is also truly a *termination* of inflammation.

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* Watson's Lectures on the Practice of Physic, vol. i, p. 171.

† Compare further on this point Simon's Anthropochemie, p. 182; and Williams's Principles of Medicine, § 437 et seq.

§ 96. Inflammation itself, it is known, may, by virtue of the species of the healing process in which it tends to terminate, act as a means of cure of some other disease. Again, one species of the healing process may counteract the wrong direction which another species is taking. And lastly, the termination in mortification itself, may come in as a curative process.

§ 97. As stagnation and exudation are the essential parts of inflammation, so the essential objects of the inquiry into the healing process of inflammation are how the circulation is reestablished in the part, and what becomes of the exuded matter.

III. MICROSCOPICALLY-VISIBLE PHENOMENA ATTENDING THE REESTABLISHMENT OF THE CIRCULATION.[*]

§ 98. If the stagnation has not already existed long, and if the apparent fusion of the red corpuscles is not to any great degree, reestablishment of the circulation is observed to take place in the following manner. Oscillation of the agglomerations of corpuscles within the vessels occurs, such as was observed at the commencement of the stagnation. The red corpuscles resume their individual distinctness, and are disposed readily to become detached from each other, so that at every stroke of the heart, some are forced into neighbouring anastomosing vessels in which the circulation is going on, and are carried along in the stream. Sometimes the whole of the corpuscles within a vessel, are seen to be thus at once set afloat.

§ 99. If the stagnation has already existed for some time, and if the apparent fusion of the red corpuscles is in a great degree, reestablishment of the circulation does not so readily ensue. In this case the red corpuscles are not observed to become again distinct individually, but the agglomerations of them having become loose in the vessels, and being at every stroke of the heart made to protrude a little into the neighbouring vessels into which they open, and in which the circulation is going on, pieces of them are detached in the form of irregular flocculi which are carried along in the stream of the circulating blood, where the red corpuscles appear to be at last separated from each other, and to become individually distinct, being at the same time restored to their natural collapsed state, by the action of the plasma.[†]

§ 100. Though the circulation has thus become reestablished, the vessels may be observed still to have an unusual quantity of colourless corpuscles accumulated on the inner surface of their walls, so that in comparison with their natural caliber, but a very small stream of blood is allowed to thread its way along.[‡]

§ 101. Besides colourless corpuscles, some red ones also continue here and there adhering to the walls of the vessels, but still pale and distended. At length, however, they may be seen to become detached, and to be carried along in the stream.

§ 102. The absorption of exuded matter, and the disappearance of the red tinge of the adjacent parenchyma which had resulted from the transudation of hematin eventually ensue.

§ 103. Lastly, it is some time before the dilated vessels fully recover their usual caliber.

IV. EXPLANATION OF THE MICROSCOPICALLY-VISIBLE PHENOMENA ATTENDING REESTABLISHMENT OF THE CIRCULATION.

§ 104. In the first place it is to be observed that though the vessels may be found still dilated, this is not to be assumed as an indication that nervous influence has not been again restored to them. The overstretched fibres are unable all at once to contract as usual; and as regards the capillaries, they have not recovered their elasticity weakened by over distension.

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[*] See Hastings, *ut supra*, and especially Emmert, *ut supra*.

[†] Sometimes as reestablishment of the circulation occurs at one place, a new stagnation makes its appearance at another.

[‡] This circumstance of the reestablishment of the circulation in vessels in which a large accumulation of colourless corpuscles still exists would alone, in the absence of other reasons, be sufficient to show the unfounded nature of Dr. Williams's opinion, referred to in last Report, that accumulation of colourless corpuscles originally determines the stagnation of the red ones.

§ 105. The reestablishment of the circulation may be said in a general way to depend on the cessation of the disposition which the red corpuscles have to remain aggregated, and to adhere to the walls of the vessels, in consequence of which the red corpuscles now yield to the *vis a tergo*.

§ 106. As to the cause of this cessation of the disposition of the red corpuscles to remain aggregated, and to adhere to the walls of the vessels: If the opinion as to the immediate cause of the stagnation of the blood enunciated in the former Report, §§ 75, 76, be well founded, it may be naturally inferred that the return of nervous influence to the walls of the vessels is the cause sought for. When, however, the changes in the stagnant blood are taken into consideration, (§§ 1, 2, 4, 6, 9,) it must be admitted that besides restoration of nervous influence, some other cause is in operation.

§ 107. As the apparent fusion of the red corpuscles which supervenes more or less quickly on stagnation, even when the mass of blood is healthy, is like that which is presented by the red corpuscles of buffy blood, and appears to be owing to the action of plasma inspissated as regards its protein constituents, but deprived of its salts by exudation of serum, (§§ 4, 6, 9,) so the additional cause sought for, may perhaps be found by comparing with the behaviour of the red corpuscles in the process of reestablishment of the circulation that presented by the red corpuscles of buffy blood out of the body, after the coagulable plasma has been by their close aggregation pressed out from among them, viz., that the red corpuscles have then lost much of their disposition to remain aggregated, as is shown by the little cohesion of the under part of the clot. This, without going minutely into the matter, may be said to be owing partly to the abstraction of the coagulable plasma from among the red corpuscles, and partly to the direct action on them of the serum, which is by and by separated. The addition of a saline solution at the same time that it causes collapse of the distended corpuscles, disposing them to separate.

§ 108. To apply the inference which may be drawn from the comparison now made in explanation of the process of reestablishment of the circulation in inflammation. In consequence of the abstraction of coagulable plasma from among the red corpuscles stagnant within the vessels by exudation, and the reabsorption of the serum into the obstructed vessels, (§§ 7, 8,) the red corpuscles are disposed more readily to separate from each other, and from the walls of the vessels, and thus to yield to the *vis a tergo*.

§ 109. Hence, as the explanation offered by Henle of the immediate cause of stagnation was rejected as such, (see former Report,) but adopted (in the present Report) as applicable to the production of the apparent fusion of the red corpuscles, (§ 9,) so the explanation which Henle offers of the apparently fused state of the red corpuscles, viz. reabsorption of serum into the obstructed vessels, (§ 7,) whilst it is rejected as such, (§ 8,) is here admitted to play a part at least in the process of reestablishment of the circulation.

§ 110. The explanation which Henle himself offers of the process of the reestablishment of the circulation is this: "Whilst the tendency of the vessels to contract returns, the plasma on the one hand must acquire the normal state, or at least one the opposite of the inflammatory, so that the red corpuscles readily separate from each other again; the impulse of the arteries on the other hand becomes stronger, on account of the contraction." (Bericht, &c., p. 162.)

§ 111. According to Emmert, "by the exudation of a part of the stagnant and changed blood, (which exudation is known to have occurred by the red colour of the adjacent parenchyma,) the mass of the contents of the vessels becomes less, and the cohesion of it appears therefore to become weaker, for close to where this mass meets by anastomoses with the circulating blood, it is seen to be readily detached and carried away."

§ 112. The probability that some of the stagnant red corpuscles may dissolve, has been above admitted (§§ 12, 89); but unless the conditions above considered (§§ 105-7) were in operation, this solution could not contribute to the reestablishment of the circulation. The cause of the inflammation continuing, the vessels would be refilled by stagnating blood as fast as they were emptied. This, however, would explain the long continued exudation which sometimes goes on at one place.

§ 113. The fate of the exuded matter[*] constitutes, as above mentioned, one of the essential objects of the inquiry into the nature of the healing process of inflammation, but before proceeding with this inquiry, it is necessary to consider more particularly than has yet been done, the exuded matter itself.

V. EXUDED MATTER.

§ 114. Exudation takes place either into the interstices of the parenchyma, or on surfaces natural or raw. In the case of a natural surface, its epidermis, or its epithelium if it be resisting, is raised up into a blister by the exuded matter.

§ 115. As already said, the exuded matter from being at first serous, comes at last to contain a greater or less quantity of fibrin or fibriniform matter, (oxyprotein,) and in this state is a clear viscid fluid, usually called lymph.[†]

§ 116. As this fluid is the same as the plasma of the blood, it of course has the same properties, physical and chemical. It may remain fluid or coagulate, and having coagulated, the serum may or may not be separated. In the latter case, the exuded matter is gelatiniform, in the former the coagulum is consistent or diffluent, according to the proportion of fibrin or oxyprotein contained in the exuded matter. The serum which is separated, may either be soon removed by absorption or collect.

§ 117. Examined microscopically, the recently-exuded matter appears quite amorphous, without any trace of organization, except that when coagulated, it may be more or less indistinctly fibrous, and covered with oil-globules, appearances which, however, have nothing in common with the organization which afterwards ensues.

§ 118. The corpuscles which the fibrinous matter very soon after exudation is found to contain, have been alleged to be the colourless corpuscles of the blood, which have escaped from the vessels; but this is not the case. As already said, none of the corpuscles of the blood pass out along with the exuded fluid as long as the vessels are entire.

§ 119. The corpuscles in exuded matter are new formations developed after exudation, developed in it in fact, as in a blastema.[‡]

VI. ABSORPTION OF EXUDED MATTER.

§ 120. If much matter has not been exuded, absorption of it takes place, as the circulation in the obstructed vessels becomes reestablished, and this the more readily if the exuded matter is still fluid.

VII. DEVELOPMENT OF ORGANIC ELEMENTS IN THE EXUDED MATTER.

§ 121. When absorption does not take place, the exuded matter becomes organized; in it, as in a blastema, organic elements are formed. The process by which this takes place is altogether the same in principle as that by which the normal development is originally effected. At the same time that the exuded matter becomes changed morphologically, it is changed chemically; thus, for example, it may now yield gelatin (pyin), no trace of which, Mulder affirms in opposition to Bouchardat's statements, is found in the buffy coat.

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[*] See, on this subject, Vogel's article 'Entzündung,' in Wagner's Handwörterbuch der Physiologie, p. 341 et seq.; and Henle's 'Bericht.'

[†] The exudation of lymph from a wounded surface was found by Dr. John Thomson, to take place in four hours after the wound; by Sir A. Cooper, in six hours in dogs, and twelve or fourteen in man.

[‡] The colourless corpuscles observed stilll accumulated on the inner surface of the walls of the vessels in which the circulation has become reestablished, (§ 100,) appear to be the same as those referred to by Mr. Travers in his recent work on Inflammation, under the name of lymph-globules or particles. The organic elements formed in the exuded matter as in a blastema above spoken of, (§ 119,) appear to be what Mr. T. describes as the separated lymph-particles, and which he thinks are the material of new structures. They are, he says, separated subsequently to the effusion of liquor sanguinis. The deposit effused with the liquor sanguinis which is on the contrary an amorphous exudation, and serves merely for the intermedium of organisation in a close apposition of surfaces (being separated from the liquor sanguinis with which it is effused,) would not serve as a base for the new solid; it is soon absorbed, being only a temporary band or adhesive layer in harmony with the parts, though serving the important purpose of consolidation by anastomosis of the contiguous vessels of opposite sides, or union by the first intention.

§ 122. There are three principal ways in which the exuded matter has been found to be developed and disposed of.

1. After having attained a certain development, the organic elements are, if formed in the interstices of a parenchyma, dissolved and removed by absorption, or if formed on a surface, thrown off.

2. The organic elements undergo further development into tissues, homologous or heterologous.

3. The organic elements formed in the exuded matter are nucleated cells, pus-corpuscles, which undergo no further development, but are discharged along with the fluid in which they are suspended, (liquor puris,) in the form of pus from the body, in greater or less quantity.

§ 123. *First form of organization of exuded matter.* The elements in respect of form, which are in this case developed in the exuded matter are the "filled cells" of Henle,—the "granule-cells" of Vogel,—a certain stage in the development of which is represented by the "compound inflammation globules" of Gluge.

§ 124. According to Vogel, the granule-cell is nucleated. It is at first from 1-3375th inches to 1-1125th inches, and gradually grows until it reaches the size of 1-900, 1-675th inches. The contents of the cell are at first in small quantity. Afterwards the cell contains a large quantity of small dark granules, like elementary or pigment granules, so that from being at first transparent and colourless, the cell afterwards becomes quite opaque, from the colour of its contents, assumes even a brown or black tint, and looks like an aggregation of granules, the nucleus, and frequently also the cell-wall being quite imperceptible. The addition of water or acetic acid, however, by distending the cell wall, brings it into view.

§ 125. Acetic acid at last dissolves the cell-wall, but leaves the nucleus and granules. Ammonia or potass dissolves both cell-wall and nucleus, but not the granules. These, however, are in general soluble in ether, whence it would appear that they are composed of fat.

§ 126. When by the action of acetic acid, the wall of the cell is dissolved, and the granules scattered, the nucleus comes into view. It is distinguished by its size, granular appearance, and generally by the existence in it of a nucleolus.

§ 127. According to Vogel, the granule-cells are capable of no further development, but after reaching their full size, and becoming quite filled with granules, they begin to undergo a retroceding metamorphosis. The nucleus and cell-wall disappear, the granules remaining, and for a time still held together by a connecting substance; in this stage constituting the compound inflammation globules. Ultimately they become completely separated from each other.

§ 128. After this breaking up of the granule-cells, the whole exuded matter originally present is converted into a semifluid pappy mass, consisting of the separated granules suspended in the serum of the originally exuded plasma, in which state it is readily absorbed, the serum first, and lastly, the granules after being dissolved.

§ 129. Henle gives a different view of the succession in the stages of development, and retroceding metamorphosis of the organic elements under consideration.

The organic element in the state of the so-called "compound inflammation globule," which is an agglomeration of minute semitransparent granules held together by an amorphous gelatiniform mass, but without any membranous envelope, he supposes to be the first stage, and to result from elementary granules aggregating together. In the second stage the agglomeration of granules becomes surrounded by a membrane; whence a granule-cell results. The granules then dissolve gradually from within outwards, leaving only a few to represent the nucleus.

§ 130 Granule-cells may be formed along with pus-corpuscles in the matter exuded, on an inflamed surface, e.g. a mucous membrane in catarrh, and may be thrown off entire. Bühlmann* correctly considers the compound inflammation globule as identical with the *corpuscula granulosa* found in the sputa in chronic catarrh, and which is composed of an aggregation of corpuscles which he calls *albuminous corpuscles.* What Bühlmann describes as *melanotic cells* appear to be the same in the *stage of granule-cells*, the granules being dark. Many of such cells are found in the sputa, without the granules being dark.

§ 131. Within these cells as they occurred in the sputa of a catarrh when going off, and which were of various sizes, some as large as 1-300th of an inch, the author of this Report observed smaller nucleated cells covered over and imbedded in the granules. These smaller cells varied in number from one or two to a great number, according to the size of the parent cell. This observation, of the correctness of which he repeatedly satisfied himself, recalled to his mind the development of cells from the embryo cell of the ovum, with which is connected the division and subdivision of the yelk. It is however to be remarked that in the latter case, the embryo cells metamorphose the yelk, in order to the development of the embryo, whereas the formation of cells within the granule-cells leads to nothing.

§ 132. *Second form of organization of exuded matter.* This consists in the conversion of the exuded matter into tissues. The process by which this is effected is in general altogether the same in principle as that by which the tissues are originally developed. It would be beside the purpose of this Report, to enter into any inquiry as to what tissues are capable of being regenerated from matter exuded in inflammation, still more would it be beside the purpose of this Report, to enter into any inquiry regarding the different modes in which different tissues are regenerated.

§ 133. Regeneration of epithelium or epidermis, of cellular tissue, and of capillary vessels, is all that it is necessary to direct attention to here.

§ 134. *Epithelium or epidermis.* The development of new epithelium or epidermis, in other words the skinning over of a sore on a mucous membrane or on the skin, is too well known to require more than mention here.

§ 135. *Cellular tissue.* When the cell-theory was first promulgated, it was supposed that both in the original genesis, and in the regeneration of cellular tissue from exuded matter, perfect nucleated cells required to be first formed, and that these cells by farther development, gave rise to fibres; Henle has, however, shown in his 'General Anatomy,' p. 198, that in the original genesis of the cellular tissue, perfect nucleated cells are not required, and he now expresses it as his opinion, that although nucleated cells round or elongated into flat fibres, are always met with where new cellular tissue is being formed, the principal mass of the fibres is developed in the following manner.

§ 136. There are formed numerous round nuclei, (exudation corpuscles,) in the cytoblastema, which elongate, becoming slender, and arrange themselves one after another in rows. At the same time the cytoblastema is resolved into flat fibres from 1-5600th to 1-3700th of an inch broad. On the surface of these flat fibres lie the nuclei which become partly absorbed, partly coalesce, to form nucleus and elastic fibres. The broad fibres either remain at this stage of development, in this state resembling the fibres of the middle coat of the arteries, or they split into finer fibrils, begin to curl and become true cellular tissue.

§ 137. Mr. Gulliver, appealing to the fibrous structure which fibrin presents immediately on coagulation, has been led to express doubts as to the universality of the application of Schwann's doctrine. Mr. Gulliver is quite correct when he says that he could never see any satisfactory evidence that the fibrils of fibrin are changed cells. Direct observation clearly shows that the fibrin is at once formed into fibres in the act of coagulating. But Mr. Gulliver is not correct in adducing the fibrous structure of fibrin as an argument against the development of fibres from cells.

§ 138. It has been above shown with Henle, that Schwann's theory required limitation, but the following remarks are given by Henle to serve as an answer to Mr.

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* Beiträge zur Kenntniss der kranken Schleimhaut der Respirationsorgane und ihrer Producte durch das Microscop, p. 52, Bern, 1843.

Gulliver's question, " What is the proof that these fibrils, (of coagulated fibrin,) may not be the primordial fibres of animal textures ?"

" In most cases the softening and solution of coagula, by means of which they are rendered fit for absorption, or changed into pus, depends on a process of organic metamorphosis, which at its commencement is not to be distinguished from reorganization. The first step of the process, or at least a change accompanying it, is the disappearance of the retiform fibrillation, if such was present. Without the coagulum losing its consistence, and often at a time when the new microscopical globular elements are first deposited in separate groups, those fibres become finer, paler, and at last invisible. They have thus no direct share in the formation of new tissues. To this I might admit one exception. The fibres of the striped coat of the arteries, and of the heart which lies immediately underneath the epithelium, or when this is wanting, which forms the free inner surface of the above-mentioned organs, resemble so closely in form and arrangement the fibres of fibrin, that we cannot refrain from supposing they both have a similar origin. In the fully-formed state the fibres of the striped coat are broader and darker, and do not change in acetic acid ; they are, however, met with also narrow, pale, and often scarcely to be distinguished. Admitting that the fibres of a fibrinous lamella have in consequence of some chemical change, lost their solubility in acetic acid, (as is the case with epithelium-cells,) we have a lamella of the striped vascular coat. It is especially by the production of numerous layers of this membrane, that the morbid thickening of the arterial walls is produced which precedes the so-called atheromatous degeneration. Whether the fibre which yields the material for this purpose is derived directly from the blood circulating in the vessels themselves, or exuded from the *vasa vasorum* is as regards this question, indifferent." (Bericht, p. 177.)

§ 139. *New vessels.* It is here proper to remark, in the first place, that it is a not uncommon though a very erroneous notion, that many of the vessels which for the first time become visible in an inflamed part—the conjunctiva for instance—at the commencement of inflammation are new productions. Not only is this not the case, but even in an advanced stage of an inflammation, when exudation has taken place, it may be that no new vessels are formed, although the exuded matter undergoes development into other structures.

§ 140. On this point Dr. Hastings, more than twenty years ago, recorded the following results of his observations (ut supra, pp. 93-4): " In the foregoing experiments, if inflammation began and terminated without any lesion of the part affected, new vessels were never formed. After the most attentive scrutiny, the author is induced to believe, that the apparent formation of new vessels is always caused by numerous capillaries becoming dilated, and carrying an arterial red blood. In all cases, however, in which inflammation arises from wounds, a whitish matter seems to be deposited by the divided vessels, when they are much dilated and full of an arterial red blood. After this matter has been deposited some time, small vessels can be seen in it, which communicate with the inflamed capillaries of the incised part. Thus, through the medium of these new vessels, a slow circulation is at first effected; but by degrees the recently formed substance becomes more vascular, and then a free communication ensues between the capillaries of the opposite edges of the wound; and the newly deposited matter at length assumes an appearance nearly similar to the rest of the web."

§ 141. Before considering this question further it is also proper to remark, that what is often meant by " organization" is the development of new vessels ; but as has been already said, organization of exuded matter may take place quite independently of the development of blood-vessels. The development of new vessels, in fact, is in itself a process of organization which presupposes the development or organization of other tissues. New vessels are not formed for the purpose of " vitalizing" " effusions of the organizable materials of the blood," for such effusions are already vitalized. It is from such effusions that the new blood-vessels themselves are developed, and that along with the development or organization of other tissues, such as the cellular. The blood-vessels are formed in order to fetch and carry away the materials concerned in the nutrition and further development of these tissues.

§ 142. All the best observations on the development of new vessels hitherto tend to the establishment of the proposition that it takes place in this way. At the same time that the new cellular or other tissue is being developed cells are formed, which coalesce with and open into each other, and form networks of capillary vessels, at first quite separate from and independent of the old vessels of the part with which they only afterwards enter into communication, as Mr. Hunter supposed.

§ 143. The opinion, so generally entertained, that the new vessels are merely prolongations of the old, appears to have arisen from the circumstance that, in the ordinary modes of examination, the new vessels have not been recognized except in advanced stages of development, when they have already formed a communication with the old; for, until this communication be established, the new vessels cannot be injected, either naturally or artificially. This also explains the apparent suddenness with which new vessels are formed.

§ 144. But it is to be remarked, that what has been sometimes taken for new vessels may be referred, as Henle points out, to a deceptive appearance, viz., to blood effused into the interstices of the network of fibrinous fibres, the red corpuscles of which blood have agglomerated into rolls, and these again into branched and retiform figures. Henle thinks that the appearances described by Andral, Hasse, Skoda, and Kolletscha, as rapid vascularization and formation of blood in pleuriditic exudations, may have been of this nature.

§ 145. The newly developed tissues having served a purpose which was merely temporary, may undergo a retroceding metamorphosis in order to be absorbed, as in the preceding case. As examples of this, may be mentioned provisional callus uniting fractured bones, the new vessels formed in the cornea, &c.

§ 146. *Third form of organisation of exuded matter.—Suppuration.* The elements, in respect of form, which are in this case developed in the exuded matter, are *pus-corpuscles.* Pus corpuscles are globular cells measuring, on an average, 1-2250th of an inch in diameter, finely granulated on the surface, and sometimes presenting an indistinct appearance of a nucleus. By the action of water or dilute acetic acid, the cell-wall becomes distended and transparent, so that the diameter of the corpuscle is increased, and the nucleus comes distinctly into view. And it is now seen as if composed of two or three, more rarely four granules, usually separate and distinct from though closely applied to each other. Sometimes, however, the nucleus granules are seen to be continuous and homogeneous throughout, forming one multilobed whole.

§ 147. Pus-corpuscles are suspended in a considerable quantity of the serum of the originally exuded plasma (*liquor puris*)—the whole forming a sort of emulsion-like fluid, well known under the name of *pus.*

§ 148. Pus is developed from matter exuded either in the interior of organs, in the cellular tissue, &c., or on the surface of organs or open wounds. In the first case the pus collecting forms an abscess, and in the second case there is a suppurating surface.

§ 149. Pus-corpuscles are not capable of any further development, but they, and the fluid in which they are suspended, are expelled from the body. The pus of an abscess in general tends to the surface, for the purpose of being evacuated. The matter which forms on a surface, either on the exterior of the body or the wall of a cavity communicating with the exterior, may be considered as already evacuated.

§ 150. In some cases, pus not being expelled from the body the liquor puris is absorbed, and the corpuscles becoming broken up are also eventually removed by absorption. The process, as described by Vogel, is this: When the pus is not artificially evacuated, and does not make a way for its own escape, it may undergo still further changes. The pus-corpuscles gradually collapse, the pus is converted (especially when the serum is absorbed) into a thick, grumous mass, or into a thin fluid mixed with greasy flocculi, and the mass, as well as the flocculi, appear under the microscope as a matter quite undefined, consisting of small granulary molecules, generally under 1-11250th of an inch in size, which are not to be distinguished from other dissolved and decomposed organic matters, such as, e. g., encephaloid or tuberculous mass, from organic detritus in general. In this state, but only in this, the pus is capable of being wholly absorbed.

§ 151. It is thus seen that whilst the granule-cells developed in matter exuded into the substance of organs are as a regular result broken up, dissolved, and absorbed, the pus-corpuscles developed in a similar situation tend to the surface, in order to be evacuated directly from the body, absorption occurring merely as a rare exception to this rule.

§ 152. But it is to be remarked that granule-cells are also sometimes found in pus, or, as above shown, in puriform sputa, and are in this case directly expelled from the body.

§ 153. *Genesis of pus-corpuscles.* According to Henle and Vogel, pus-corpuscles are formed in the exuded matter thus: In the exuded matter small bodies, sometimes single, generally joined together in twos and threes, first make their appearance. These are the nuclei. Around them the cell-wall is gradually formed. This cell-wall is at first delicate and quite transparent, but afterwards becomes finely granular, by which its transparency is impaired and its nucleus either rendered indistinct, or altogether concealed.

§ 154. There are differences of opinion as to whether the division of the nucleus above mentioned as being generally shown by the action of acetic acid be an early and progressing stage, or a late and retroceding stage. According to Henle and Vogel, it is an early and progressing stage. The small bodies seen applied to each other in twos and threes are elementary granules, which gradually coalesce to form a simple nucleus, but the earlier the stage of development, they are the more readily disjoined by the action of water or acetic acid.

§ 155. According to Vogel the blastema in which pus-corpuscles are formed is either the *already coagulated* fibrin of the exuded plasma, or the plasma with its fibrin *as yet uncoagulated* The first is the case in all pus-formations in the interior of organs, in the cellular tissue, and, in short, in all cases in which the exuded matter has time to coagulate before the commencement of organization. The second is the case in regard to pus which appears on the surface of organs, or in cavities which open externally, on all mucous membranes, on the surface of the cutis, in open wounds, &c.

§ 156. According to Vogel, when the pus-corpuscles are formed in a blastema of already coagulated fibrin they are at first observed here and there, as if imbedded in the amorphous or indistinctly fibrous blastema. Afterwards the whole exuded matter is gradually converted into pus-corpuscles. These, as soon as they have reached their full development, separate from each other and become mingled with the serum which had separated from the fibrin at the time of its coagulation. The pus-serum is the original blood-serum poured out along with the blood plasma, perhaps somewhat modified. It is only when the pus-corpuscles become mingled with it that fluid pus presents itself.

§ 157. It is to be believed that at the commencement of a phlegmonous abscess, pus is produced in the manner just described, but when some has already collected it is probable that what is subsequently formed is developed on the inner surface of the walls of the abscess, in the same way as pus on a suppurating surface, the plasma, as fast as it is exuded, being formed into pus-corpuscles.

§ 158. Suppuration on a mucous surface, for example, may be said to be an excessive but imperfect development of epithelium cells, and which are thrown off with morbid rapidity. The suppuration on a wounded surface may, to a certain extent, be viewed in the same light, the anormal epithelium in the form of pus being intended to protect the surface.* So also the formation of pus on the walls of an abscess (pyogenic membrane). The pyogenic membrane is a new vascular structure, somewhat analogous to granulations, developed from the exuded matter. The production of anormal epithelium in the form of pus, originally determined by the irritation of a foreign body, or of a bit of dead cellular tissue, or dead coagulated fibrin, is kept up afterwards on the surface of the pyogenic membrane by the irritation of the matter itself.

§ 159. *Conditions determining whether one or other of the above-described modes of development of the exuded matter shall take place.* Whether one or other of the above-described modes of development of the exuded matter shall take place, appears to depend on the quantity and quality of the exuded matter, and the place where it is deposited. These, again, depend on the nature, degree, and seat of the inflammation. The quality of the exuded matter will also, of course, depend on the condition of the blood, and this again will depend partly on the inflammation itself, and partly on the state of the constitution of the individual in whom the inflammation exists.

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* In order to protect the new tender granulations, they are covered with Pus. (Billing's Principles of Medicine.)

§ 160. As regards particular states of the constitution—Mr. Dalrymple* has called attention to "the rapid organization of lymph in cachexia;" meaning, by "organization," the development of new vessels, or vascularization.

§ 161. It is quite true that lymph exuded within the eye, for example, in inflammations occurring in cachectic states of the constitution—in syphilitic iritis, in scrofulous iritis—more frequently become vascular than in cases of iritis occurring in otherwise healthy subjects. But is this not explicable by the circumstance that in such cases as the former the inflammatory action subsides less quickly and perfectly, and the exuded matter is accordingly less readily absorbed than in cases of healthy inflammation?

§ 162. The circumstance that the lymph which is exuded in drops on the iris in syphilitic iritis at first yellow, afterwards assume a red hue more or less marked, is supposed by Mr. Dalrymple to denote organization, i.e. vascularization of it, but might not the redness be owing either to dissolved colouring matter, or, as in the cases above referred to, (§ 144,) to extravasated red corpuscles?

§ 163. *Is blood in substance capable of reorganization?* Mr. John Hunter was of opinion that it is, Dr. John Thomson adduced very good reasons against it, more lately Mr Dalrymple has published a case which he thinks is an example of it. Dr. Pirogoff also has published affirmatory observations, and on the question Professor Henle † remarks: "It is frequently, but incorrectly affirmed, that plastic exudations only, but not blood in substance, are capable of reorganization. The development of thrombus, of the corpora lutea in the ovaries, &c., is a decided contradiction to this opinion."

§ 164. In considering this question, it is to be remembered that to answer it in the affirmative it would not be necessary to demonstrate the existence of new vessels, though if *new* vessels did exist they would be a proof not only of organization, but of a somewhat high degree of it.

§ 165. The proofs of the organization of blood in substance, which have been adduced, have been principally the existence of blood-vessels, as evidenced by the blood coagula admitting of being injected. But in such a high degree of development or organization, the mass of the coagulum should have been itself found already converted into tissue—cellular tissue for example. Has this been proved to be the case? And supposing it to have been found to be the case, who will say that it was an example of blood in substance organized, rather than of exuded lymph coloured with effused blood?

§ 166. In his case Mr. Dalrymple‡ does not speak of the mass of the blood-clot otherwise than if it were still in the form merely of coagulated blood, though "minutely organized with innumerable new and minute vessels," whose arrangement was so determinate and uniform as to leave no doubt of the entire dissimilarity of the organization of this tissue, from the periosteum on the one hand and the bone on the other; in short, that though the larger vessels might be old, the rest were all new.

§ 167. If the mass of the clot was still blood in substance, Mr. Dalrymple endeavours to prove too much from the existence of the vessels. If, on the other hand, the mass of the clot was composed of tissue interwoven with blood-vessels, then the case is no proof of the organization of blood in substance; for, to repeat, Who will say that no lymph was originally exuded, that it was blood alone which was originally effused?§

§ 168. Pirogoff's observations, quoted by Henle,|| in support of the opinion that blood in substance is capable of organization, are as follow: "The new substance which unites the cut ends after section of the tendo Achillis is produced, according to Pirogoff,¶ from effused blood. If the section is so made that little blood is effused

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* Med Chir. Trans. for 1840. † Bericht. ‡ Loc. cit.

§ Dr. Macartney says that he has succeeded in forcing an injection into the coagula formed in the cavities of the heart after death, which injection presented the appearance of red elongated lines! (On Inflammation, p. 51.)

|| Bericht, p. 210. ¶ Ueber die Durchschneidung der Achellesschne, Dorp. 1840, p. 18.

(from the skin towards the bones), the sheath of the tendon collapses, its walls grow together, the cut ends of the tendon become thick and clubbed, and the function is not reestablished. In some cases injection of blood into the sheath of the tendon, when performed immediately after the operation, served as a substitute for the natural bleeding." There is no proof here that the effused blood forms the new substance.

§ 169. In regard to the organization of the corpora lutea, the following are the conclusions to which the observations hitherto made by the author of this Report seem to point.

In the first place the matter, whatever it is, out of which the yellow body is developed, is effused into the interstices of the stroma of the ovary immediately outside the walls of the Graafian follicle. The vessels and fibres found in the yellow body, are not of new formation, but the original ones which connect the walls of the Graafian follicles with the stroma of the ovary, and which are stretched and separated by the effusion into their interstices of the matter whatever it is, from which the proper substance of the corpus luteum is developed.

§ 170. The corpus luteum may therefore be excluded from the question of vascular organization.

§ 171. As to the proper substance of the corpus luteum.—The microscopical elements of this have been found somewhat different in different cases. It has in one case been found composed of masses of elementary granules, and imbedded in them pale delicate cells having somewhat of the appearance of red corpuscles of the blood bleached and collapsed by the action of reagents, except that they were larger and nucleated. In another case along with the minute elementary granules, a considerable number of larger bodies, apparently of the same substance, and which resembled that of the nuclei of pus-corpuscles, for example. Some of these bodies seemed to contain nucleoli, and in several instances there was the appearance of a very pale cell-membrane around them, of about double the diameter of the red corpuscles of the blood. In a third case, besides the elementary granules, there were merely shapeless particles like shreds of broken-up cells.

§ 172. From such observations, coupled with the well-known fact that the corpus luteum at length disappears, the author of this Report has been led to the opinion that the matter exuded into the stroma of the ovary around the Graafian follicles, to form a corpus luteum, attains merely a certain degree of development, and then undergoes a retroceding metamorphosis, in order to be rendered fit for eventual absorption, as in the process described at §§ 127-8. The fibres and vessels in the yellow body being the same fibres and vessels which connected the outside of the Graafian follicle with the stroma of the ovary.

§ 173. The masses of elementary granules may be compared to granule-cells in the stage of the so called compound inflammation globules. As to the nucleated cells, they might be viewed as formations developed within the masses of elementary granules, in a manner similar to what is above described in regard to the granule-cells contained in the sputa of a fading catarrh. (§ 131.)

§ 174. Seeing that the corpus luteum does thus present a certain degree of organization, the point to be determined in reference to the question under consideration is whether or not the matter from which the corpus luteum is developed be blood in substance.

§ 175. The matter is, according to all observers, at first *bloody looking;* but the author of this Report is not aware if any microscopical observations have been made, showing that it is pure blood, and not fibrinous exudation mixed with blood, or showing that if at first pure blood, this does not give place to fibrinous exudation before organization commences.

§ 176. The only observation at all bearing on the point which the author of this Report has himself made is the following : A very large and recent corpus luteum. The appearance which a section of it presents to the naked eye, somewhat like that of a clot of blood. Here and there only a trace of the characteristic yellow appearance of the fully-formed corpus luteum can be detected. Membraneous bands composed of the vessels and fibres between the burst Graafian follicle and the stroma of the ovary, seen extending through the clot-like mass.

§ 177. As to the microscopical characters of this mass: It consists of granulous corpuscles like compound inflammation globules, closely aggregated, and some red blood-corpuscles interspersed among them. The granulous corpuscles appear of a yellowish red tinge, as if they had imbibed colouring matter from the red corpuscles which are somewhat bleached and shrivelled.

§ 178 From this it is certain that there had been blood effused; but the presence of perfect red blood-corpuscles cannot be admitted as a proof that the granulous corpuscles were formed from them. Were the granulous corpuscles formed from the fibrin of the plasma of the effused blood, or were they developed from exuded lymph, the presence of the red blood-corpuscles being owing to extravasation of blood occurring in addition to the exudation?

In other cases it has been sufficiently proved that the so-called compound inflammation globules are not formed, as Gluge supposed, from red blood corpuscles.

§ 179 The reestablishment of the circulation and the fate of the exuded matter having been considered, a review may now be taken of them in their correlations as they are exhibited in the different species of the healing process.

VIII. RESOLUTION OF INFLAMMATION.

§ 180. When resolution of inflammation of a vascular part, visible externally, takes place, the pain and heat cease, the redness disappears, the swelling subsides, and the part is at last restored to its natural state.

§ 181. When resolution of inflammation of the cornea above referred to takes place, the irritability of the eye ceases, and, on the one hand, the congestion of the neighbouring conjunctiva and sclerotica diminishes; then, on the other, the exudation in the substance of the cornea disappears.

§ 182. Resolution of inflammation consists in the reestablishment of the circulation, and absorption of the exuded matter. The exuded matter may have been still fluid, and may not have become in any way organized, but it is probable that in many, if not most cases apparently ending in resolution (such as it has just been defined), the exuded matter has first become organized into granule-cells, as above described. These are then either thrown off from the surface on which they were formed; or if formed in the interstices of a parenchyma they are, by the process of retroceding metamorphosis, again rendered fluid in order to be absorbed. This constitutes what Vogel calls resolution of exuded matter.

§ 183. "It is," says Vogel, (p. 345) "especially observed after inflammations of *internal organs*,—the brain, lungs, spleen, liver, &c. In most chronic inflammations of the cerebral substance and inflammatory softenings of this organ there is formation of granule-cells. In all inflammations of the lungs in which the patients died from other causes after the occurrence of resolution, as ascertained by the general phenomena as well as by auscultation, I found this process. On external parts, in the cellular tissue, in muscles, on membranous organs, the formation of granule-cells as an event of inflammation is more rarely observed, perhaps, for the reason that there is seldom opportunity to examine carefully external parts after inflammation which has not passed into suppuration, but become resolved. After internal inflammation of the eye, in consequence of needle operations, in which the patient died from other causes some time after the operation, I twice found formation of granule-cells in the exuded matter poured out from the inflamed parts into the interior of the eye."

The author of this Report, as above shown, considers the *corpus luteum* of the ovary as a physiological example of a transitory formation of a similar nature.

IX. HEALING OF A WOUND.

§ 184 *In the healing of a wound* conversion of the exuded matter into tissues takes place under two different kinds of circumstances. And according as one or the other obtains, there is what surgeons call "healing by the first intention" or by "adhesion," or "healing by the second intention" or by "granulation and suppuration."

§ 185. The stagnation and exudation, and therefore the redness and swelling of the part, do not cease on the occurrence of either of these processes. They continue until the healing process is accomplished, the stagnation, or at least a slow circulation in the part, being the necessary condition of exudation; the exudation again that of the supply of material wherewith regeneration is effected.

§ 186. In the last Report the cornea was referred to as a part which, from its non-vascularity, forms a good subject for observing, in a manner uncomplicated by the presence of the vessels in which the blood is stagnant, the development and disposition of the exuded matter in the healing process. The matter exuded into the substance of the cornea from the vessels of the adjacent conjunctiva and sclerotica may (as far as examination with the naked eye, or the eye assisted merely by a magnifying glass goes,) be seen to undergo the different modes of development above described.

§ 187. It may be seen that pustules form and that, in the healing of incisions and ulcers of the cornea, adhesion and granulation, and, lastly, cicatrization, may take place without the development of new vessels—a convincing proof that neither suppuration nor "organization" necessarily implies the development of new vessels.

§ 188. But, as hinted in the former Report, new vessels may be formed in the matter exuded into the cornea, and this again affords a very interesting and readily observable example of the development of new vessels, uncomplicated by the previous existence of other vessels in the part.

§ 189. Here the mode of development of new vessels, as described above (§§ 142-3), is to be recalled to mind, in order to guard against the very obvious though erroneous inference, that the new vessels are produced merely by the shooting of the old vessels of the conjunctiva, for example, into the cornea; the new vessels not being visible until, in consequence of an inosculation with the old, they become injected with red blood.

§ 190. The new vessels, having served their purpose, shrink and disappear, and it is not until this that cicatrization is completed. (§ 145.)

§ 191. *Healing by the first intention or adhesion.* In this case the matter exuded on the cut surfaces becomes forthwith, and all of it, converted into tissues—cellular tissue and capillaries—by which the divided parts are reunited. An epithelium or epidermis is then formed on the surface in the ordinary way, and cicatrization is completed.

§ 192. *Healing by the second intention, or granulation.* This is a slower process than adhesion. The inflammatory congestion persisting matter continues to be exuded. One part of it is converted into cellular tissue and capillaries—granulations are composed of these new tissues in process of development; another part is converted into pus, which, as above said, serves as a sort of epithelium to the granulations.

§ 193. As the healing approaches completion, the quantity of exuded matter converted into pus becomes less and less in comparison with that expended in the formation of tissues. At last, no more pus being formed, the exuded matter is developed into epithelium or epidermis, and cicatrization is in this case also completed. As this takes place the granulations contract and become less vascular by the shrinking and disappearance of many of the vessels which existed in them, as is so distinctly observable in the case of the cornea, in regard to all the vessels.*

§ 194. That the tissue of cicatrice is not quite homologous with the old tissue is very evident in the case of the cornea by the resulting opacity.

§ 195. The newly-formed tissues—whether their formation be by the adhesive process or by granulation—besides effecting union of divided parts, or supplying the place of lost parts (regeneration), may be formed in excess. If the tissues thus developed in excess be homologous, *hypertrophy* is the result; if heterologous, *tumours* of different kinds, *induration*, &c.

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* The process going on in an ulcerating sore is the opposite of that going on in a granulating sore. For whereas, in the latter, the exuded matter is developed partly into tissues, partly into pus; in the former the exuded matter is not only not organized into tissues, but those at the surface of the part lose their vitality and are thrown off, as Mr. J. W. Earle (on the Nature of Inflammation, in Medical Gazette, vol. xvi, p. 254, London, 1835,) and Dr. Billing remark, in minute portions with the discharge, which is either a mere sanies, or at the most an imperfectly developed pus.

This death of the ulcerating surface is not owing to any destructive action of the exuded matter which constitutes the discharge, but is owing to the same condition which determines the death of the exuded matter itself. From this it is seen that *ulceration* belongs to the head of *mortification*.

OBITUARY—Dr. Barlow.

[For the following brief memoir of Dr. Barlow's life and writings we are indebted to the pages of the Provincial Medical and Surgical Journal. Although written (as there stated) by "one of Dr. Barlow's intimate friends," (Mr. Soden,) it is far from over-colouring the character or over-stating the merits of that excellent man and accomplished physician. We speak from long personal knowledge of his worth and we say deliberately, we have never known a man possessed of more gentlemanly and honorable feelings, or of a more truly philanthropic, liberal, and patriotic spirit than our lamented friend.]

Edward Barlow was born at Mullingar, county of Meath, Ireland, on the 25th of June, 1779. His father was a distinguished practitioner at that place, and died August 9, 1825, at the advanced age of eighty-one. The Westmeath Journal, in announcing his death, stated that "he was for fifty-six years surgeon to the County Infirmary, and for the greater part of that period, until years and infirmity limited his exertions, in possession of the almost undivided medical practice of the surrounding country—a sure proof of the zeal and ability with which he exercised the arduous duties of his professional calling."

Dr. Barlow commenced his professional education under the guidance and direction of his father, who, from an early period, intended to bring up his son to the medical profession, in the expectation that he would succeed to his private practice, and public appointment at the Infirmary. In furtherance of these objects, and to enable him to become a Licentiate of the Dublin College of Surgeons, he was bound apprentice to his father, and after the expiration of his apprenticeship, attended the various lectures and hospitals in Dublin, Edinburgh, and London; at the latter place he was a pupil of Cline and Cooper, at the Borough Hospitals. He became a Licentiate of the Dublin College of Surgeons in May, 1801, and graduated at Edinburgh in June, 1803.

He resided in Dublin, and practised as a surgeon till 1807, when he came to Bath, and commenced that career as a physician, which soon led to celebrity and distinction. He cultivated medical science with zeal and assiduity, and communicated to the public the result of his reflections in various writings. He became physician successively to the Bath City Infirmary, (previously to its junction with the Casualty Hospital, when the two charities formed the United Hospital), and to the General Hospital. He performed the duties of physician to both these Hospitals in the most assiduous and laborious way. He was scarcely ever absent on his allotted days of attendance, and generally visited both Hospitals daily. At the United Hospital he usually spent from three to four hours in investigating and prescribing for the complaints of the out-door applicants, and the patience with which he listened to their histories. and the kindness of his demeanour towards them, made him a great favorite with this class. His exertions, however, were not limited to objects connected with his profession. He was the zealous and eloquent advocate of every project that was calculated to extend knowledge, promote humanity, or in any way to be useful to his fellow-creatures. He was one of the original founders of the Literary and Philosophical Institution, and ever afterwards an active member of its Committee. He was chiefly instrumental in establishing a Phrenological Society in Bath, and was always ready to furnish a paper at their meetings, when no other contributor could be found. He was a zealous supporter of the Humane Society. Amongst the poor who applied as out-patients at the United Hospital he often found that great misery arose from want of proper clothing, and to remedy in some degree this evil, he formed a charity for the purpose of distributing flannel waistcoats to such objects as appeared likely to be benefited by them. This charity is now attached to the Hospital, and has been productive of great comfort to many poor persons who were suffering from rheumatism.

Indeed, it is not saying too much in praise of Dr. Barlow, to assert that his benevolent feelings led him to support with his pen, his purse, and his personal exertions, every object of public charity and utility.

It is scarcely necessary to remind the readers of this journal how warmly he was interested in the prosperity of the Provincial Medical Association We have all admired his zeal, and been delighted with the eloquence with which, at our annual meetings, he always advocated every measure that was likely to promote the welfare of the society, or advance the honour and respectability of the profession. At our future assemblies his absence will indeed be deplored, and we shall all feel that we have sustained a loss which cannot be easily supplied.

Dr. Barlow's private practice was considerable as a consulting physician, but not so extensive in regard to general practice as might have been expected from his talents and his zeal, united as they were with the advantage of being connected with two hospitals, both of which afforded ample experience, and presented favorable opportunities of observing disease. There can be no doubt, however, that if he had desired more extensive private employment he might have obtained it; but he loved the science and disliked the trade of medicine. He was sufficiently affluent to be satisfied with a limited income from his professional exertions, and a belief generally prevailed, that professional emolument was a matter of indifference, or, at least, of secondary importance, to him.

In all the social and domestic relations of life he was most exemplary. He was a kind husband, an affectionate father, and a sincere friend. His manners were at times reserved, but he was generous and benevolent. His habits were retired, and he was not fond of much society, but no one could receive his friends with greater hospitality and courtesy. He had a deep sense of religion, which he manifested, not so much by the observance of austere forms, as by a conscientious discharge of all the duties of life. His health began to decline about two years before his death, when he experienced a severe domestic calamity, from the shock of which he never recovered. His changed appearance at the meeting of the Association at Leeds was observed by all his friends, and from that time he progressively declined. Latterly, his emaciation became extreme, and he finally sank under the effects of disease on the 2d of April in this year.

The governors of the Bath Hospital met on the 1st of May, to elect a successor to Dr. Barlow, and before they separated the following resolution was unanimously passed, the whole body of governors simultaneously rising, as a mark of respect to the memory of one who was so universally and deservedly esteemed: "That this court desire to record the expression of their unfeigned sorrow at the decease of the late senior physician, the lamented Dr. Barlow, and their gratitude for the services which, during the period of twenty-five years, he so cheerfully and efficiently rendered. Whether they regard his skill, his humanity, his attention to the sick and suffering, or his conduct as a member of the honorable profession to which he belonged, they feel that his departure has left a blank which will not easily be filled."

Dr. Barlow has been long known to the medical profession by the extent and value of his writings, which are characterized by elegance of composition and copiousness of illustration. He wrote with great fluency and remarkable rapidity; such indeed was his ready command of language that he appeared to have the power of exhausting every subject on which he treated, almost without an effort. This facility of expressing his ideas led him probably to an early as well as a frequent use of his pen, for his first contribution to medical literature was written while he was a student. It is impossible to enumerate all Dr. Barlow's publications, as, besides his more important works, he wrote in newspapers, and other periodicals, on various topics of local and temporary interest; but the following list contains his chief medical productions:

'History of a considerable wound of the Brain, attended with singular circumstances, by Mr. Edward Barlow, student of Medicine at Edinburgh, from Westmeath, Ireland.' (Duncan's Annals of Medicine for 1802.)

'Dissertatio Medica Inauguralis, de Peritonitide Puerperarum.' Edinburgh, 1803.

'Case of Dysphagia, together with some other unusual affections, supervening on inflammation of the lungs, wherein a gum-elastic tube was advantageously employed as a passage to the stomach.' (Dublin Medical and Physical Essays, for June, 1807.)

'Observations on Medical Reform; illustrating the present condition of medical science, education, and practice throughout Great Britain and Ireland, and proposing such alterations therein, as appear most likely to succeed in remedying the several evils which abound in this profession, and which have at length become subjects of universal complaint.' Dublin, 1807.

Dr. Barlow's other works on Medical Reform were

'An attempt to develope the Fundamental Principles, which should guide the Legislature in regulating the Profession of Physic.' (Edinburgh Medical and Surgical Journal, vol. xiv.)

'Exposition of the present state of the Profession of Physic in England, and of the Laws enacted for its Government.' (Edinburgh Medical and Surgical Journal, vol. xvi.)

'An Essay on the Medical Profession, showing its natural unity, and suggesting such arrangements as would render its condition conformable to just principles of Political Science, and conducive to the interests both of the profession and the public.' (Edinburgh Medical and Surgical Journal. vol. xxviii.)

'An Essay on Medical Reform.' (London Medical Gazette, vol. xiii.)

Besides these articles, Dr. Barlow wrote the Reports that were presented to the Provincial Association on Medical Reform.

Dr. Barlow's other contributions to the Medical and Surgical Journals were:

'Pathological and Practical observations,' (Edinburgh Journal) occupying part of seven numbers in vols. ix and x.

'Case of laceration of the fibres of the gastrocnemius muscle, treated without rest or confinement.' (Ib. vol. xix.)

'A case of bronchocele successfully treated by iodine.' (Ib. vol. xxi.)

'A case in which chalybeate pills were retained for an unusual time in the intestines.' (Lancet, March, 1827)

'An account of a tape worm.' (Midland Reporter, No. 13.)

'On spinal weakness, and some effects of incipient curvature.' (No. 14.)

'On dropsy, with coagulable urine.' (Ib. No. 16.)

The Transactions of the Provincial Medical and Surgical Association are enriched by the following productions of his pen:

'The objects and modes of medical investigation.'

'Biographical memoirs of the late Dr. Thackeray, of Bedford.' (Vol. i.)

'The retrospective address delivered at the first anniversary of the Association, held at Bristol in the year 1833.' (Vol. ii.)

'Records of ovarian tumours.' (Vol. iv.)

'Address at the meeting of the Association, at Bath.' (Vol. vii.)

'Address on vacating the chair, at Liverpool. (Vol. viii.)

He wrote the following articles in the 'Cyclopædia of Practical Medicine:'

'Antiphlogistic regimen; Congestion of blood; Determination of blood; Physical education; Gastrodynia; Gout; Plethora; and Rheumatism.

The 'Essay on Physical Education,' has been translated into most of the continental languages, and is generally admired.

In 1822 he published 'An Essay on the Medicinal Efficacy and Employment of the Bath Waters, illustrated by Remarks on the Physiology and Pathology of the Animal Frame, with reference to the Treatment of Gout, Rheumatism, Palsy, and Eruptive Diseases.'

It will be perceived that the subject of medical reform occupied Dr. Barlow's attention at an early period of his life The pamphlet published at Dublin in 1807 was a reprint of letters previously inserted in a newspaper. It was dedicated to Lord Henry Petty, the present Marquis of Lansdowne. The author did not attach his name to it; but in his preface says that "he is veiled in an obscurity which curiosity shall in vain attempt to penetrate." It may not therefore be generally known that this work was written by Dr. Barlow.

Medical reform was a favorite theme with Dr. Barlow throughout his life; and how zealously and ably he vindicated its cause must be in the recollection of most of the members of the Provincial Medical Association. He was examined before the Parliamentary committee appointed in 1834, to inquire into the state of the profession; but his evidence was never printed, having been lost in the fire that destroyed the Houses of Parliament.

I ought to have included in the list of his works one which, though not strictly medical, is on a subject sufficiently connected with our profession to be deemed interesting to and worthy of consideration by medical men. I allude to a pamphlet which he published in 1825 (anonymously), and entitled 'An Apology for the Study of Phrenology,' in which the author maintains its truth and asserts its utility. Dr. Barlow became a convert to this science at the amiable and lamented Spurzheim's first visit to Bath in 1814, and was ever afterwards its zealous advocate. He was a regular attendant at the meetings of the Phrenological Society during its existence, and read three papers on phrenological subjects before the members in 1840 and 1841.

The tribute which I now offer to the memory of our departed friend is very inadequate to his merits, but it is written by one who was long and intimately acquainted with him; one who admired his talents, esteemed his virtues, and will ever sincerely lament his death. It was not my intention to give a complete history of his life; and if this brief sketch of his character should disappoint the expectations of his friends, they may derive consolation from the assurance that his works have secured him a lasting fame with his profession, that his memory will be held in grateful and honorable remembrance by his numerous friends in that Association of which he was not only one of the founders, but one of its most useful, active, and eloquent members.

BOOKS RECEIVED FOR REVIEW.

1. Traité complet de l'Anatomie, de Physiologie, et de la Pathologie du Système Nerveux Cérébro-spinal. Par M. Foville, M.D., &c. Prem. Part, avec un Atlas de xxiii Planches.—Paris, 1844. 8vo, pp. 676.

2. An Essay on Pneumo-Thorax. By H. M. Hughes, M.D.—London, 1844. 8vo, pp. 37.

3. The actual Process of Nutrition in the Living Structure, demonstrated by the Microscope, &c. 5s.

4. Prize Essay on the Nature and Objects of Medical Science. By P. H. Williams, M.D.—London, 1844. 8vo, pp. 52.

5. Medicines, their Uses and Mode of Administration. By J. M. Neligan, M.D., Lecturer in the Dublin School of Medicine.—Dublin, 1844. 8vo, pp. 432. 12s.

6. Geology. By D. T. Ansted, F.R.S. Part I. London, 1844. 5s.

7. A History of British Fossil Mammalia and Birds. By R. Owen, F.R.S. &c. Part I.—London, 1844. 2s. 6d.

8. Outlines of Pathology and Practice of Medicine. By W. P. Alison, M.D., &c. Part II.—Edinb. 1844. 6s.

9. On Conical Cornea. By J. H. Pickford, M.D., —Dublin, 1844. 8vo, pp. 16.

10. Illustrations of the Theory and Practice of Ventilation, &c. By D. B. Reid. M.D., F.R.S.—London, 1844. 8vo, pp. 541. 15s.

11. The Results of the great Operations of Surgery, during a Practice of 22 Years; showing a high rate of success, at the Liverpool Infirmary. By J. P. Halton.—Liverpool, 1844. 8vo, pp. 34.

12. Reply from the Surgeons of the Liverpool Northern Hospital to a Pamphlet published by J. P. Halton—London, 1844. 8vo, pp. 39.

13. General Report of the Royal Hospitals of Bridewell and Bethlem. London, 1844. 8vo, pp 81. 7s. 6d.

14. Chemistry, as exemplifying the Wisdom and Beneficence of God. By George Fownes, PH.D.—London, 1844. 8vo, pp. 184.

15. The principal Offices of the Brain and other Centres. By J. Swan.—Lond. 1844. 8vo, pp. 31.

16. Scrofula; its Nature, Causes, and Treatment, &c. By W. Tyler Smith, M.B.—London, 1844. 8vo, pp. 172. 7s.

17. Practical Observations on the Prevention, Causes, and Treatment of Curvatures of the Spine. By S. Hare, Surgeon. 2d edition, revised and enlarged. London, 1844. 8vo, pp. 177.

18. An Essay on the Tongue in Functional Derangements of the Stomach, Bowels, &c. By E. Williams, M.B. Cant.—Lon. 1843. 8vo, pp. 119.

19. A Treatise on the Use of the Sympathetic Nerve and its Ganglions. By T. B. Procter, M.D. Lond. 1844. 4to, pp. 47.

20. The Chemical and Physiological Balance of Organic Nature. By M. J. Dumas and M. J. B. Bossingault. 3d ed. Lond. 1844. 12mo pp. 156.

21. Mesmerism and its Opponents; with a Narrative of Cases. By G. Sandby, jun., A.M., Vicar of Hinton.—London, 1844. 8vo, pp. 278.

22. A Dictionary of Medical Science. By R. Dunglison, M.D. 4th ed.—Philadelphia, 1844. 8vo, pp. 771.

23. Homœopathy Unmasked. By A. Wood, M.D.—Edinburgh, 1844. 8vo, pp. 196. 4s. 6d.

24. A Manual of the Practice of Medicine: the result of fifty years' experience. By W. C. Hufeland. Translated by C. Bruchausen, and R. Nelson, M.D.—London, (New York ?) 1844. 8vo, pp. 630.

25. The three Cardinal Means of the Art of Healing. By W. C. Hufeland. — New York. 8vo, pp. 79.

26. On Dysmenorrhea and other Uterine Affections in connexion with Derangement of the Assimilating Functions. By Edward Rigby, M.D.—London, 1844. 8vo, pp. 138.

27. Remarks on the use of Vivisection as a means of Scientific Research. By R. Jameson —London, 1844. 8vo, pp. 52.

28. Facts and Observations on the Sanitary State of Glasgow during the last Year. By R. Perry, M.D.—Glasgow, 1844. 8vo. pp. 37.

29. Chemistry simplified in its application to the Testing of Alkalies, &c. By Andrew Ure, M.D. F.R.S. 8vo, pp. 84.

30. The Nature and Treatment of Deafness and Diseases of the Ear. By W. Dufton, M.R.C.S. —London, 1844. 8vo, pp. 118. 4s.

31. On the Nature and Treatment of some of the more important Diseases, Medical and Surgical. By J. C. Hall, M.D. 2d edition, enlarged. —London, 1844. 8vo, pp. 247. 7s. 6d.

32. First Lines for Chemists and Druggists, preparatory to examination, &c. By J. Steggall, M.D.—London, 1844. 18mo. pp. 170. 3s. 6d.

33. Remarks on the Efficacy of Matico as a Styptic and Astringent. By Thos. Jeffreys, M.D. 2d edition.—Liverpool, 1844. 8vo, pp. 37.

THE

BRITISH AND FOREIGN MEDICAL REVIEW,

FOR OCTOBER, 1844.

PART FIRST.

Analytical and Critical Reviews.

ART. I.

1. *Guide du Médecin Praticien, ou Résumé général de Pathologie Interne et de Thérapeutique appliquées.* Par F. L. J. VALLEIX, Médecin du Bureau central des Hôpitaux de Paris, etc. Tome I.—*Paris*, 1842.
Guide of the Practical Physician, or General Summary of Internal Pathology and Therapeutics, clinically applied. By F. L. J. VALLEIX, Physician to the Central Bureau of Hospitals of Paris. Vol. I.—*Paris*, 1842. 8vo, pp. 566.
2. *The Practice of Medicine; a Treatise on Special Pathology and Therapeutics.* By R. DUNGLISON, M.D., Professor of the Institutes of Medicine in Jefferson Medical College, Philadelphia, &c.—*Philadelphia*, 1844. *Second Edition.* Two Volumes, 8vo, pp. 632, 683.

THE writer of one of the works whose titles stand at the head of this article, M. Valleix, in his well-written and well-reasoned preface, traces —and we think successfully—the causes which in France have produced a demand for works of a practical character. The physiological school of Broussais, which, though in the main false, had mingled with it enough of truth to give it an air of great plausibility, had obtained such entire possession of the minds of men, that tradition was interrupted, medical "history was become a dead letter," the yoke of authority was broken. But this medical revolution was shorter-lived than have proved certain political convulsions. Observation directed to the object of really illustrating the point in dispute—and it was so directed by that first of medical logicians, Louis—speedily demolished, even before its founder had quitted the scene, that ill-based fabric, the physiological system. But the traditional lore derived from antiquity had been extinguished by this system, whilst this had in its turn fallen before close observation and logical reasoning; and the followers of Broussais, in other words, almost the entire existing generation of French physicians, were at sea without a compass. Hence has arisen a demand for safe guides of practice, from an immense majority of practitioners of France.

From similar but not identical causes—for the doctrines of Broussais had but little influence on the British mind—the same demand exists in this country. In the early part of this century, especially after the publication of the works of Laennec, the current of the public mind set strongly towards pathological anatomy. Great expectations were entertained from it, and were, to some extent, realized in the improvement of diagnosis. But this result was not of a nature perfectly to satisfy the spirit—essentially utilitarian—of the profession in this country. They might for a time study medicine as an abstract science, but it would only be in the expectation of improvements in the *art* speedily resulting from it. But these results did not necessarily or speedily flow from pathological research. To recognize and name a disease was found to be one thing, to cure it another: the latter did not flow as a corollary from the former: it occurred as a contingency infinitely more rare than was expected, and disappointment was the result. A change came o'er the spirit of the age: "we want books useful at the bedside," was the cry; and, at once as an indication of the existence of this demand, and as a supply to meet it, the press poured forth 'Cyclopædias of Practical Medicine,' 'Libraries,' 'Dictionaries,' and treatises on the same subject, in rapid succession, from Craigie to Watson. Our transatlantic brethren abate nothing, as is well known, of the practical and utilitarian character of the Anglo-Saxon race whence they are descended, and were just as likely as ourselves to be soon weary of contemplating and classifying morbid products as one would objects of natural history, provided they led to no tolerably prompt result in the saving of life or the alleviation of suffering. With them, too, the demand is for therapeutics, and to meet this demand we have, (with others,) the work of Dr. Dunglison.

At first glance it might seem that whilst from M. Valleix we should derive a view of the condition of French practice exclusively, the work of our *quasi* fellow-countryman, Dr. Dunglison, would convey a knowledge of treatment as pursued solely among ourselves and our descendants. We do not find in either work, however, these exclusive views. Both are rich in references to opinions and practices prevalent among the best authorities in Great Britain and France. This is as it should be. An interchange of the treasures of mind, at least, constitutes a commerce in which there is an augmentation of general wealth, without impoverishment of any one. The general scope of the works is the same, the main object of both being therapeutics; both furnish so much of symptoms, causes, diagnosis, and pathological character as serves to render them intelligible guides to practice; and though their arrangement is not strictly identical, we can so adapt parts to each other as to furnish the reader with the opinions of both on any given disease at one view.

We select diseases of the respiratory organs as the ground on which first to show the characters of the works before us. Dr. Dunglison considers diseases of the larynx as the first in order in this department; not so, however, M. Valleix, who, prior to reaching the larynx, dedicates 180 pages to the classification, description, and treatment of diseases of the nasal cavities. We shall, however, commence at the lower point, and show how diseases of the larynx are handled by the respective authors.

Dr. Dunglison's classification of these diseases is exceedingly simple, viz. into acute, chronic, and œdematous laryngitis, designating by this last term what in this country and France is generally called œdema of the glottis—the name originally given to the disease by Bayle, who first described it, in which the sub-mucous cellular tissue of the larynx is occupied by a serous or sero-purulent infiltration. The division of M. Valleix is more complex, viz. into, 1st, simple acute laryngitis; 2d, simple chronic laryngitis; 3d, stridulous laryngitis, (pseudo-croup, laryngismus stridulus;) 4th, pseudo-membranous laryngitis, (croup;) 5th, œdematous laryngitis, (œdema of the glottis;) 7th, tumours of various sorts developed in the larynx; and, 8th, aphonia. It is evident from the respective classifications of the authors, that Dr. Dunglison has referred to a different compartment diseases referred by M. Valleix to the larynx. The former writer considers both croup and pseudo-croup (laryngismus stridulus) as laryngo-tracheal—correctly so, certainly, in the first case but with questionable accuracy in the latter.

Laryngitis. *Acute laryngitis* is the first disease of this class mentioned by both writers. In the account of this disease by M. Valleix we find more of system, more of the precise division of causes into predisposing and occasional, and more prolixity than in that of Dr. Dunglison; for though writers of our race are charged by a distinguished French author, Madame de Staël, with addiction to "longueurs," we cannot but opine that the charge of tediousness lies, with much more truth, against her own countrymen. French works, however, are so well put together, they are sent forth to the world so complete in all their parts, that we are led to pardon a somewhat undue consumption of time in their perusal, on the score of the workmanlike perfection and neatness of the manufacture. The symptoms of the acute and intense laryngitis of M. Valleix and of the merely acute of Dr. Dunglison nearly correspond. They are, pain in the larynx, never very considerable, increased in some degree by pressure on the upper edge of the thyroid cartilage. Deglutition is sometimes difficult; in some cases reported by English writers, says M. Valleix, liquids could not be swallowed. The same writer mentions the feeling as of a foreign body in the larynx as a symptom not less frequent than the preceding, and more distinctive. There is cough, attended, as the disease advances, with mucous expectoration—occasionally, Dr. Dunglison remarks, the sputa being tinged with blood. The voice is hoarse, but this hoarseness, at the expiration of a certain time, gives place to almost total extinction of voice, and, in a case reported by Dr. Wilson, (Medico-Chirurgical Transactions,) to its complete extinction. On auscultation of the larynx, says M. Valleix, there is a sonorous rhonchus or blowing sound (souffle), very intense and very harsh. M. Blaud has observed a very remarkable laryngo-tracheal mucous *râle.* Symptoms of asphyxia are naturally associated with the impeded passage of air through the larynx; hence the face is generally puffed and pale, the eyelids are swollen, and the lips blue, especially during spasmodic exacerbations, which are of frequent occurrence. Orthopnea, as might be supposed, generally exists. A comatose condition usually precedes dissolution. Fever, with a rapid pulse, accompanies the disease.

We annex from the work of M. Valleix a synoptical table of the diagnosis of laryngitis from affections with which it may be confounded:

“*Laryngitis.*	*Pharyngeal Angina.*
“Pain on a level with the upper edge of the thyroid cartilage, increased by pressure; deglutition of liquids little difficult.	Pain at the base of the lower jaw or the sides of the neck, augmented by pressure; swallowing very painful and difficult.
Voice hoarse, subsequently extinct.	Voice nasal and disagreeable.
Inspiration hissing; very great restraint of the respiration shortly after commencement of disease.	Inspiration simply painful; respiration is not restrained till a long time after the commencement of the disease.
Inspection displays only redness and swelling of the epiglottis.	Inspection displays swelling of the tonsils and the posterior part of the pharynx; the diagnosis is then very easy.
Simple Laryngitis.	*Croup.*
Expectoration of mucous, thready, feathery matters.	Expectoration of similar matters, but containing often fragments of laryngeal false membrane.
Inspiration as above.	Inspection often displays the presence of false membrane on the tonsils.
Simple Laryngitis.	*Acute ulcerated Laryngitis.*
Matters expectorated purely mucous. [Dr. Dunglison says they are sometimes bloody.—*Rev.*]	Streaks of blood in the matters expectorated. [A doubtful characteristic.—*Author.*]
Simple Laryngitis.	*Acute Œdema of the Glottis.*
Intervals of calm slightly marked.	Intervals of calm more marked. [A doubtful characteristic.—*Author.*]
Commencement of disease from a state of health.	Commencement in the course of an inflammatory affection, or of an acute ulceration in the vicinity of the larynx.
Simple Laryngitis.	*Foreign Bodies in the Larynx.*
Commencement gradual, though more or less rapid.	Commencement sudden.
Cough augmenting gradually in violence.	Sudden paroxysm of coughing, and at first of extreme violence.
Intervals of calm, as above.	Intervals of calm much more marked.
Fever from the commencement, and as much more intense as the local symptoms become more rapidly severe.	No fever at first, notwithstanding the severity of the local symptoms.”

We need scarcely remark that both our authors recommend vigorous antiphlogistic treatment for this affection when severe. Dr. Dunglison advises, as the first measure, the abstraction of blood from a vein, *till an impression is made on the system*, and the free application of leeches to the upper and front part of the neck. When the reaction which generally succeeds copious bloodletting takes place, he meets it by contra-stimulant doses of tartrate of antimony and potash, in doses ranging from one third of a grain to a grain, (anything under a grain we should consider a small dose in such a disease,) cathartics simple or cathartic enemata, and mucilaginous drinks. The patient should be forbidden to speak, and his chamber should be kept at a mild and agreeable temperature.

Should these not prove efficacious, a *revellent* treatment, he says, should be adopted, consisting of the free administration of mercury, so as to induce ptyalism. With this view, he recommends that either ten grains of blue-pill or one grain of calomel should be administered night and morning, whilst a drachm of mercurial ointment is rubbed into the thighs night and morning, and incisions made with the scarificator are dressed with the same ointment. Where spasms have intervened, and tracheotomy is not practicable, full doses of opium (three grains of soft opium in pill, or its equivalent in the preparations of opium or salts of morphia,) are indicated.

M. Valleix presents us his *methodus medendi* in a more detailed form than does Dr. Dunglison; but its general purport is, with an exception in the case of mercurials, very much the same as that of the latter gentleman. He arranges his plan of treatment in the form of prescriptions (ordonnances.) Of these the first for an adult is a light decoction of marshmallows and poppy-heads, edulcorated with gum-syrup; 2d, a bleeding from the arm, of twelve or fifteen ounces; 3d, fifteen or twenty leeches to the laryngeal region, to be repeated in the evening, if it appears necessary; 4th, vaporous fumigation, with the following decoction—marshmallows, about a drachm, and the head of a single poppy, boiled, in a quantity of water not specified, for twenty minutes. The vapour of this is to be directed during ten minutes towards the throat; the head to be covered carefully after each fumigation; the vessel containing the decoction not to be brought too near the mouth, in order not to augment the local heat: this process to be repeated night and morning. Two similar prescriptions, arranged for children aged from six to ten years, and for those from two to six years are then furnished, framed upon the same principle as the preceding.

M. Valleix admits, however, that the cases are rare in which we can limit ourselves to proceedings so simple; and that threatening suffocation soon compels the physician to resort to measures, if not more efficacious, certainly more energetic. Of internal means, he speaks favorably, for young children, of syrup of ipecacuanha, given in spoonfuls, till abundant vomiting is produced; for an adult, he recommends emetic tartar, in doses of one or two grains, combined with about a scruple of ipecacuanha, to be given in divided doses; or, he proposes, that two grains of emetic tartar should be dissolved in a pint of *tisan*, and that of this solution large drinks should be given, till vomiting and purging—which are productive of great relief—take place.

Under the head of external means we have every variety of rubefacient and blister mentioned: common blisters, ethereal extract of cantharides, frictions with croton oil, sinapisms, lotions of diluted muriatic acid—any of them to be applied near the part affected, or at some distance from it, as recommended by Drs. Arnold and Watson, (Med. Chir. Trans. Lond., vol. vi, p. 135, and vol. ix, p. 31;) but, excepting one, these various methods of accomplishing a rubefacient or vesicating purpose are all well known in this country. The one to which we allude M. Valleix praises, under the name of magistral blister (vésicatoire magistral), as a singularly prompt means of producing vesication; and we readily believe him. The following is the form: Take of powder of cantharides and of wheat flower each equal parts, and of vinegar a sufficient quantity to make a soft paste, to be applied to the skin. We think this form deserving of credit, both from the facility of its composition and, we feel convinced, for the promptitude and efficacy of its action.

We are subsequently presented (for the work is remarkable for the copiousness of its information) with a view of the practice adopted by different medical men in this disease. From this portion of the work we select the proceedings adopted by Dr. Henry Chavasse, in the case of a young girl, aged twenty, in the second day of the disease, with success. Blood was drawn from the arm, to the amount of forty ounces nearly, (1000 grammes;) eighteen leeches were applied to the neck; a purgative enema was given, and then a pill was administered every third hour, containing, (unless there be a very material typographical error,) fifty grains of calomel, and about one third of a grain of opium, along with a draught of liquid acetate of ammonia and camphor mixture.

These particulars will be found at p. 228 of the first volume of the work of M. Valleix. That a gross error exists there can be little doubt, at once from the extravagance of the dose of calomel, and the utter disproportion between the mercurial salt and the opium, although the table of *errata* throws no light upon it. We are sorry to observe such faults in a work destined for students of all ages, for the inexperienced may be led by them into errors actually fatal, whilst into those more advanced they are calculated to infuse distrust of a (generally) very valuable book. We think such faults very likely to occur, as the table of *errata* (though it has by no means comprehended all it might,) proves, from what we consider the very inconvenient mode adopted by the French, of stating all quantities in "*grammes*," or fractions of a "gramme," as a consequence of which writers are thrown, in expressing many quantities, into the decimal form, in which errors are very apt to occur. To take the example before us, the prescription runs thus:

℞	Calomel . . .	16 grammes.
	Opium, in powder . .	0·1 gramme.
	Conserve of roses .	q. s.

Make six pills.

Now we feel no doubt that the first line of the prescription should be written, "calomel 1·6 grammes;" in fact, that the printer, as he might easily do, has multiplied the dose of Dr. Chavasse by ten.

Even with this obvious and necessary correction, the practice of Dr. Chavasse, in the bleeding and mercury alike, is bold and decisive; but probably not more so than the circumstances of the case before him demanded, he having been called on the second day of a very severe disease. We do not hesitate to admit that it is more to our taste than the more moderate proceedings which win the favour of M. Valleix. For our own part, we never can treat acute laryngitis without having the narrowness of the chink of the glottis before our eyes. The treatment of Dr. Chavasse is not, however, the object of especial censure from M. Valleix; the former writer, nevertheless, must accept his share of the following general denunciation:

> "Calomel invariably constitutes part of the treatment employed by English physicians in acute laryngitis, and they all concur in praising it very much; but we should seek in vain in the facts for a confirmation of what they advance. In all cases, in fact, the calomel is associated with active means, such as blisters and even tracheotomy; and it is impossible to distinguish not only what is its effect,

but even whether it have an effect. We shall see in our exposition of the practice of Dr. Chavasse, how calomel is administered." (Valleix, t. i, p. 222.)

Had M. Valleix employed a treatment absolutely simple, had he relied on bloodletting alone, he might, perhaps, without presumption, have cavilled at the more complex proceedings of others. But, considering the nature of his practice, may we not with as much reason as he can plead, say, "Decoction of marshmallows and the head of a poppy form invariably part of the treatment recommended by M. Valleix, but being as invariably associated with other means, how do we know what is its effect, or whether it has an effect?" In a similar strain may we proceed with his fumigations, his ipecacuanha, his blisters, his emetic-tartar, and even with his bleeding—what is their effect, or have they an effect? M. Valleix seems to forget that medicine—if a science at all—is a science of observation; experiment has led some one to the conclusion that mercury does good in inflammatory diseases, laryngitis included; the information spreads, experiments are multiplied, and the universality of its employment by British practitioners—which M. Valleix mentions apparently as a censure—is evidence of the successful issue of many of these experiments. The experiment, too, of Dr. Chavasse was successful, which is more than M. Valleix ventures to predicate of his own more timid proceedings; the only case he mentions, besides that of Dr. Chavasse, which resulted in recovery, being one by Zacutus Lusitanus, who bled a pregnant woman seven times in one day on account of laryngitis, of which she was cured.

Other means failing to give relief, tracheotomy must be resorted to, says Dr. Dunglison; adding, however, and with truth, that it is of no use unless performed early, and before the patient is evidently sinking. Ryland has collected from various quarters twenty-eight cases of the disease; in six of these the operation was performed, and in four a cure was effected; in the other two temporary relief only resulted. The saving of the lives of two thirds, however, is a strong incentive to the operation. The operation, M. Trousseau says, frequently fails, from too small a canula being employed. He recommends a middle-sized canula at first, and gradually increases the size, until the air almost ceases to make a noise in passing through it during a deep inspiration.

M. Valleix does not appear to advocate *early* tracheotomy, as does Dr. Dunglison. He says that modern physicians are agreed that when suffocation is *imminent* the operation is indicated. Our own experience tells us that this may prove too late; for the last case we saw in which the operation was attempted, was under such circumstances, and the patient expired before it was completed. It is true that this operation was rendered tedious by the almost ossific rigidity of the rings of the trachea; but what we saw of the case convinced us that, without this untoward obstacle, the issue would have been the same. It appears to us, that, by delaying the operation till near the point of suffocation, we incur the risk indicated by Riverius, in words quoted by M. Valleix, of having the death of the patient imputed to the medical attendants: *propter metum infamiæ, quæ medicis et chirurgis impendet, dum æger post operatione occumbit;* and, what is much worse, of having deserved it.

Chronic laryngitis. The chronic form of laryngitis receives, as it merits, an ample share of the attention of both our authors. There is a copious reference to authorities in both of them, indicating an appeal to the same quarters by both. The names of Ryland, Andral, Louis, Graves and Stokes figure equally in the pages of the American as of the Gallic writer. The latter, with that fondness for method which characterizes him as an author, divides the disease into the slight (légère) and intense forms. We think the simpler plan of Dr. Dunglison — who gives the symptoms, with an intimation that cases vary in intensity—the more elligible one.

We copy the symptoms, as detailed by Dr. Dunglison:

> "Pain is felt in the larynx, but its precise situation may vary; at times it extends over the larynx, but at others is restricted to a small space, and generally to the region of the thyroid cartilage. Commonly a kind of tickling sensation exists, which provokes coughing. The pain, too, is exasperated by coughing, speaking and deglutition, especially when ulcerations exist, and they are situate above the ventricles of the larynx.
>
> "The breathing of cold air, and pressure upon the larynx likewise augment it. The voice is almost always changed, being hoarse and, at times, so much enfeebled as to be inaudible. The aphonia may supervene suddenly or gradually and ultimately be complete.
>
> "Cough is a constant concomitant; and when the mucous membrane is much swollen it becomes hoarse and even croupy. In the first instance it is dry, but subsequently it is accompanied with the expectoration of mucus, mixed occasionally with pus or blood. At other times a membraniform matter is expectorated for months, and at others a considerable quantity of false membrane is thrown off, after which the patient rapidly recovers.
>
> "Occasionally portions of cartilage are mixed with the mucus, or bloody sputa, and, in such cases, there is always accompanying hectic. Chronic laryngitis has indeed been divided into two heads: the first comprising that which affects the mucous membrane and the submucous tissue, and the second that which implicates the cartilages; the latter, it has been conceived, having perhaps the best claim to the name phthisis laryngea, from the incurable nature of the affection, and the hectic and emaciation which invariably accompany its latter stages. (Ryland.)
>
> "When chronic laryngitis is slight, and there is not much narrowness, the difficulty of breathing may not be great; but if it be attended with much tumefaction of the lining membrane the dyspnea is considerable, and the sound rendered, on inspiration, sonorous and peculiar. It is evidently, too, augmented by paroxysms. The air of inspiration likewise gives rise to a snoring (ronfflement), or whistling (sifflement), which may be continuous, or recur in paroxysms. These local symptoms may be so slight that the general health does not suffer to any great degree. Commonly, however, more or less sympathetic febrile disorder is apparent, under which nutrition falls off, and atrophy supervenes. The disease now merits the term 'laryngeal phthisis,' which is, however, in the immense majority of cases, connected with the presence of pulmonary tubercles. (Andral.)
>
> "Chronic laryngitis may be primary, or it may succeed to acute laryngitis; and when apparently terminating in health, it is readily reproduced by exposure to cold, errors in diet, &c. &c. Its duration varies from a few months to several years. When the fauces are inspected, but little evidence of disease may be perceptible; at other times, however, the mucous membrane is injected, and the follicles are so large as to resemble split peas. Whether this enlargement of the follicles is the cause or effect, may admit of a question. The enlarged follicles probably exist lower down, where they cannot be inspected. This form of laryngitis is the one often known under the name of "clergyman's sore-throat." Chronic laryngitis may terminate in health, but it is more likely to end fatally; and this may occur in different modes, either by the lungs becoming implicated, or by the extent of the laryngeal lesions themselves, which may excite severe irritative fever, or interfere with the entrance of air into the lungs, and thus in-

duce asphyxia. In almost all cases of phthisis laryngea the disease is complicated with pulmonary tubercle. (Andral, Louis, Stokes.)

> "Dr. Stokes, indeed, asserts that, after ten years of hospital and private practice, he never saw a case presenting the symptoms of laryngeal cough, purulent or muco-purulent expectoration, semi-stridulous breathing, hoarseness or aphonia, hectic, and emaciation, in which the patient did not die with cavities in his lungs. In some the laryngeal affection appears to be primary; but, in the great majority, symptoms of pulmonary disease existed previous to its appearance. Such also is the result of the author's observation. In many cases of pulmonary phthisis, sore-throat, hoarseness, or aphonia, with cough occur; but the case is different when the laryngeal symptoms have been primary. (Graves.)"—(Dunglison, pp. 235-7.)

We cannot help thinking that Dr. Dunglison has comprehended at least two different diseases in this description—what has been called (not very correctly) laryngeal phthisis and chronic laryngitis. We concur in the view of Dr. Stokes that those who manifest the symptoms of the former disease die with cavities in the lungs; but our experience does not lead us to the conclusion that the laryngeal affection is ever the primary condition in phthisis, as this able writer supposes it to be in a small proportion of cases. In all cases of what has been supposed to be laryngeal phthisis we have found on our first examination indubitable signs of cavities or other disease of the lungs already existing, so that our opinion would at the least accord with that of Andral, that "it is probable that idiopathic chronic laryngitis *rarely* [we should say *never*] produces the symptoms of phthisis; but the two diseases are frequent concomitants." In this latter clause we unequivocally concur; indeed the observations of the accurate Louis have shown that in one fourth of the cases of phthisis ulceration of the larynx has been observed; in one sixth ulceration of the epiglottis, and ulceration of the trachea was met with more frequently than either of the other lesions.

Now we cannot help thinking that to treat of such a condition as this, which is evidently part and parcel of phthisis, under the name of chronic laryngitis, and as an independent disease, is a gross violation of nosological precision, and, what is more, is calculated to lead to results practically pernicious. On this head M. Valleix expresses himself with much good sense:

> "Non-syphilitic and non-cancerous ulcerated laryngitis is *almost* (?) always a complication of phthisis pulmonalis. This is a point which we shall subsequently discuss. So that whatsoever enables us to recognize incipient phthisis (for in the case of confirmed phthisis there is no difficulty,) will serve to establish the diagnosis. Emaciation, cough, night-sweats, feebleness, and, above all, alteration of the sound of the thorax and of the respiratory murmur under the clavicles, ought to be carefully investigated, for none of these symptoms exist in simple idiopathic laryngitis. In some cases, the examination of the patient not having been made with due care, laryngitis alone has been supposed to exist, though this has not been the case: this proves that the diagnosis is sometimes very difficult, for the pectoral symptoms, which alone can guide us, are then very trifling." (Valleix, t. i, pp. 242-3.)

Dr. Dunglison's prognosis is rendered exceedingly and very unnecessarily gloomy by the confusion—he must pardon the expression—which appears to pervade his mind regarding the nature of the disease. When he says "chronic laryngitis may terminate in health, but is more likely to end fatally," he must be understood as expressing, in the first clause of the sentence, what is probable, and highly probable, with respect to the disease, which ought alone to have been considered on the present occasion; in the second he must be regarded as stating the truism, that phthisis is generally fatal. The prognosis of M. Valleix is of a very different description. He says:

> "According to those authors who have paid most attention to chronic inflammation of the larynx, simple laryngitis tends naturally to a cure; the excesses and imprudences of the patient alone keep up the disease and often aggravate it." (Valleix, t. i, p. 246.)

There is, however, a chronic laryngitis existing as an independent disease; and this is clearly exemplified in what Dr. Dunglison calls, and by no means inappropriately, the "clergymen's sore-throat." It is certainly not limited to gentlemen of the clerical profession; but they being more exposed than most classes of the community to one of its causes, inordinate exertion of voice, it prevails very much among them, especially among the younger and unseasoned portion. It should be remarked, however, that men whose vocation is not speaking would neglect an affection of the larynx, for which a clergyman, who is called upon once if not twice a week to address a large congregation, would hurry to his medical adviser for assistance:

"For he who lives to preach, must preach to live."

Hence it would not be correct to consider that it prevails among clergymen in comparison with the rest of the community, in the proportion in which the former class seek medical relief from it.

There is sometimes in this disease, for there is but one simple chronic laryngitis, whether it affects clergymen or others, injection of the mucous lining of the fauces, and enlargement of the follicles; whilst on the other hand these conditions are frequently not perceived, but the laryngeal affection is clearly denoted by very slight pain, increased by inspiring cold air, pressure and exertions of voice, when it becomes such as to compel the patient to cease speaking. The sensation frequently does not amount to pain, only to a very unpleasant tickling, but equally with pain exciting cough, and compelling the patient, if speaking, to stop instantly. The condition of the voice often varies in the same patient in the same day, being for a period perhaps little modified, at other times very hoarse, then muffled and very feeble so as to constitute almost total aphonia. There is sometimes no constitutional disturbance in the disease, and when any exists it is very slight. Simple chronic laryngitis is a common sequel of influenza.

Besides simple chronic laryngitis, and that form of affection of the larynx which is associated with phthisis, this organ may be affected with syphilitic and cancerous ulceration. There will not be any considerable difficulty in discriminating between these diseases and the simple affection now under consideration. The diagnosis of M. Valleix seems to us succinct and precise. The symptóms of cancerous laryngeal affection, he says, are emaciation, feebleness, languor; pale yellow complexion (cancerous cachexia); severe pain; crepitation sometimes produced by pressure on the cartilages of the larynx; expectoration of muco-purulent matters and cartilaginous *detritus;* and sometimes thickening, deformity, and ulcers of the epiglottis. Syphilitic disease of the larynx is to be distinguished by antecedent syphilitic infection; venereal ulcers, and eruptions in other parts of the body, exostoses, &c.; at an advanced period expectoration of muco-purulent matters, and syphilitic cachexia. There is, as a general rule, not much difficulty in discriminating syphilitic affections of the larynx from simple laryngitis or from phthisical disease of the organ. A case, however, in which we witnessed the confusion of the syphilitic with the phthisical form is fresh in our memory; but a syphilitic ulcer on the posterior pillar of the palate, the lower portion of which could be traced to the side of the larynx, where it was lost, and a little confidential conversation with the patient decided the case in our mind; whilst a moderate mercurial course decided it in the opinion of the family, for a patient supposed to be consumptive was in a month restored to perfect health.

The remedial measures suggested by both our authors may be stated to be those, in a mitigated and more continued form, recommended by them for the acute disease. Both mention bleeding from the arm as advisable in some cases at the commencement of the disease; leeches to the laryngeal region are suggested; and M. Valleix, on the authority of MM. Trousseau and Bellocq, cupping the nape. Counter-irritants (revellents) are praised by both; and the whole tribe of these remedies—blisters, ointment of emetic tartar, frictions of croton-oil, the application of caustic potash every eight days on the side of the larynx (recommended by MM. Trousseau and Bellocq), and a small seton under the skin on each side of the neck near the larynx (as suggested by Dr. Baillie), are passed in review. We need scarcely remark that tisans form part of the measures of M. Valleix. He recommends those of rather a stimulant or tonic character, and names a decoction of bardana sweetened with syrup of fumitory, or one of ground-ivy adulterated with syrup of capillaire. Medicines of the narcotic class form an important part of the prescriptions of both our authors. Laudanum and tincture of digitalis in conjunction are recommended, by Dr. Dunglison, to be given internally in doses of about six minims each three times a-day; and the same authority suggests the endermic employment of the salts of morphia, half a grain of the acetate or sulphate being sprinkled over a blistered surface. M. Valleix, from the same class, besides opium and lactucarium, the authority for which quoted by him is Dr. Walker, (in the London Med. Repository for 1822,) suggests the use of extract of hemlock, as prescribed by Dr. Baillie. These are a part of the many evidences furnished by his book of the really very extensive acquaintance of M. Valleix with British medical literature. The inhalation of narcotic vapours is recommended, especially by M. Valleix. The form he suggests is, infusion of elder-flowers about three pints, powder of stramonium a drachm nearly; the vapour to be inhaled, for which purpose a common teapot answers very well; or the same medicine may be smoked in a pipe, as prescribed by M. Cruveilhier. For this purpose, a scruple (nearly) of the dried leaves of the datura stramonium and an equal quantity of the leaves of sage should be used for a pipe, and this quantity may be smoked daily or even three times a day. The leaves of belladonna may be substituted for those of stramonium; but in this case the dose should be increased by about a fourth or fifth.

Topical applications directly to the membrane affected, either in a liquid state or in powder, are mentioned by both our authors. Dr.

Dunglison prefers them in solution, and regards the nitrate of silver as the best of the class. It may be used in the proportion of ten or fifteen grains to the ounce of water, as advised by Graves and Stokes, and it has been prescribed by MM. Trousseau and Bellocq as strong as one part of the nitrate to two of water. The plan suggested by Mr. Cusack, of Dublin, for its application he considers as the best. A brush of lint of the requisite size is sewed on the end of the finger of a glove, which is then drawn on the index-finger of the right hand. The patient is made to gargle with warm water, and the lint being dipped in the solution can be readily applied to the larynx.

The acid nitrate of mercury has been recommended for the same purpose as the solution of nitrate of silver; but M. Valleix cautions us against it on two grounds,—the danger attending its use, and the want of positive information as to any favorable result hitherto obtained from it. Dry powders have been recommended to be drawn or blown into the larynx. The powders, according to M. Valleix, used for the purpose are, 1st, sugar; 2d, subnitrate of bismuth, either pure or mixed with an equal weight of sugar; 3d, sulphate of zinc 1 grain mixed with 36 grains of sugar; 4th, sulphate of copper and sugar in the same proportions as the sulphate of zinc; 5th, alum, 5 grains mixed with 10 of sugar; 6th, acetate of lead, about 2 grains mixed with 14 of sugar; 7th, nitrate of silver, 1 grain mixed with 72 of sugar. M. Bretonneau has suggested a cumbrous apparatus for introducing these powders into the larynx; but the purpose seems better answered by an open glass tube, or, what was used by Aretæus in angina maligna for a similar purpose, a reed. Into one or the other of these instruments the powder is introduced, and the instrument is placed in the mouth; and by a sudden inspiration after a complete expiration, the powder is conveyed into the throat. A portion rests on the pharynx; another portion is conveyed into the larynx, and the patient is cautioned to repress coughing that it may rest the longer in contact with the laryngeal lining. The testimony in favour of this mode of medication is not so strong as for liquid cauterization with nitrate of silver.

Mineral waters taken at the spring are recommended by M. Valleix, on the authority of MM. Trousseau and Bellocq, and Louis. The waters recommended are the "Eaux-Bonnes," those of Cauterets, of Saint-Sauveur, and of Luchon. We have seen ourselves the greatest benefit from travelling, especially from a cold and damp to a warm and dry climate. The effect is not suddenly brought about, but we have seldom found it fail ultimately to effect a cure.

Œdematous laryngitis. That very distressing and dangerous disease, œdema of the glottis, is discussed by M. Valleix under the title of œdematous and sero-purulent laryngitis, whilst Dr. Dunglison simplifies the name by omitting the latter adjective. Both writers take much pains to indicate the diagnostic marks between this disease and acute laryngitis. Both are agreed that in the early stage of *œdema glottidis* its symptoms are simply those of a mild degree of the former disease. As the disease advances, the diagnosis becomes more clear. M. Valleix arranges the diagnostic marks in a tabular form. They are in substance as follows. In *œdema glottidis,* there are very violent paroxysms of dyspnea, in acute laryngitis the paroxysms are less marked and violent; in the former disease, inspiration is singularly sonorous, sibilant, and difficult, expiration being comparatively easy and exempt from sound, whilst in the latter, respiration is not marked by any peculiar noises, nor is there that discrepancy between its two parts, which is manifest in the former; in œdema of the glottis, the quantity of air entering the lungs being very small, the respiratory murmur is very feeble indeed, almost abolished throughout the thorax, whereas in simple acute laryngitis, this murmur is rather loud. Finally, in œdema, we can by carrying the finger to the upper part of the larynx, detect there a soft ring or cushion, caused by the infiltration of serous or sero-purulent fluid into the submucous cellular tissue, which is of course wanting in laryngitis.

The affection of the breathing is exclusive of the swelling round the larynx, one of the most distinctive symptoms of the disease. According to M. Valleix, inspiration is extremely difficult, requiring the greatest efforts on the part of the patient, and amounting frequently to true orthopnea. On the contrary, expiration takes place with so much ease, that M. Blache regards it as a pathognomonic sign. During inspiration there is often produced a wheezing, a guttural snoring, often very loud. One observer, M. Legroux, has remarked a peculiar sound like the crowing of a cock, or the sudden closing of a valve at the conclusion of each inspiration. Another indication of the great restraint of inspiration is the necessity which patients experience of keeping the mouth half open, notwithstanding which, the *alæ nasi* are expanded to the utmost at each inspiration.

This disease is generally fatal, Bayle having observed but one recovery out of seventeen cases. What is to be done to avert a fatal result? The pulse being often moderate, and there being seldom much heat of skin, antiphlogistic remedies are seldom required to the same extent as in laryngitis; yet leeches and blisters are generally advisable. Antimonial emetics afford relief, and with this view the potassio-tartrate of antimony may be given every two hours, in doses of two or three grains. Mercurial inunction is sometimes associated with the employment of the antimonial solution; or, as Ryland suggests, instead of the antimonial solution, five or ten grains of calomel are given every hour, with a view either from their purgative action, or from their revellent effect on the general system, of checking the inflammation of the glottis, and promoting the absorption of the effused fluids.

"Should these remedies," says Dr. Duglison, "have been employed, and attacks of impending suffocation be urgent, the operation of bronchotomy must be practised; but, as in cases of ordinary laryngitis, it must not be postponed too long, otherwise it also must fail. It has been proposed, that, the moment the nature of the disease is known, the operation should be practised, and that it should be followed up by the revellent treatment just recommended; (Vavasseur;) or that in its place, and the plan is preferred by some, an elastic gum-tube, open at both extremities, and of a proper size, should be introduced into the larynx from the mouth, so as to keep the passage open, (Bayle, Thuillier.) It is scarcely possible, however, to conceive that any benefit could acrue from the presence of an extraneous body in such a condition of the parietes of the larynx. [We should add, too, the difficulty of conceiving that such a body could for a moment be borne—Rev.] Another plan, scarcely, if at all, more feasible, is to pass the finger into the glottis, and to make pressure, so as to diminish their volume, (Thuillier;) and lastly, it has been proposed to scarify the engorged parts, (Lisfranc,) so as to permit the escape of the fluid. Both these plans have been characterized as fantastic, very difficult, if not impossible, of accomplishment, and more likely to increase than diminish the existing evil. (Ryland.) The last is undoubtedly extremely difficult of execution; but, if it could be carried satisfactorily into effect, it would offer great probability of benefit. The danger of the disease is, in some measure, proportioned to the extent of the tumefactions, and any method that would abstract the effused fluid could not fail to afford essential relief." (Dunglison, p. 242.)

It should be remarked that M. Lisfranc, (Journal gén. de Méd. t. lxxxiii, 1823,) who had the merit of first suggesting the scarification of the engorged parts, quotes five cases in which this operation was followed by immediate relief, and ultimately by a complete cure. In a sixth case many similar operations, performed at variable intervals, served only to palliate. There were deep lesions of the larynx, which ultimately destroyed the patient.

Stridulous laryngitis. We find ourselves very much puzzled to determine whether M. Valleix means by stridulous laryngitis, (laryngite striduleuse, pseudo-croup,) a totally different disease from the laryngismus stridulus of Dr. Ley, or whether he is taking a different pathological view of the same disease. He considers his stridulous laryngitis as a disease recurring in severe paroxysms with long intervals, during which there is very slight, if any, indisposition.

His description of the paroxysm is as follows:

"At the same time, the breathing is rapid, interrupted, and panting, and there is produced during inspiration an acute sound, more stridulous, more broken than that of hooping-cough, and which has been described by the names of stridulous, hissing, hoarse, sonorous respiration, and the crowing of a cock. It is especially the English physicians who have employed this last expression. These phenomena deserve all our attention, because for a long time they served to characterize croup; whereas it results from recent observations, that they belong rather to stridulous laryngitis than to croup properly so called. It is these peculiar cries which have procured for the disease the names of crowing inspiration, stridulous laryngitis, &c.

"When the child experiences a little relief, he complains or he cries, and then we can observe the modifications of the voice. It is hoarse and broken, yet always preserves a certain degree of force. I find in the writings of English authors, some cases in which *aphonia* is mentioned; but the subjects in which this symptom has been observed, were not attacked with a simple stridulous laryngitis; they experienced nervous attacks of which the suffocation and aphonia were a mere complication; sometimes, however, the voice may be extinct during the paroxysms. Jurine and M. Bretonneau have mentioned instances of it, but they are rare." (pp. 282, 283.)

"Whilst the infant experiences these distressing symptoms, he labours under great anxiety. His face is congested; his lips, especially, become violet; a little later, a frightful paleness is observed, and his eyes express terror. Dr. John Henderson (Observations on the Diseases prevalent among the Natives of Hindostan) has observed that the eyes are haggard. The patient throws his head backwards to breathe; he carries his hand to his neck; *there is sometimes a degree of distortion of his limbs, or feeble convulsions during the efforts of inspiration;* we discover, in one word, all the signs of asphyxia. Such is a paroxysm of stridulous laryngitis in its highest degree of intensity." (p. 284.)

The evidence adduced by M. Valleix for regarding this disease as a slight inflammation of the larynx is of the feeblest kind. He says that authors are agreed that there is little or no fever in the affection described by him; and in fatal cases pathological lesions in the larynx are very slight or null, "a little mucus, a feeble redness, have been observed in some cases, and, most frequently, authors have been content with saying that the larynx is exempt from change." He speaks of the lesions which some English authors have found in the membranes of the brain as indicative of a totally different disease from the one he is treating of, saying that in this case the principal malady was an arachnitis, of which the suffocation was only a symptom. "Dr. Clarke," he says, "attributes the disease to cerebral irritation; but it is a malady foreign to the larynx, *eclampsia*, which has led him into error." He speaks slightingly of the opinion of Dr. Ley, who attributes *laryngismus stridulus* "to tumefaction of the bronchial glands, which, compressing the *nervus vagus*, determine a mortal spasm of the glottis." But how slight soever the evidence in favour of his own view, he is not the less disposed to consider the question as definitively decided in his favour, and all opinion at variance with his own is stigmatized as error, as in the following passage:

> "I have pointed out some slight convulsive moments at the height of the paroxysm, and at the moment when the suffocation is most alarming. Nevertheless many English physicians, among whom I shall cite principally Henry Rees, (On a Species of Convulsions in Infants, etc. in the Lancet, vol. i, p. 679, 1831,) J. Cheyne, (the Cyclop. of Pract. Med., art. Croup,) Clarke, (A peculiar Species of Convulsion in Infant Children,) have described at length convulsions which take place in some children, and have insisted much on their importance. The perusal of their cases and of their general descriptions, have convinced me that they have regarded as belonging to stridulous laryngitis the real convulsions of eclampsia, whether a stridulous laryngitis had complicated this last disease, or that the spasm of the glottis had been in these cases but a simple symptom." (Vol. i, p. 285.)

Again he remarks:

> "The two most accurate observers, MM. Bretonneau and Guersent, have never seen stridulous laryngitis cause death. In the cases collected by English physicians, on the contrary, we see a certain number of examples of fatal termination. Whence arises this difference? always from the same cause. Many of these last authors have not been sufficiently strict in their diagnosis; whenever they have seen a paroxysm of suffocation arise rapidly in the course of apparent good health, they have admitted the existence of this disease; and it is this confounding of many diseases of very different severity which has introduced error into the results which they have obtained." (Vol. i, p. 289.)

He admits, however, on the authority of Home, Millar, Wichman, Vieusseux, and Lobstein, fatal cases, which can be referred only to stridulous laryngitis, and which M. Guersent himself regards as apparently belonging to this affection.

The best conclusion that we can come to on this subject is, that the disease described by M. Valleix and French writers generally under the name of stridulous laryngitis is a totally distinct disease from the spasmodic affection of the glottis of children known in this country and Germany by various designations, but yet in both countries regarded as a spasmodic, not an inflammatory, disease; or if inflammatory, it is considered that the inflammation does not exist in the larynx. Our main reason for coming to this conclusion is the fact that men of such experience as Bretonneau and Guersent should never have seen a case of the French disease fatal, whilst our English malady presents to every man of experience too many examples of fatality. In both diseases there is spasm of the glottis, but in the affection described by Valleix, the spasm appears to have its source in the part affected; whilst the irritation exciting the spasm in the disease of English and German writers is situated remotely from the larynx. Writers, however, are by no means agreed as to the situation of this remote irritation, and hence the multitudinous names by which the disease has been designated, such as "asthma dentientium," "asthma thymicum," &c. Indeed, we are by no means convinced that the irritation exciting the spasm is uniformly in the same locality; the variety may be in the things observed, not in the observers.

Dr. Dunglison's account of the disease, which he calls spasm of the glottis, is at once a faithful and succinct transcript of the opinions of English writers on the subject, and as such we recommend it to the student for perusal; but as it presents nothing new, we leave it without other comment.

Croup. The subject of croup, called by Dunglison inflammation of the larynx and trachea, and by M. Valleix pseudo-membranous laryngitis, is discussed by both writers at much length; but not with perfect accordance of sentiment. It will be observed that there is a difference in the names, that of Dr. Dunglison being comprehensive, and including all the parts especially involved in the disease, whilst that of M. Valleix is, in this respect, imperfect, but superior in another, since it depicts a characteristic of every complete case of the disease. We think, however, croup a better name than either, since it involves no disputable point either of site, nature, or tendency, and is, moreover, everywhere intelligible.

The most material difference, however, between these writers, regards the relations subsisting between croup and the diphtherite of M. Bretonneau. On this point they shall speak for themselves:

> "We have seen above," says M. Valleix, "that, according to the most precise observations, it has been impossible to establish a difference of nature between pseudo membranous pharyngeal angina and the laryngitis now under consideration. Everything, in fact, is common to them, except the seat of disease, and besides they proceed very frequently the one from the other. This is a truth now recognized, and against which the objections of certain authors, among which I shall mention those of Dr. Cheyne, (Cyclop. of Pract. Med., vol. i, p. 99,) cannot prevail. This physician does not conceive that 'we can regard these two affections as identical, because,' &c. I have cited this passage only to show how feeble are the objections urged against one of the most important results of modern observation; for, although M. Bretonneau himself has quoted a considerable number of authors who, before F. Home, had a glimpse of this truth, it must be acknowledged that it is to his able researches that we owe the complete demonstration both of the proper characteristics of diphtheritic pharyngeal angina, and of its identity with pseudo-membranous laryngeal angina. Nevertheless, as if he feared to go too far, M. Bretonneau asks (Loc. cit. p. 281) whether concretions of a non-diphtheritic nature may not be developed in the larynx. But on examining the facts attentively, we fail not to see that all the pseudo-membranes developed in the larynx are of the same nature, and that there does not exist between them any distinctive sign of material value. These two diseases differ, then, I repeat it, only in situation. But as on the one hand the difference of situation is capital, and as, on the other, diphtheritis may be limited to the larynx, the physician, whilst acknowledging that pharyngeal angina, and pseudo-membranous laryngitis are of the same nature, and are frequently confounded together, ought to make two distinct diseases of them, and study them with care separately." (Valleix, pp. 342-3.)

Dr. Dunglison takes a view very different from this. He, too, shall speak for himself. After stating the opinions of Bretonneau, and a declaration of M. H. Bell's, that the ideas of this well-known pathologist "are, at the present day, generally admitted," he says:

> "It is not accurate, however, to ascribe such sentiments to the generality of pathologists. Almost all admit, that the essential character of croup, in the child, is a violent inflammation, accompanied by an exudation of plastic lymph; but the generality of American and British practitioners regard this idiopathic inflammation of the larynx and trachea, or rather of the latter—for the larynx is generally but little implicated—as very different from the diphtheritic inflammation that occurs in diphtheritic stomatitis and pharyngitis, in which, as has been shown, there is always precedent fever, and the formation of false membranes in the mouth and pharynx, which may extend down the windpipe, and give rise to symptoms of croup in the advanced stage of another and totally different disease." (Dunglison, p. 244.)

He afterwards quotes with approbation, and, we think, with success, the diagnosis of Dr. Stokes between what this writer very properly terms primary and secondary croup. The main points of his diagnosis, (independent of the difference in the locality of the primary attack,) are, that in the primary affection the fever is symptomatic of the local disease, and this fever is inflammatory; in the secondary one, prior to the occurrence of the local affection, there is a disease existing accompanied with fever, which is typhoid; again, primary croup demands antiphlogistic treatment, which is often successful; the secondary one requires the tonic, revulsive, and stimulating modes of cure; the primary disease is sporadic, never contagious, the secondary constantly epidemic and contagious (?); the first is a disease of childhood, the second affects chiefly adults; in the one the pharynx is healthy and dysphagia absent or very slight, in the other the pharynx diseased and dysphagia common and severe; in the primary disease catarrhal symptoms are often precursory to the laryngeal ones, the secondary disease supervenes without such a preliminary; in the primary disease, complication with acute pneumonia is common, in the secondary rare; lastly, in primary croup, there is no characteristic odour of breath, in secondary, the breath is often characteristically fetid.

Various circumstances mentioned in the works of our authors, and our own observation, lead us to the conclusion that Drs. Dunglison and Stokes take the correct view, that primary and idiopathic croup is a different affection from croup engrafted on diphtheritis or any other disease. One of these circumstances is the rate of mortality ascribed by authors, Caillau and Double estimating it at two thirds of those attacked; another observer, J. Frank, cured thirty-nine cases and lost twenty-seven, whilst Jurine and Vieusseux ascribe to it a fatality of one in ten only. Dr. Dunglison remarks that no such mortality attends the disease wherever he has practised and seen it; on the contrary, where the practitioner is called sufficiently early, it is usually very manageable. The scenes of Dr. Dunglison's observation have been various and extensive, comprising the banks of the Frith of Forth, the margins of the English lakes of Cumberland, London, Paris, the interior of Virginia, Baltimore, and Philadelphia. M. Valleix says that a very great discrepancy exists among authors regarding the mortality from this disease, one estimating it at almost the *totality of the cases*, another at one half, another at a ninth only. We find it impossible to reconcile these discrepancies otherwise than by supposing that certain parties have confounded in their estimates the results of the mortality of two distinct diseases, the very fatal diphtheritic laryngitis and the comparatively tractable primary croup. We would add, too, that the greater prevalence of the diphtheritic (the more fatal) species in France, is demonstrated by the very great mortality from the disease in *that*, compared with other countries. We shall venture to inquire, too, whether the very great familiarity of M. Bretonneau with diphtheritis may not arise from the nature of the locality where this gentleman practises, Tours, situated at the conflux of the Loire and Cher—whether this disease may not arise from malaria generated on the low and damp, if not marshy, banks of those rivers?

When we examine the practical precepts of our respective writers, again we see reason to conclude that they are considering different diseases. Dr. Dunglison recommends that trial should be made, at the commencement of the disease, of an emetic and a warm bath. He then says "should the symptoms not yield to the emetic and warm bath, blood must be drawn either from the arm or jugular vein, or by means of leeches;" his plan subsequently consists in the administration of large doses of the antimonial tartrate of potash; in fact, he recommends, throughout the disease, what every practitioner in this country would do in a case of croup. But what on this head says M. Valleix, or rather M. Bretonneau? "Then comes M. Bretonneau, who at first had a certain degree of confidence in bleeding, regarding it as suitable to be opposed to the progress of diphtheritis, but who was forced to abandon this view, when he saw that in no case submitted to his observation was the formation of false membrane presented by bleeding. It is in *epidemics* (who ever heard of epidemics of croup in this country?) especially that the insufficiency of bleeding has been evident to him." M. Valleix coincides in these therapeutic views of M. Bretonneau. The latter writer subsequently quotes a case in which a very abundant local bleeding was followed by a prompt extension of diphtheritis.

We consider that there is ample evidence of the non-identity of diphtheritis and croup.

We now take leave, but not *final* leave, of our authors, for we purpose at a future day to resume our critical labours on some other portions of their valuable works.

Since the foregoing observations were written, we have received a second edition of Dr. Dunglison's work,—a sufficient indication of the high character it has already attained in America,—and justly attained.

ART. II.

Diseases of the Lungs from Mechanical Causes, and Inquiries into the condition of the Artisans exposed to the Inhalation of Dust. By G. CALVERT HOLLAND, M.D., Physician Extraordinary to the Sheffield General Infirmary, &c.—*London*, 1843. 8vo, pp. 100.

THE small volume bearing the above title is the fruit of—as its author announces—the labour of years; it is now presented to the public in the hope of attracting attention to the evils it describes as assailing a large proportion of the population of the town of Sheffield; and these evils may, according to the writer, be simply, readily, and effectually corrected. Here are so many reasons giving the work strong claims to attentive consideration.

The far greater number of artisans in Sheffield are grinders of cutlery and hardware; the author estimates their number at about 3000. These individuals are peculiarly subject to thoracic disease of a distinct character, and Dr. Holland has been enabled, from his position as physician to the General Infirmary of the town, to investigate its history upon an extensive scale.

Cutlery and edge-tools are all ground either upon a dry or a wet stone, or (in some instances,) upon both, the dry first, the wet afterwards,—a few articles only, such as saws and scythes, on the wet stone alone. Now, as might be anticipated, the evils of the occupation are more essentially attached to the process of dry grinding; and it is remarkable that the ill effects of its pursuit are, comparatively speaking, of recent origin and traceable to the increase of luxury in the edifices where the workmen are employed. It appears that previous to the use of steam, the grinding-wheels were invariably situated on the rivers at a distance of from two to five miles from the town, in the midst of picturesque scenery; the grinder had thus daily exercise, and abundance of pure air circulating around him; he had frequent holidays too, in consequence of the supply of water being too great or too small. Instead of all this, steam has been introduced; the "modern *grinding-wheels* are built in the town, and are several stories in height," (a local mode of expression no doubt well understood in Sheffield, but not particularly intelligible for persons living out of the grinding districts,) and no regard whatever has been bestowed upon their ventilation. Each room is occupied by eight or ten individuals belonging to different branches of the occupation, and as wheels are now erected on valuable land at a cost of from ten to twenty thousand pounds, space must be economised as far as possible, no matter how efficiently the grinder's health may be ruined by the economy. No such things as broken windows, dilapidated doors, or shattered roofs appear in these solid and well-finished structures, and hence the clouds of dust which used to escape through the slovenly, but healthful, safety-valves of former days, now accumulate in abundance round the workman's head. Such is the density of this deteriorated atmosphere that "a stranger entering the rooms at certain times, would find it difficult to breath in them."

The writer considers it extremely difficult to trace the first morbid effects of inhalation of an atmosphere surcharged with gritty and metallic particles; and the description given affords evidence of the difficulty he has felt. "The *earliest inconveniences* experienced are occasional irritation in the larynx and trachea, exciting cough and slight *expectoration, which is saliva coloured with the inhaled dust, and is sometimes quite black;* a dryness of the throat, also, generally exists." In the next paragraph it is stated that "the *first effect* of the dust is to cause an increased action in the mucous membrane of the trachea and bronchial tubes; in consequence of which, *expectoration* is excited, *presenting nothing peculiar in its character.*" We are unable to reconcile these statements. "In some cases," it is added, "when the constitution is defective in tone and vigour, showing a scrofulous or leucophlegmatic tendency, ... purulent-like matter is observed mixed with the inhaled dust." The expectoration is said to be at first "simple saliva," yet its origin is referred to "increased action in the mucous membrane of the trachea and bronchial tubes;" a pair of statements leading to the rather curious inference that the windpipe and its ramifications are the secreting organs of the saliva. Dr. Holland considers that the fine particles of dust do not stop short in the bronchial tubes, but pass along them to the substance of the lungs, and then give rise to structural alterations,—an opinion which is very old, but for which new reasons are adduced in the writer's pages.

Dr. Holland objects to the term grinders' asthma, by which the peculiar pulmonary disease of this class of workmen is commonly known, because "in an immense number of cases which come under observation, we do not perceive the phenomena of asthma, but of *tuberculous phthisis.*" From this the reader is forced to infer that Dr. Holland regards the affection produced by the inhalation of metallic dust as tuberculous disease of the lungs.

This declaration is followed by a chapter on the "modifications in the character of phthisis, from a difference of the circumstances in which it occurs." Two or three propositions here attract our attention, less from any importance they possess in themselves, than from the very clear indications they give of the school of reasoners to which Dr. Holland belongs. "The *physiologist* would *expect* that a structural alteration in the condition of the lungs, arising from an external irritating cause, would be evolved in association with constitutional energies, very unlike what exist, when such alteration is the result of unknown vital actions." From this sentence we should judge Dr. Holland to be a person who ruminates physiology in his arm-chair, arrives at certain conclusions regarding the essence of disease, no doubt of very satisfactory character, and armed with these goes forth with a thorough-paced determination to discover at the bedside the experimental evidence of his preconceived doctrines. You may judge of a man's true character more correctly from trifles, than from the great actions of his life,—at least so hath spoken a moralist whose keenness of observation experience has taught us to deeply value. So, too, of the tone of a man's scientific mind; chance phrases such as that we have pointed out will do more to lay it bare before us, than pages of elaborate announcement of doctrine and opinion. Now here we recognize in Dr. Holland a disciple of the *à priori* school of reasoners,—that most baneful class, which has done more for the promulgation of unsound doctrine, than it is possible to estimate. Further on Dr. Holland, speaking of "tuberculous phthisis," "contends that the *pulmonary affection springs out of a condition of the animal system prone to deterioration and decay;* and that this is in our opinion more frequently a cause than an effect of the local malady, whether this be seated in the lungs, the kidneys, or any important gland or series of glands." We really and truly are unable to comprehend this; how the local malady in tuberculous phthisis can be spoken of as seated in *the kidneys*, or *some or other gland*, is beyond our conception. Either Dr. Holland's notions on the subject of phthisis are curiously behind those of the day; or here is a looseness of expression symptomatic of anything but precision of knowledge on the part of its author.

There are four chief grounds of distinction pointed out between constitutional phthisis and the "phthisis" arising from grinding cutlery.

The first is, that the latter is produced by grinding cutlery, and the former by constitutional predisposition. The next point of distinction, we are told, is one of duration. "The *average* duration of constitutional phthisis is stated," says the author, "to be almost nine months." This is an erroneous version of the fact: from the fourth to the ninth month has been found the *most common* period of death; but the *average* duration of the disease about two years. It is rather vaguely said by the author that the grinders' disease, on the contrary, will, on an average, last for years. Thirdly, the grinder, unless a consumptive diathesis exist, is, with the exception of the pulmonary affection, in good health. Fourthly, in the advanced stage of the grinders' disease there is less attenuation than in ordinary phthisis,—the breathing habitually *more* difficult, and the expectoration perhaps more copious. The pupil of the eye is seldom much dilated, nor are the teeth white. The pulse is frequently as low as eighty or eighty-five pulsations in the minute. The countenance, in this stage, exhibits great anxiety."

Chapter IV introduces us to a detailed account of "the symptoms of the disease." The "first traces of morbid action" which strike the observer are, irritation of the larynx, trachea, or bronchi, accompanied with slight and occasional cough. In persons of average vigour of constitution these symptoms excite no attention, and may continue for years without manifest aggravation; nor do they affect the general health, if they supervene in early life. If, on the contrary, they are first observed between twenty-five and thirty years of age, "the constitution is too debilitated to arrest the progress of the symptoms; consequently, these rapidly give rise to others more urgent in character, and thus one series of morbid action is quickly succeeded by another, *until at length the system breaks up,* presenting unequivocal evidence of *pulmonary phthisis.*"

In the early stage there is commonly no acceleration of pulse or fever, and the tongue rarely presents any unnatural appearance. In the next stage the cough and dyspnea increase considerably, the body is bent forwards, and "gradually sinks into less space." The appetite is good, and the digestive functions tolerably regular; the patient retains his flesh, generally speaking, to a very remarkable amount. Some quickening of pulse (*e. g.* to 75 or 80,) occurs now, but there is no hectic fever or increased heat of skin; the expectoration, chiefly mucous, may be somewhat purulent.

The chest sounds well on percussion; the respiration is natural in some few parts, puerile in others, bronchial in more.

In the third and last stage "the wretched artisan is an object painful to contemplate." The bent position of the body, and the exceedingly round shoulders constitute "one well-marked and prevailing *symptom;*" the respiration is extremely hurried, and more laborious than in ordinary phthisis; the expectoration often very copious; the mouth is not, as in the affection just named, prone to aphthous eruption, nor is the tongue smooth and polished.

The writer regards the cases in the last or advanced stage of the disease as divisible into two important classes. In the one, *cough* is the early and predominant symptom; here there is least emaciation, less tendency to diarrhea or hectic, greater oppression of breathing, and more copious expectoration; the chest is rounded anteriorly, and the sound, under percussion, particularly sonorous. The suffering and anxiety of countenance are extreme; diarrhea is seldom great.

In the other class, in which the cough is scarcely noticed by the patient, previously to laborious breathing coming on, the body wastes rapidly, the expectoration is less copious, the chest is somewhat contracted and flat, and sounds dull under percussion. "In both classes tubercles are found in the lungs, though by no means invariably in cases in which the cough was the principal distressing symptom for years."

Dr. Holland says hemoptysis occurs frequently in both classes; but is unable to state in which to the greater extent. It is said to be by no means uncommon for the grinder to bring up black, hard masses, "in appearance accretions of dust," varying in size from that of a pea to a small marble. The author knows grinders who have expectorated such masses for years, and take occasional walks (to which experience has taught them to trust,) for the purpose of facilitating their expulsion.

It is stated that the grinders are particularly subject to acute inflammatory affections—pneumonia, pleurisy, and rheumatism. The writer says that the grinder is frequently troubled with gravel; but says nothing respecting its characters.

The dulness of the sound of the chest under percussion is accompanied either with bronchial respiration, or "with a murmur as if caused by respiration performed at a distance from the surface of the lungs." The bronchial respiration the author believes to be shown, by dissection, to be the effect of enlargement of the bronchi; the second condition of respiration mentioned he "has imagined to depend on an emphysematous condition of the lungs."

The mucous membrane of the larynx, when the symptoms have existed for any length of time, becomes, in almost all cases, thickened, and is often excessively pale. The same changes take place in the mucous membrane of the bronchi and their ramifications, but to a greater extent. The membrane is often much more thickened, but usually of the same pale aspect. Dilatation of the bronchial tubes and expansion of the air-cells follow. These changes do not "prevent the subsequent production of tubercles," though these bodies "are not invariably found in the lungs." In the class of cases in which the substance of the lungs suffers first, the affection runs its course more rapidly, as has already been signified.

The differences in the morbid appearances discovered after death in the "phthisis of grinders and that occurring under ordinary circumstances" have appeared to Dr. Holland to be the following: 1. Adhesions between the lungs and pleura costalis are generally observed in the grinders' affection. We were not previously aware that such adhesions were of rare occurrence in phthisis, as Dr. Holland here leads us to suppose he considers them. 2. The next appearance which is frequently observed is great enlargement of the bronchial glands, "or, more correctly speaking, the conversion of them into a black, hard, gritty substance, varying in size from half a marble to a large hazel-nut." Dr. Holland is "not enabled to state their composition:" the idea suggests itself that he ought to have prepared himself to do so. 3. Similar substances, and "to appearance," analogous in composition, were, in several cases, detected in the substance of the lungs. And the writer states that he found them in portions of these organs which exhibited "every degree of disorganization, from the first questionable change of structure to the formation of softened tuberculous masses." 4. The lungs are frequently engorged with a black or dark fluid.

This elucidation of the "pathology" of the disease closes with a narrative of some few particulars of a case in which recovery took place. The writer comments on the fact as follows: "The point of particular interest in this case, and which, indeed, is instructive, is the *large cavity*, which was *unquestionably the cause of the unexpected recovery* of the individual thirteen years before." The notion that a "large cavity" can, *per se*, be the cause of recovery, is, to us, a very singular novelty. Many of the curious statements in the book, however, derive their originality, it is extremely probable, at least, from the looseness of the writer's style,—a looseness so remarkable that the matter is sometimes rendered quite unintelligible. Such inelegances as the following are by no means few and far between: "In the right lung an immense cavity was discovered, occupying nearly one third of *it*. An idea of *its* capacity may be formed from the fact, that the gentleman whose case *it* was, after laying *it* partly open, introduced his fist into *it*." (p. 43.) Dr. Holland is of opinion that the general dilatation of the air-tubes, which existed in this case, "perhaps contributed towards recovery, by maintaining the lungs in a condition favorable to the permeation of air." This is unquestionably a remarkable piece of therapeutical doctrine.

In considering the treatment of the affection the author maintains the division already made of the cases into two classes. In the better class, wherein the disease has not very greatly diminished the constitutional vigour of the subject, &c., the repeated application of a very few leeches, followed by blisters, with the internal use of emetics, expectorants, alteratives and tonics, is recommended by the author. In the other class of cases, "experience has not suggested any treatment which has been fraught with any large amount of benefit, beyond what is familiar to every practitioner."

Some twenty years ago the state of the grinders attracted very considerable attention, and numerous efforts were made to devise some plan by which the insalubriousness of their occupation might be lessened. It was at this time Mr. J. H. Abraham invented a magnetic mouth-piece, which was destined to attract the metallic particles evolved in the process of grinding; it of course could exercise no influence on those of the gritty kind. The invention was never much employed. Dr. Holland has laboured to invent a better, and the "trial of years" has proved the following contrivance to be "equal to the thorough correction of the evil." Here is the description of the plan:

> "A wooden funnel, from ten to twelve inches square, is placed a little above the surface of the revolving stone, on the side furthest from the grinder, and this funnel terminates in a channel immediately under the surface of the floor; *or we may consider the channel simply as the continuation of the funnel*, in order to avoid any confusion in the explanation. The channel varies in length, according to the situation of the grinder, in reference to the point where it is most convenient to get quit of the dust. If we suppose that eight or ten grinders work in the same

room, each has his own funnel and channel, *and they all terminate in one common channel, the capacity of which is perhaps twice or three times as great as each of the subordinate or branch channels.* The point where they terminate is always close to an external wall. At this point within the general channel, the fan is placed, somewhat in form like that used in winnowing corn, and to this is attached a strap which passes upwards, and over a pulley, so that whatever puts the pulley in motion, causes the fan also to revolve. The pulley is placed in connexion with the machinery which turns the stone, so that whenever the grinder adjusts his machinery to work, he necessarily sets the pulley and the fan in motion. The fan, acting at this point, whatever may be the length of any of the subordinate channels, causes a strong current to flow from the mouth of each funnel, which carries along with it all the gritty and metallic particles evolved, leaving the room in which the operations are pursued, free from any perceptible dust. When the whole apparatus is perfect and in excellent condition, the atmosphere is almost as healthy as that of a drawing-room."

Chapter VII, incomparably the most valuable and best of the volume, contains a full statistical account of the physical and mental condition of the artisans at Sheffield. The state of the grinders differs very considerably, according to the branch of the occupation they pursue, and for this reason the scissor-grinders, fork-grinders, needle-grinders, razor-grinders, penknife-grinders, tableknife-grinders, saw-grinders, file-grinders, and scythe-grinders, supply each of them the materials of a distinct section. We shall select some of the facts given, concerning the fork-grinders as a specimen of the whole.

Fork-grinding appears to be a peculiarly distinctive branch of the occupation. So much so that grinders belonging to other departments, frequently refuse to work in the room where it is carried on, and many sick-clubs have an especial rule against the admission of persons following it. The statistics of the branch are as follow:

Men employed			97
Boys „			100
Men from ætatis	21 to 25	28	
„	26 30	28	
„	31 35	8	
„	35 39	14	
„	40 and upwards	19	
		—	97
Boys from ætatis	10 to 14	39	
„	15 19	51	
	at 20	10	
		—	100

Of the 19 men aged above 40, there were only 3 who had reached their fiftieth year; and 10 of the 19 either commenced at this trade late in life, or passed several years in the army. Deducting these ten from the 97, there are 56 under 30 years of age; 8 from 30 to 35; 14 from 35 to 40; 9 above 40 and under 50. And in the class from 35 to 40, there are some who have not worked regularly at the trade from youth. These facts lead to the direct inference that a very considerable proportion of the whole number must die under 30 years of age. There are 56 under 30, and only 8 from 30 to 35 years of age; consequently, observes the author, the greater part of the 56 die before attaining the thirtieth year.

Between 1820 and 1825, one fourth of the total number following this branch of the occupation died. And here is a table exhibiting in a most obvious manner the enormous amount of mortality caused by this pursuit. It is entitled:

> "*Comparison of deaths*, at particular ages, out of 1000 deaths of persons above 20 years of age, in England and Wales; in Sheffield generally, and of the fork-grinders particularly.

Ages.	England and Wales.	Sheffield.	Fork grinders.
20 to 30	160	184	475
30 39	136	164	410
40 49	126	158	115
50 59	127	155	0
20 60	549	661	1000
All ages above 20	1000	1000	1090

> "Thus," observes the author, "out of 1000 deaths of persons above 20 years of age, the proportion between 20 and 30, in England and Wales, is 160; in Sheffield 184; but amongst the fork-grinders the proportion is the appalling number 475; so that between these two periods, 3 in this trade died to 1 in the kingdom generally. Between the ages of 30 and 40, a still greater disparity presents itself. In the kingdom 136 only in the 1000 die; in Sheffield 164; but in the fork-grinding branch 410; so that between 20 and 40 years of age in this trade 885 perish out of the 1000; while in the kingdom at large, only 296. Another step in the analysis, and we perceive that between 40 and 50, in the kingdom generally 126 die; in this town 158; and in this branch 115, which completes the 1000. *They are all killed off.*"

These facts require no comment.

These unfortunate people are shown by further statements to be in a very low state of moral and intellectual condition; and to labour under a much more than average amount of disease.

This brief outline will suffice to convey some notion of the character of Dr. Holland's inquiries, and we trust tempt many to an examination of them in their complete form. Were we to form a judgment of the author from this book, we should be disposed to consider him possessed of more enthusiasm of temperament than calmness of judgment, and unquestionably more likely to reap distinction from his humanity than his pathology. But he has the best of it in the latter particular; it is impossible to close his book without conceiving a sort of personal respect for himself, and a strong desire that he may meet with becoming gratitude from the class whose condition he has so conscientiously (and successfully if there be wisdom in governments, for he has made out a clear case for the necessity of legislative interference,) laboured to ameliorate.

Art. III.

Praxeos Medicæ Universæ Præcepta. Auctore Josepho Frank, Joannis Petri Filio, &c.

Precepts of Universal Medical Practice. By Joseph Frank, Son of John Peter Frank.

Partis Tertiæ Volumen Secundum, Sectio Prima, continens Doctrinam de Morbis Tubi Intestinalis. Quam exposuit Fredericus Augustus Benjamin Puchelt, Magno Duci Badarum à Consiliis Aulæ intimis, &c.—*Leipsiæ*, 1841.

The Second Volume of the Third Part, the First Section, containing the Treatise on Diseases of the Intestinal Canal. By Frederic Augustus Benjamin Puchelt, Privy Counsellor to the Grand Duke of Baden, &c.—*Leipsic*, 1841, pp. 786.

This work is not what the first part of its title declares it to be, the production of the celebrated Joseph Frank; but that of Professor Puchelt of Heidelberg. The latter in a brief preface informs us that Frank in 1827, proposed to him to undertake that portion of the great work on practical medicine which should comprise the diseases of the intestinal canal; and the volume before us, containing nearly 800 pages, is the result of his compliance with the proposal; advice and certain notes communicated by Frank being the amount of the learned editor's obligations in that quarter. It will be our place to examine how far he has shown himself worthy of the delegated trust.

The following is his arrangement of the very extensive subject intrusted to him: Congenital malformation of the intestines, and their abnormal form and situation; enteritis, suppuration ulceration, tenuity (marcor,) and perforation of the bowels; induration of their membranes, and tubercles, melanosis, medullary fungus, scirrhus and cancer in their tract; morbid adhesions, polypi, œdema, hydatids, and intestinal stricture and contraction; worms; flatulent affections; colic; constipation; ileus; diarrhea; dysentery; hemorrhages; and cholera. Each section of the work, and several of the subsections are prefaced by a literature of the matter in hand as copious as possible, ranging from the remotest antiquity to the last publication of Louis, or a recent number of our own Review. The author's addiction to literature, which is certainly intense, does not, however, divest him of sound practical judgment. From the days of Sydenham, a notion, founded apparently on a jocular remark of that illustrious man to Sir Richard Blackmore, has pervaded a portion of our profession that these qualities are incompatible. But the following passage shows that indulgence in research has not impaired the learned Professor's just and practical appreciation of his subject:

> "It has occasionally happened that intestinal diseases have been considered matters of little moment, by the Brunonians for instance; but it more frequently occurs that physicians seek in the abdomen a disease which exists elsewhere, and that the intestines are considered the cause and seat of almost all maladies; now they accuse crudities, now repletion, and now gastro-enteritis. Be such exaggeration far from us! Let us not attach too much importance to individual symptoms; to those derived from the tongue for example, the redness of which has been accounted a certain sign of gastro-enteritis, and which when coated has been

thought to indicate crudities. Let us not confound together those sympathetic, secondary and symptomatic affections which occur in almost every disease, with those which are idiopathic, primary and essential. On the other hand let us beware lest intestinal diseases should be overlooked, and estimated at less than their real value! Therefore it is the duty of the physician, free from preconceived opinion, and the empire of hypothesis, to consider with an unbiassed mind all the symptoms, their succession and composition, not neglecting due examination of the abdomen, as well externally by compression and percussion, as internally by the introduction of the finger, a probe, or speculum within the anus; and to weigh the causes of the disease, the epidemic and individual constitution, the family, age, habits, temperament, and preceding diseases of the patient, and the medicines already administered to him." (pp. 15, 16.)

ENTERITIS. The subject of enteritis receives ample treatment at the Professor's hands. Nowhere have we found it more learnedly discussed, or with more practical good sense. His divisions of the disease are numerous, for besides the great distinctions into serous and mucous enteritis we have the inflammation considered with regard to the portion of intestine implicated. Where it occupies the greater part of the tract of the intestines, it constitutes the *E. diffusa*, which may at its commencement be either the serous or mucous variety; in its progress it may affect all the tunics. This form of enteritis is scarcely ever due to a local cause, excepting this be poison; but arises, for the most part, from an universal diathesis and metastasis. *E. circumscripta* may arise in any portion of intestine from a local mechanical cause, such as hernia or intus-susception. Inflammation of the duodenum forms a division, and inflammation of the small intestines generally, equivalent to the *enteritis iliaca* of Sauvages, the *febris iliaca inflammatoria* of Hoffman, and the *jejuno-iliotis*, or *ileo-jejunitis* of Broussais, constitutes another. We have the inflammation of the *cecum*, designated by our author *typhlitis*; that of the vermiform appendix, and subsequently that peculiar affection adverted to by P. Frank, but first described as a distinct disease by Dupuytren, in which the cellular membrane situated between the *cecum*, and the *iliacus internus* muscle is the seat of inflammation, which in its progress is wont to extend to the intestine, and terminate in suppuration. This Puchelt calls *peri-typhlitis*. The affections of the other large intestines are described under the names of *colitis*, and *architis* (inflammation of the *rectum*), and that inflammation of the cellular membrane surrounding the gut whence fistula arises, is described, but not, as it perhaps should have been consistently with his own nomenclature, under the designation of *peri-architis*. Of his *enteritis serosa*, he remarks that it has not its sole seat in the serous covering of the intestines, but that the abdominal peritoneum is involved, and that the disease is properly referred to peritonitis. There is some confusion of idea, and consequently of nomenclature pervading the profession on this head. When the disease occupies chiefly, as it does in puerperal females, the abdominal peritoneum, the bowels being amenable to medicine, we should call it peritonitis. When, however, the intestinal peritoneum is at first the seat of the inflammation, the muscular tunic, as it generally does, partaking of the affection, the bowels being obstructed, we should apply to it one of the names suggested by Abercrombie, viz., *E. peritoneo-muscularis*, or *seroso-muscularis*. We are aware that we have high continental authority, (that of Louis for instance, who considers the only real enteritis to be that associated with diarrhea,) against us. But we regard the ancient nomenclature, modified in the one form of enteritis by the affix we have selected from Abercrombie, or for brevity's sake, by the word *serosa*, and in that of the other by the term *mucosa*, at once more in accordance with the characters of the respective diseases, and with anatomy, (for we have been taught to consider their peritoneal coat as a portion of the intestines,) than the modern innovation.

After the discussion on E. serosa, we have E. mucosa, considered under two heads, viz., as it affects grown persons, and infants, the latter constituting the *E. mucosa infantum* of Abercrombie. *E. totalis* is then described, not as an affection, as the name might seem to imply, of the whole tract of the intestines, but of the entire tunics of any given portions. We then have the general division of the various forms of *enteritis* into acute and chronic, and next follow in order the following species: E. traumatica, phlegmonosa, erysipelacea, and exanthematica, rheumatica, arthritica, hemorrhoidalis, catarrhalis, scrofulosa, carcinomatica, nervosa, and putrida, which last, the author says, is conjoined with petechiæ and other putrid symptoms, and is prone to terminate in gangrene.

We are thus presented with twenty-eight varieties or species (as we may choose to view them) of this one disease. Here is certainly division enough and to spare; but in truth, in judging of German medical or other literature, allowance must be made for the peculiarities of the German mind, its tendency at one time to discern or almost create distinction where nature has established little or none; and at another to discover relations imperceptible to other eyes, and to associate into one vast system the "discordia semina rerum." These peculiarities, however, merit forgiveness on the score of the singular industry, learning, profundity and other high qualities of this truly gifted people.

As specimens of the author's manner we select his descriptions of two forms or varieties of enteritis, viz., typhlitis and perityphlitis:

"Aretæus, Celsus, Bonetus and Fabricius Hildanus have observed the *intestinum cæcum* to be primarily affected with the inflammation which we have called *typhlitis*. Van Swieten, and J. P. Frank, have likewise noticed this disease; but Unger, Copland, Posthuma, Holscher, Albers, and Burne have described it more accurately. Foreign bodies, such as plum- or cherry-stones, hardened feces, or worms, excite this inflammation. Morgagni has seen the cecum after a fall, distended with excrements and inflamed, and Speir has observed it ruptured and inflamed in consequence of external violence; and there can be little doubt, that chilling of the body occasionally gives rise to the disease. Finally, inflammation of the cellular coat situated behind the *cecum*, and of the vermiform appendix may, either of them, extend to the *cecum*. Some obstruction of the bowels having preceded, a pain more or less severe arises in the right iliac region, which is increased by pressure and motion of the body, so that the patient desires to keep it in a state of repose; sympathetic pains extend, on any movement of the body, from the sacral region to the right thigh, and the testicle is drawn up to the abdominal ring. There is fever which in stercoral typhlitis is moderate, but in other forms of the disease is very severe; vomiting sometimes supervenes, so too do symptoms of ileus. The pains are less severe, when the mucous membrane alone is inflamed, nor are the bowels obstructed, but on the other hand a mucous and bloody diarrhea prevails, with gripes and colic pains preceding each evacuation. Typhlitis passes not unfrequently to the cellular tunic behind the cecum, sometimes to the peritoneum, or to other portions of the intestinal tube; likewise it may continue in a chronic form, and leave behind it induration of the intestine." (pp. 147-50.)

Passing over his description of inflammation of the vermiform appendix, we proceed to transcribe that of perityphlitis, omitting only, for brevity's sake, Dr. Puchelt's abundant marginal references to the publications, (be they original works or periodicals,) and the pages of the publications, in which the observations of the authorities he quotes have been published.

"Very frequently the cellular membrane, situated between the blind intestine and the iliac muscle is seized with inflammation, which often passes to the intestine itself, and terminates in suppuration. This disease we regard as peculiar, and have called it perityphlitis. P. Frank alluded to it, but Dupuytren first regarded it as a distinct disease. After his day, Husson and Dance, Meniere, Abercrombie, Unger, Pezerat, Téalier, Goldbeck, Corbin, Ferrall, Puchelt, Copland, Truchsess, Smith, Burne, Posthuma, Wilhelmi, Albers, Piotag, Lebotard, Bricheteau, Grisolle and others, have frequently observed this disease, and fully described it. It arises most frequently from cold; and besides may be excited by a damp dwelling house, sordes of the first passages, worms, spirituous liquors, preparations of copper and lead, disturbance of the uterine system, child-bearing, emetics and purgatives, riding on horseback, and affections of the mind. After a feeling of indisposition of a few days' duration, and loss of appetite, white tongue, eructations, nausea, and obstructed bowels or diarrhea, there arises excruciating, pungent and wandering pain of the abdomen, felt in the epigastric region and other points; the abdomen being painful on pressure but soft, the bowels costive. Before four and twenty hours have elapsed, the pain passes to the right iliac region, and becomes fixed there. Sometimes it extends to the urinary bladder, in which case, the urine, ordinarily pale, becomes thick, of a reddish brown colour, sparing in quantity, and heat is felt in discharging it. By pressure not only is the circumscribed pain in the iliac region increased, but a tumour is perceptible, elastic at first, afterwards possessing a considerable degree of hardness. Motion of the body, and especially that of the right lower limb, increase and aggravate the pain so much, that patients generally lie on the back or right side, without changing their position, with the feet drawn up. There is fever, a dry and hot skin, paleness of the face, bowels rarely in a liquid state, generally costive; sometimes vomiting is present, but rarely *ileus*;* nevertheless, expulsion of flatus takes place, some increase of the cecal pain preceding such expulsion. Patients remain in this state for some days, till the disease terminates either by treatment, by crisis or suppuration. When crisis takes place by urine and perspiration, there remains frequently a certain degree of sensibility in the affected part, an intermitting pain, a sense of fulness, and likewise a degree of positive intumescence for some time. When an abscess takes place in the cellular substance behind the *cecum*, the pus may be effused into the *cecum* itself, healthy purulent matter being excreted *per anum*, and the patients being speedily restored to health. The matter sometimes but very rarely finds issue through the urinary bladder, uterus or vagina. There is sometimes fluctuation in the anterior surface of the iliac region, or in the lumbar region behind the crest of the *os ilium*, whence arise ulcers and fistulæ, producing hectic, communicating at once externally, and in the cecum, especially fatal, often so by means of stercoraceous effusion into the peritoneum, and peritonitis. In such cases the inflammation has extended from the cellular membrane to the cecum, vermiform appendix and peritoneum, and these parts have been found inflamed and disorganized. When this occurs, it may be a matter of question where the inflammation has been primarily seated. Wilhelm is of opinion that the inflamed cellular tissue may become gangrenous, though no case is given which proves it. An observation, however, of his own seems to prove it.* I was present, and saw the dissection; the cellular tissue was rather destroyed by sphacelus than distended with pus. Certainly no abscess had been formed. Finally, we must remark that cachectic subjects attacked with this disease scarcely ever escape suppuration; but in florid and robust subjects by proper treatment it may be easily and safely prevented. In the former class the disease passes into the chronic state, in the latter its course is sometimes very brief." (pp. 152-7.)

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* The author uses this term in the sense of stercoraceous vomiting, or (to take his own definition) it is "a disease in which feces are scarcely or not at all expelled *per anum*, but are vomited, pain and anxiety being accompanying symptoms." We think that our very learned author would have done better had he given the precise symptoms of every disease he treats of, rather than comprehended any portion of them under a term which we consider as considerably vague. He treats of ileus or iliac passion as a distinct disease. It is not this; but an assemblage of symptoms, varying in different cases, indicative of more than one disease of the intestinal canal; violent spasm, permanent stricture, intus-susception, enteritis, &c.—Rev.

Two cases are referred to by M. Puchelt, in which perfect recovery took place from the very deplorable circumstance of stercoral fistula from perityphlitis. The one is quoted from Vogel, (Handb. iv bd. p. 321,) the other from Jabgerschmidt, (Ephem. naturæ curios. dec. iii, b. ii, obs. 156, p. 245.)

When he comes to the subject of treatment, we find our author's practical precepts judicious. He advises that the force of the disease should be broken by antiphlogistic measures proportioned to its severity. He would not allow the small, weak, and contracted pulse so common in this disease to be an obstacle to bloodletting, or to its repetition in diffuse, serous, total, acute, traumatic, phlegmonous, or hemorrhoidal enteritis. This decision is the more creditable, since his countrymen Hildenbrand, quoted by him, deems lightly of the influence of venesection in enteritis, and has not much faith in leeches. Supported, however, by authorities extending from Hippocrates and Aretæus through Quarinus, Fordyce, Hoffman and Bang, to Abercrombie, the learned Professor adheres to (what we deem) the orthodox practice, but subjoining, "ne quid nimis!" We have a crisis to come, and blood must be spared that this may be accomplished. We think the word crisis vague, and we know that amongst continental physicians it is made a plea for feeble proceedings. It is true that we cannot at once cure any inflammation; that peculiar condition of the capillaries which is the very essence of this affection, cannot be annulled at a blow. What we really effect by bloodletting and other antiphlogistic measures is the placing of the inflammation within the sanatory power of the constitution. By such measures, the dilated and distended vessels commence the process of contracting and unloading themselves, and the resorption of effused fluids is begun. The completion of the process may require days, and during this period additional means of the same nature as those originally employed may be demanded to accelerate it if tardy, or to combat some augmentation of the morbid condition. How often have we watched the course of this process in the eye! Now what is called crisis, the increased secretion of urine, the relaxation of the cutaneous exhalents, the general relaxation indeed of the system, does not appear to us to be the means by which nature accomplishes her work, but an indication that she has accomplished it, or nearly so.

With regard to internal measures, we are cautioned against saline or other matters which may irritate by contact, but are advised to give oily and demulcent matters, such as almond emulsion, oil of almonds recently prepared, and decoctions of pearl-barley, oatmeal, and marshmallow. Fomentations of infusion of chamomile flowers, or of milk with soap, and oily frictions to the abdomen are recommended. Semicupia and the tepid bath are advised in perityphlitis and other cases of partial enteritis; but where there is extreme anxiety and restlessness they are thought (justly) to increase distress. Dr. Puchelt would limit the use of cold fomentations recommended by Smith, Abercrombie and H. Wolff, and the internal use of ice suggested by Arnster, to traumatic and phlegmonous enteritis. The employment of emollient and oily enemata is suggested. though we are told that even these should be avoided when the lower part of the intestine is inflamed. Metastatic derivatives, such as sinapisms and blisters to the inferior extremities, and the surface of the abdomen are recommended at the commencement of the disease, and the author intimates that metastatic derivation is the principle on which bloodletting and enemata are beneficial. Without entering into a lengthened argument on the matter, we take the liberty of remarking that the latter point is very questionable indeed. When the force of the inflammation is broken, and loss of blood is of no further avail, calomel should be resorted to. It is of the greatest use in serous enteritis, but of less in the mucous and total variety. Where it is counter-indicated, he suggests the employment of mercurial frictions to the abdomen. Severe pain should be allayed by opium, hyoscyamus, or aqua laurocerasi, especially one of the two latter, they being exempt from the constipating effect of opium. We are counselled to employ laxatives of the milder kind in the advanced stage of the disease. Those recommended are castor-oil, sulphate of magnesia in emulsion or gum-water; and where bile abounds, cream of tartar and pulp of tamarinds. Such are the general therapeutic precepts of Professor Puchelt for the treatment of this very dangerous disease. Certain modifications are suggested adapted to its different varieties, such as common sense would suggest to most practitioners.

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* Diss. de Perityphlitide, Heidelberg, 1837, p. 16.

These practical precepts for the management of enteritis, excepting some difference in form, correspond very precisely with the means resorted to in this country. We coincide in his cautions regarding certain points of practice, especially in that regarding the warm bath. We do not know a more barbarous proceeding than the taking of a patient suffering under some acute and it may be extensive phlogosis from his bed to plunge him into a bath. The unavoidable motion and exertion aggravate pain, and accelerate the circulation, whilst the latter effect is produced likewise by the heat of the bath. We have never known such a proceeding otherwise than injurious under the circumstances described; and in more than one case it has appeared to contribute very much to the fatal result. In acute articular rheumatism, to which disease there seems an idea to prevail that it is especially applicable, we have found it especially pernicious. Independent of the disturbance and exertion very injurious in a class of diseases of which repose is one of the most important remedies, the patient is at once transferred from a temperature of from 65° to 70°, to one of 98°, and the latter is applied through the denser medium which transmits it the more rapidly to the body. What but an exciting effect, under circumstances demanding abatement of excitement, can be expected; and such we have ever found to follow.

The author's language regarding the use of calomel in the serous form of enteritis will coincide with the views of British physicians, and is such as certainly a few years ago we should not have heard from any point of the continent.

DYSENTERY. The character of the chapter on dysentery leads us to bestow on it especial notice. It is certainly the most learned and elaborate treatise on the disease we have ever seen. It extends over 103 pages, twenty-six of which are occupied by a literature of the disease, commencing with Hippocrates, and terminating in the third volume of the Transactions of the Medical and Physical Society of Calcutta, published in 1835, and comprising references to the independent works and journal communications of above 1000 writers. It is quite supposable that a considerable per centage of the works referred to may have not contributed much to the abatement of the ravages of the disease; but that such a phalanx of writers should have chosen this theme is evidence of the magnitude that this scourge of armies has possessed in the eyes of the profession in all ages of the world. After a very copious and accurate description of symptoms we are presented with the necroscopy as furnished by Rokitanski. This writer classes the morbid appearances under four heads, according to their intensity. In the first class we learn that under a thin ash-red coloured mucus, the mucous lining of the great intestine is shown swollen and red in lines (striæ); and the epithelium displays either vesicles or furfuraceous scales, under which the mucous coat appears broken, and may be removed in the form of a pulp. The exudation from the intestine is acid. In the second form the same process has extended over a greater space, the mucous membrane in many places being covered with an ash-coloured, tenacious mucous, and itself being transformed into a gelatinous substance which may easily be detached. At the same time many protuberances, arising from serum effused into the submucous coat are visible; the affected part of the intestine is dilated, distended with gases and brown-coloured fluid, its membranes are thickened, and the follicles are marked either by inflammation, copious secretion or softening. In the third variety, these protuberances (arising from effusion into the submucous tissue) are so accumulated, that the internal surface (of the intestine) appears unequal, and studded with eminences (clivosa); the mucous membrane covering these tubercles is partly in the same condition as in the second form, partly changed into a crust firmly adhering, obscurely red, stained with blood, or of a greenish hue, or again, it is entirely destroyed so that the submucous coat appears almost naked. The tubercles are sometimes confluent. The intestine contains a brown, ichorous, fetid, and flocculent matter. In the fourth and last condition, when the disease has attained the highest degree, ample tracts of mucous membrane are changed into a black, friable and, as it were, burnt mass; the submucous coat is seen in the first place full of dark blood, and a *seroso-bloody* fluid, or pale and containing in its vessels a black, pulverulent blood, or finally it is full of pure pus. The affected part of intestine contains within it a cadaverous, brownish black fluid like coffee, and is either dilated as in the former cases, or collapsed, the muscular coat being denser than natural, pale, ash-coloured, elastic and very friable. The serous membrane has lost its brilliancy, it is full of injected vessels, and is covered with a brown ichorous exudation; the mesocolon and the mesentery itself are occasionally in a similar condition; the mesocolic glands are swollen, obscurely red, and turgid with blood. Not unfrequently the morbid process extends beyond the valve of the colon, so that the mucous membrane of the small intestine is affected in a slighter degree than the colon.

Besides this ample detail, Professor Puchelt deems it necessary to add his own observations, closely corresponding with those of Rokitanski, and quotations from various writers, especially our own, on a portion of the subject not sufficiently described by this writer, but well known to army medical men as the source of that distressing and fatal disease, chronic dysentery, intestinal ulcers. His references on this branch of the subject are to Christison, (Edin. Med. and Surg. Journal, vol. xxxi, p. 217,) to Smith, (Ibid. xlii, v. p. 345,) and to Cheyne, (Dublin Hospital Reports, vol. iii, pp. 28, 32.) The two latter, Smith and Cheyne, mention perforation of the intestine as an occasional result of such ulcers, though Smith had seen but one example, in the transverse arch of the colon, and this to so small an extent, that no effusion into the peritoneum had occurred. Cheyne mentions its having sometimes occurred unexpectedly, and its having been fatal from effusion. Gintrac (Journal de Méd. Pr. de Bordeaux, Jan., 1839,) depicts a singular species of dysentery, in which the mucous crypts of the large intestine were affected in the same way as the segregated and agminated glands of Peyer are in typhus.

We should have considered the evidence from these morbid appearances, connected with the well-known symptoms of the disease, as conclusive of its inflammatory nature, and that the inflammation is primarily and essentially seated in the mucous lining and glandular follicles in the colon and rectum. This view, which all our experience of the disease shows to be the only correct one, is here included among various other opinions of the nature of the disease, and is supported by the authorities of Fournier and Vaidy, Broussais, Foderé, Neuman, Müller, Jaeger, and Jarwandt. The author's opinion approaches to this, so far as we can discern it through the not very clear medium of the language in which it is conveyed. The following is his own account of it:

> "I should think that the blood, in the first place contaminated, or in some degree poisoned by a *miasm*, produces in the intestine, especially in the colon and rectum, a peculiar condition which may be compared to an eruption (exanthemat), and which may pass into inflammation, ulceration, induration and gangrene, and produce either simple, inflammatory, nervous, or putrid fever. But I willingly confess that this opinion, equally with others, reposes less on observation than on inference." (Note, p. 644.)

Besides inflammation, or such an approach to it through the two successive stages of contaminated blood, and an exanthematous condition, as our Professor selects, we have dysentery considered, and on able authority, as rheumatism, catarrh of the intestines, spasm of the great intestines (Cullen,) erysipelas, acrid bile, fever turned inwards on the intestines, (Sydenham,) and as a primary affection of the nervous system, the ganglionic system, and of the veins. Quitting, however, this branch of the subject, and passing over his very learned disquisition on the varieties of the disease, of which he enumerates thirteen, quoting very largely from British writers, particularly on the subject of tropical dysentery, we hasten to examine the section which comprises his method of cure.

On this branch of the subject we have much learning, very much reference to the cerated glass of antimony, and to the purchase by Louis XIV of the secret remedy of Helvetius (which proved to be ipecacuanha), and other matters of much moment, in all probability, in a day gone by; but we do not find a rational plan of proceeding suggested, such as in the present state of medical science might have been expected. The vagueness of the Professor's own opinion of the nature of the disease diffuses itself through the section on treatment. He tells us that he would treat dysentery as he would smallpox, scarlatina, continued fever, or typhus; that any primary plan of treatment, which may break the force of the disease, not being clearly indicated, we must acquiesce in a mode of proceeding adapted to the symptoms, unless we choose to act on this or that hypothesis or preconceived opinion. We think that not only has pathological research, but—what we deem more deserving of reliance—clinical observation been employed in vain, unless we have attained to some principles in the treatment of this disease. It is true that the actual practice may vary in force and, in some degree, in kind in individual cases; yet some principles, some adaptation to a recognized pathological condition should be discernible in it.

Of general bleeding he speaks with excessive reserve, questioning whether it abates the intensity of dysentery as it does that of other inflammations, and expressing the opinion, that in a mild disease it is superfluous, and in a debilitated and exhausted frame and a malignant dysentery it is injurious. The whole weight of his own doubts is thrown into the scale against bleeding; yet in its favour he quotes the highest names in medicine—Sydenham (himself a host), Huxham, Akenside, Zimmermann, Mursinna, Rademacher, Somers, Bampfield, Annesley, Antenrieth, Mautz, Hauff, Cheyne, Juncker, and P. Frank. This last writer states that he had on one occasion bled a patient in dysentery three times with a successful result; and the same distinguished authority mentions the prevalence at Strasbourg of an epidemic of dysentery which was exceedingly fatal until medical men resorted to bloodletting. Our own experience is in accordance with the views of these distinguished parties. Many cases, certainly, have we seen successfully treated without bloodletting; but in severe cases abroad and in the provinces of this country, where general excitement of the system has been associated with indications of severe local affection we have found general bloodletting very beneficial, and in some such cases, as it appeared to us, essential to the cure. Leeches he approves of, once or even more frequently applied, provided architis (inflammation of the rectum) be existing, or peritonitis impending; and recommends the anus, perineum, or sacral region as suitable points for their application. Of opium he speaks with general praise as a remedy of dysentery, saying that there scarcely exists a case in the treatment of which it may not find place. He quotes the examples of British writers—Christison, Smith, and Cheyne—who have themselves administered and recommended to others very large doses of this drug in this disease. But he deems it necessary at the close of his quotation to insert a caution, implying censure on the practice of these writers. "Beware," says he, "of following these examples without due consideration, for they show rather what the human frame can bear than what ought to be done."

On the subject of calomel as a remedy of dysentery, he confines his information to what has been delivered by British writers; and these, with his characteristic industry and research, he quotes in abundance, from the days of Cleghorn to those of the Johnsons, Annesleys, and Ballinghalls of the present time. But no comment of his own does he offer, either on what has been called "the scruple-dose practice of India," or the more moderate proceedings adopted in temperate climates. For our own parts we acknowledge the value of mercury, associated with depletion—general, local or, both—in dysentery, as we do in other inflammatory diseases; but we prefer in this disease the forms less calculated to irritate the bowels, such as the gray oxyde, conjoined with opium and ipecacuanha, as they exist in Dover's powder.

Next follows an enormous list of tonics, mucilaginous and bitter, astringent and bitter, merely bitter, and those called antidysenteric. Of these medicines, vegetable and mineral, *we* find a difficulty in referring a considerable proportion to any recognized class in the *materia medica.* The author acknowledges that many of them have been introduced lest he should appear to have omitted anything; a charge very unlikely indeed to lie against our learned author.

We now take leave of Professor Puchelt, without, however, professing to have given a complete review of his work, but only specimens of his manner of discussing the more important and familiar of the diseases belonging to the department of general pathology allotted to him. To whom would we recommend this, the completest treatise on diseases of the alimentary canal we ever saw? Not to the practitioners of busy England: something more compendious is required to answer their prompt demands. But we can recommend it to the medical *literateurs* of this or any other country, as a wholesale storehouse, whence materials may be drawn for the purposes of retail doctors, both in town and country. Might it not merit the notice of the Sydenham Society? By one feature of the work our love of country (no discreditable feeling, we trust) has been much gratified—the very large proportion of its vast wealth that is derived from British sources.

Art. IV.

Praktische Monographie der Bandwurmkrankheit durch zweihundert sechs Krankheitsfälle erläutert. Von Dr. Andreas Ignaz Wawruch. Mit einem Vorworte von Dr. Ignaz Rudolf Bischoff.—*Wien*, 1844.

Practical Monograph on Tapeworm, illustrated by Two Hundred and Six Cases of the Disease. By Dr. A. I. Wawruch. With a Preface by Dr. J. R. Bischoff.—*Vienna*, 1844. 8vo, pp. 212.

The above work, just issued from the pen of an experienced veteran professor and practitioner of the Austrian school of medicine, upon a curious and somewhat difficult subject, we have perused with considerable satisfaction; and conceive that we shall perform no unprofitable task in furnishing some account of it to our readers.

The author introduces the subject with some interesting gleanings, in the way of history, respecting the extent to which the existence of intestinal worms, with their pathology and treatment, was known to philosophers and physicians of the ancient world, including those of India, Egypt, the Hebrew nation, Greece, and Rome. The perusal of this part of the work will amply repay the German reader, supplying matter to the curious in such matters both instructive and entertaining. It does not, however, fall within our design to furnish any sketch of these literary records; our present object is to exhibit to the reader just so much of the substance of the work as shall appear to possess direct practical importance.

The usual division of tapeworm into the *tænia lata* and *tænia solium* is, in the estimation of our author, quite sufficient for all ordinary considerations; although it is his own conviction that between these two well-marked species there are many intermediate, exercising, however, no differential influence either upon the pathology or treatment.

The physical description and characteristic differences here afforded of the two leading species of tapeworm, correspond very much with those which are given in most elementary treatises on the subject accessible to the English reader; hence, we pass over this portion, and proceed to other matters bearing on the physiology of the subject.

The origin of a worm and the coming into existence of a world, excel, in a like degree, the capacity of the human understanding. The most brilliant and acute theories respecting the mode in which intestinal worms arise have ever crumbled into dust, when the interrogative *how* has been too closely proposed. In Dr. Wawruch's estimation, the problem of spontaneous or equivocal generation must ever remain unsolved; he states, however, that experience speaks too loudly in favour of the independent origin of worms to render it necessary to take refuge in the idea of continuation by generation. The nutrition and growth of the tapeworm, when born, is almost as difficult a matter as the question of its origin; the organs of prehension and absorption being verified with less certainty and precision in this than in many other species. It is left for the departmental student to decide whether the growth of the body be merely from the head downwards, or, what is more probable, by absorption of nutrient fluid through the entire surface of the skin. It is a certain fact that hunger, on the part of the patient, constitutes the most perilous opponent—yea often occasioning the death—of this parasite. Notwithstanding the occasional activity of its movements which, in many instances, distress the patient most grievously, no system of nerves has, as yet, been demonstrated; nor, indeed, any well-marked fibres adapted for motion; the contraction and extension of its joints, as evinced during protrusion of a portion from the rectum, and alternate retraction of the same, leave little doubt, however, of their existence. Moreover, that the movement of the worm through the entire track of the alimentary canal, in each direction, must occur with great activity is clear from the frequency of knots, observable in its track, which can only have been formed by the head having made a sort of leap through loops arranged by the disposition of the body.

Unlike that which obtains in certain other species of worms, the *lumbricus teres* for example, where the sexes occur separately, propagation taking place by the congress of male and female, the tapeworm is hermaphrodite, containing within each individual of the species both sets of organs designed for reproduction. Respecting the precise mode in which this function is performed, a variety of theories have been proposed, none of which, however, are at all matter of demonstration: and, for the present, the question remains entirely undetermined. With respect to the way in which parts torn and cast off are regenerated, the author summarily rejects the old notion, prevalent before the discovery of the head, that the worm has its growth at each extremity; as also the proposition that its increase in length takes place by extension of the individual joints; he himself advances that the tapeworm makes good its length, after fragments are thrown off, through extension of the indistinctly articulated folds about the neck, and by apposition of new joints to the end of the tail; a proposition which he considers in great part proved by Andry's striking experiment, wherein this acute observer stuck a silken thread through a protruded limb; this action, of course, occasioned the animal's speedy withdrawal into the alimentary canal; a month afterwards, the patient took a drastic purge, which caused its expulsion; and, since the experiment, it had obtained an addition of about forty joints below the thread, constituting by so much an increase to its length.

Tænia alone, amongst intestinal worms, would appear occasionally to attain a considerable age; the more so, in the instances where it happens to arise in the more vigorous stage of human existence. There is always a difficulty, however, in determining the actual age, in consequence of the uncertainty, whether the duration of the disease be kept up by the presence of one and the same individual, or by continuous regeneration. In one of his own cases, the author states that the disease had had an existence of thirty-five years; and, notwithstanding the apparently successful application, repeatedly, of what had been considered an effective cure, the patient was yet not freed. The age can with little certainty be predicated from the appearance of the portions passed off. Generally speaking, however, the young one is tender, and readily torn; the adult (so to speak) is firm in texture, vigorous, and vivacious; and the aged has commonly a surface marked by frequent wrinkles, and its body is, moreover, of a leather-like tenacity. Sometimes the worm will grow old with man himself, and accompany him even to the grave.

Tapeworm has its principal seat in the small intestines, although, on account of its length, it may extend through the entire track of the alimentary canal, from the pylorus down to the rectum. The presence of this intestinal pest would seem to be considerably under the influence of certain specialities in the soil; thus, there are countries in which it is seldom or never observed; for example, in Quito, and in the higher tracts of Bohemia; a circumstance explicable in part probably by the great elevation of these places above the level of the sea. Low lands subject to floods, the vicinity of the ocean, especially at the mouths of great rivers, and deep valleys, seem most exposed to this evil. What is somewhat remarkable, particular countries seem to possess a species proper to themselves, and some even a distinct genus. The inhabitant of Switzerland, for instance, whilst in his own country, will suffer from the presence of the *tænia lata;* this shall have been got rid of, and, on changing his residence for Vienna, he shall be infested with one of another genus, the *tænia solium.* The German, emigrating to Russia, shall experience the development of the *tænia lata,* a species rarely if ever occurring in his own country. These are curious phenomena, complicating still more the difficult inquiry respecting the origin of intestinal worms.

As regards the symptoms which denote the existence of this disease, it is with tænia as with others of the entozoa, the only certain indication of its presence is the actual dejection of some part of the parasite, seeing that every morbid feeling or appearance induced by it may be occasioned by other causes. Nevertheless, the intensity and duration of the symptoms, with the mode in which they are associated, will, in a great many instances, furnish the highest probability respecting the true character of the ailment, and sometimes render it all but matter of absolute certainty. Signs of derangement in the state and functions of the alimentary canal, with the sympathetic results, constitute the sum of the symptoms; and of these we have a graphic account in the work before us. As it corresponds, however, very much with the experience and reading of all who have, in any degree, studied this matter, we shall not transcribe any portion of it, but proceed to the examination of the next chapter.

Diagnosis. It has been before stated that no combination of suspicious symptoms can render the presence of the tapeworm-disease a matter completely assured; the dejection of some portion of the animal is required to lead to a certain diagnosis; but, generally speaking, a mere glance at the rejected fragment will enable the experienced physician to recognize, not only the fact of its being tapeworm, but, moreover, the particular species to which it belongs. Sometimes the patient's description of the part thrown off will be sufficient for the practitioner to determine whether the disease be the tænia *lata* or *solium;* for Dr. Wawruch states, as a conclusion arrived at from his own observations, that the former parts only with great pieces of at least from six to twelve inches in length; whilst the latter generally casts off a number of little gourd-like limbs. This diagnostic circumstance rests upon the different structure of the two species; the one is rendered firm and coherent in its texture by certain longitudinal lines running down its body, whilst the other, besides being deficient in these lines, possesses a much looser tissue, and one which, during intestinal action, is very readily torn.

As exhibiting the facility of mistake in the diagnosis, on the part even of the most experienced, and the necessity of careful investigation in all cases prior to the exhibition of powerful remedies, we take the following statement:

> "I cannot refrain from mentioning a remarkable case which placed me, in making the diagnosis, in the most painful embarrassment, at least for a short time. A patient, who for some years had suffered from worms, besought the aid of the late most excellent Dr. Bremser, (deceased, alas, too early for science!) who, after a brief and superficial examination, prescribed for him his electuary and a small flask of the (so called) Bremserian oil, doubtless upon the supposition that he was infested with tapeworm. After a long and fruitless employment of both medicines, the patient became affected with the most vehement griping, bloody urine, tenesmus, and finally an excessive diarrhea, which caused the ejection of an entire roll of half-decayed material and tape-like skins. In the certain conviction that the destruction of his monster had been accomplished, the patient ran to Dr. Bremser, and related the occurrence, who, trusting to the account, fortified the patient in the glad supposition, and counselled a perseverance in the use of the stinking oil. This was done, punctually, for some time. But the bloody urine,

and a vehement burning pain in both kidneys, compelled the patient to desist from taking the oil. The wretched man now came to me, and brought in a glass the portions that had been cast off by stool. At first sight I was startled, and hesitated so about the diagnosis, that I requested the person seeking my advice to leave the problematical fragments with me for further examination. I soon attained complete certainty. I remarked that the long skins lying flatly over one another were unjointed, that indeed they must have been cylindrical; since, by obstructing the air at one end with a small pointed reed, they could be inflated, and be made to assume a rounded tubular form. I saw now, plainly enough, that they were *exuviæ* of the common round worms, burst, and, in great part, decayed." (p. 47.)

It is observed that the diagnosis of tapeworm malady, when no ejection of fragments takes place, is so difficult and uncertain as to be exceeded by that of no other disease. It places, at times, the most experienced practitioner in the greatest embarrassment; seeing that many of its general symptoms are common to it and several other diseases, from which, however, before treatment, it must be carefully distinguished. The author enumerates colic, hysteria, hypochondriasis, hemorrhoidal affections, common intestinal worms, and certain nervous affections, as occasionally presenting features likely to complicate the diagnosis, in so far as they, in common with tapeworm, disturb in a variety of ways the functions of the abdominal viscera.

Etiology. The etiology of this disease is discussed under the two heads of "predisposition" and "exciting causes." Among the circumstances *predisposing* to the malady, our author mentions sex as exciting no inconsiderable influence, the female being more liable to its attacks than the male. For this greater tendency to the development of tapeworm on the part of the female, no satisfactory reasons can be given; it is an observed fact; and the causes are as difficult of explanation as the disproportionate prevalence of gout, apoplexy, or stone in the bladder, with the other sex. Some reason may probably be afforded in the earlier occurrence of puberty in the female, in the softer skin, and in the weaker condition of the digestive organs, and consequent greater abundance of mucus, as tænia is most frequently coincident with the reproductive stage of existence, and general laxity of tissue; and although it cannot be denied that occasionally, both in old age and at the earliest period, this worm will be found, it is yet a comparatively rare circumstance and an exception to the rule. Dr. Wawruch states that, in all his experience, he had never had to treat a female in whom the first symptoms seemed to have arisen after the age of childbearing. Each epoch of life would appear to favour some peculiar parasitic formation; thus, infancy tends mainly to the production of round worms, ascarides, and common lice, whilst senility seems possessed of immunity from these annoyances; and, in correspondence with such facts, it is considered that the origin of tapeworm has some special alliance with the generative period of life, and its more vigorous stages.

Notwithstanding the objections that many have offered to the idea of there being any hereditary predisposition to this affection, it is here maintained that the tendency is frequently propagated from parent to child, just as much as the liability to gout, epilepsy, mania, hysteria, and several other ailments of a like kind. The author does not teach that it is the actual *material of generation* which is transmitted, but rather that it is an unfathomable *something* which no human mind will ever thoroughly appreciate, but a something which may yet be demonstrated as truly existing. An irregular mode of life, interfering with the integrity of the digestive function, would appear to operate at one and the same time, as a predisposing and as an exciting cause; and, for obvious reasons, whether the malady be regarded as mainly dependent upon a cachectic habit of body, or as the consequence of nervous debility. The occupation is a circumstance to be studied, as probably standing in some relation to the development of this disease. Dr. Wawruch, in reviewing his registers of cases, considers that an undue preponderance obtains among shoemakers, tailors, cooks, and butchers; a circumstance probably to be explained by these classes frequenting, in undue proportion, the hospital where the observations were chiefly made. The influence of the probable injury done to the energy of the assimilative powers by a sedentary mode of life in some of these instances, or by excess or deficiency of proper nutrition in others, is duly recognized; but yet, to explain the frequent occurrence of tapeworm among butchers, he suggests, with some confidence, that the emanation from fresh-slaughtered cattle, as a true animal material, not only contributes to obesity, but must also tend to the generation of parasitic organisms; an idea which he leaves to the physiologist and practitioner to estimate at what they regard as its just worth. We think that the fact, so far as true, receives a simpler explanation from the well-known circumstance that these men most commonly overload and unduly stimulate the stomach. Our own experience—not very extensive, however—has not led us to ally the affection especially to any particular business; but we have generally observed it to prevail where causes prejudicial to the assimilative functions have operated to a great extent, irrespective of class or occupation.

Amongst other predisposing circumstances the existence of previous attacks of intermittent fever is here mentioned; and the author states that he brings to this idea, long since advanced, though often disputed, the solid support of facts. He observes that both diseases belong to the most vigorous epoch of life—to that period when the generative function is the most powerful; that intermittent fever almost ever leaves its after-evils in the viscera of the abdomen, the locality of tænia; further, that in those countries where the one affection prevails extensively, so does the other; that, for example, in Prague, where ague is seldom heard of, tapeworm is almost unknown; and that in Vienna the two diseases exist both in large proportion. Finally, he affirms that, upon a revision and comparison of his hospital registers for upwards of twenty years, almost all who were treated for tænia had suffered at an earlier period from intermittent fever; a fact to the confirmation of which, or otherwise, the attention of the profession in this country would be well directed. Feverless affections of the skin—such as the itch, scaly tetter, certain forms of porrigo, and herpes—operate, according to our author, as predisposing causes; a conclusion deduced by himself from his own observations, and confirmed, he informs us, by those of his friend Bischoff; and one attempted to be explained by the intimate relations subsisting between the skin and mucous membrane of the alimentary canal.

In discussing the exciting causes of the disease in question, Dr. Wawruch, at some length, proposes to show what he conceives to be the erroneous character of the views commonly entertained with respect to the influence of diet; he maintains that almost the exclusive adoption of a purely vegetable diet, or of one vegeto-animal—such as eggs, butter, milk, cheese, and so on—is not conducive, as many have supposed, to the generation of worms; and in support of this proposition, he appeals to numerous facts, including his own recorded and detailed experience. He adduces the statement of Professor Reinlein, who had paid considerable attention to this affection, that in a cloister where, for an indefinite period, the rule had been to partake only of vegetable diet, with an occasional use of eggs, milk, butter, cheese, and fish, not one of its inmates had been ever known to suffer from tapeworm. A rich, animal, and stimulating diet he regards as a much more powerful cause of the development of the malady; and, as before stated, his own patients have been persons subjected to the operation of this influence, in very undue proportion. He mentions, moreover, that, on appealing to classical antiquity, a confirmation of this view will be obtained; for Theophrastus of Cresus affirms that in Athens, except among the Athletics, tapeworm was hardly ever witnessed; and the diet of these latter was notoriously of a gross, animal kind. Thus, when Diogenes the cynic was asked why the athletics were so rude and uncouth, he replied, with his customary abruptness, because *they lived on hogs' lard and cows' flesh.* Our author states that the conclusion in this respect has been forced upon him by experience, as it is in opposition to his preconceived opinions. He allows, however, that the following out of any plan of diet, if succeeded by any notable derangement of the alimentary functions may tend, in the predisposed, to originate the parasite. We think so.

Terminations. Tapeworm has never been known to issue directly in death, not even in those cases where the patient, deprived of all medical aid, has been affected by it uninterruptedly, from earliest youth to old age. Indeed, it may be affirmed that, in many cases, human life is so little endangered by its presence, that sometimes, in opening a dead body, a large tapeworm will be discovered, of the previous existence of which neither the physician, from observation, nor the patient, from feeling, have had the remotest idea. An actual cure, however, from the unaided efforts of nature is so exceedingly rare, that it should never be anticipated in any individual instance; although a spontaneous liberation will occasionally occur, and that, too, without being followed by regeneration. The influence of tænia in the production of other ailments, sympathetic or more remote, has been vehemently maintained and as strenuously opposed. Whilst many names of high repute have held that a malady is hardly to be found, especially affecting the nervous system, that may not be induced by intestinal worms; others, of equal authority, have contended that their presence is conducive to the perfection of health, by their fulfilment of an appropriate office in the alimentary canal—probably the absorption of some viscous residuum of the chyle. It is truly a melancholy task for the student in medical literature to acquaint himself with much of the past history of his profession, discovering, as he almost ever does, the total want of harmony in the results obtained, up to a late period, by observers in so many departments; it is nearly always, as in the present case, one group of authorities say one thing, another say just the opposite of the foregoing, and the truth being seldom altogether with either party. We have read carefully over the chapter in the volume before us, in discussion of the terminations of the tapeworm-disease, and cannot help thinking that, although Dr. Wawruch is at no extreme point, he somewhat overrates the secondary consequences. Our own conclusions—deduced alike from reading and experience—certainly are, that no *special* ills flow from the presence of tænia, but only such as usually result from protracted irritation of the gastro-intestinal mucous membrane, upon whatever cause dependent; and that just in so far as worms of every kind aggravate or induce this state of things in particular individuals, they bring about the diseases of a class generally recognized as coming from this source; and that, in habits of body attended by undue sensibility of the lining tissues of the stomach and bowels, tapeworm may induce slow hectic fever, marasmus, cerebral disease, convulsions, and so on, in the same way as such results will often arise in the absence of all parasitic formations.

Prognosis. As before observed, tænia prevails more extensively in the female than in the male sex; and, in like manner, success in treatment is to be anticipated with the latter much more readily than with the former. The explanation of this phenomenon is to be sought in the firmer corporeal structure of the male, and in the greater activity of man's general career. On these accounts the male system allows of a freer employment of the means of cure, and, where necessary, of a more protracted continuance thereof. Another circumstance favouring the prognosis, with respect to the curability of this disease in the male, is to be found in the comparative uniformity of condition that obtains in man. Women, from very slight causes, are liable to fainting, to hemorrhages, to convulsive attacks, and nervous affections of every kind; to say nothing of the occurrence of the catamenial periods, of pregnancy, or parturition, lactation, and the more decided climacteric changes; all which things offer special hindrance, in many instances, to the adoption of appropriate therapeutical measures, or at least to an adequate perseverance in the same.

As the malady under consideration is one mainly affecting the mature period of life, from the accession of puberty to full manhood, and through the whole of middle age, so at those times is the prognosis of cure the most certain. When the disease, however, does occur in children sufficiently energetic measures, with a view to expulsion, can rarely be had recourse to, for obvious reasons, especially in the case of such as are rickety and scrofulous. In like manner, old people but little endure effective treatment, particularly if suffering from habitual giddiness, asthma, chronic cough, debility of the bladder, or threatnings of apoplexy. Our author goes on to speak of the influence, as modifying the prognosis, of temperament, corporeal structure, mode of life, hereditary constitution, idiosyncrasy, and some other circumstances. We do not, in his remarks upon these matters, notice anything very striking; they seem introduced more to give completeness to the discussion than for their novelty, being chiefly a repetition, in other words, of what had been previously advanced, and which we have before alluded to.

The tænia lata is more readily expelled than the tænia solium, and that without there being any difference in the remedies employed. No satisfactory reason can be given for this fact. It is suggested that the former species may be endowed with life less *intensively*, and, on that account, be the more speedily destroyed by pharmaceutical means. The fact itself, however, the author puts in the form of an aphorism, and rests it upon simple experience. In the same way, he advances a series of propositions, constituting what he calls the "empirical prognosis," following each of them up with brief explanatory observations. Some of these we shall translate literally, and there leave them; premising only that in each of the following aphorisms he speaks but of the tænia solium (kettenwurm); though, from the context, we presume that he refers to the tapeworm generally.

The advanced and completely formed tapeworm is more easily expelled than one too young. Joints that are of a decayed, withered, ill-coloured, leathery, and shrivelled appearance indicate a favorable prognosis; but those of a bright white character, and in active motion, point to one that is less so. The tapeworm which has but lately parted with considerable portions of its length is more difficult to expel than that which daily allows individual joints to break off, be these ever so numerous. The tapeworm, frequently but ineffectually attacked by strong remedies defies the expellent treatment (*abtreibekur*, a term apparently used exclusively in reference to his own method,) much longer than that which has for years been uninterfered with. The tapeworm, associated with round worms or with ascarides, is seldom or never expelled in the first course of treatment.

The accession of vomiting some time after the medicines have been taken often indeed delays, but does not always frustrate the successful issue. The occurrence of diarrhea too early or too late is prejudicial to success. The more courageously the patient takes the remedies, and the less unpleasantness he experiences from them, the more certain is a fortunate result. The success of a first attempt at cure guarantees a similar result a second time, in case of a regeneration of the worm. Success occurs more frequently in winter than in summer; greater circumspection in the treatment, however, being required in the winter season. The next aphorism may probably provoke a smile, on which account we forewarn the reader. The last four or five days of the waning moon seem favorable to a happy issue. The author, however, states that he has, for nearly twenty years, been in the habit of watching the real or imaginal relations subsisting between intestinal worms and the phases of the moon; and as a simple affair of experience, which he does not attempt to explain, he avows his reliance upon the accuracy of the proposition just adduced.

The prognosis in respect of the probable regeneration after expulsion is set forth in the following propositions, each, as before, being succeeded in the work itself, by an appropriate commentary. The chief of these we shall give literally. The expulsion of the worm, together with its *capital* extremity, does not always ensure against the reappearance of the same. The inability to discover the thin cervical portion, with the head, is no proof of failure of the treatment, and furnishes no ground for an unfavorable prognosis. It is, thence, only a prejudice, that the tapeworm, if torn, must always be reproduced. The period (after expulsion) of six, eight, at most ten weeks, is to be regarded as the surest criterion of there being no probable regeneration. The expulsion of two or more young tæniæ at the same time gives the least favorable prognosis. The continuance, on the part of a patient, of the previous mode of life, after expulsion, lets the speedy reappearance of the tænia be feared; and the contrary, in the opposite case. The frequent repetition of the expulsive treatment seems able, with certainty, to extirpate the worm at last.

Therapeutics. The curative treatment of tapeworm is one of the most difficult problems in practical medicine; a proposition, the proof of which is eloquently furnished by the host of remedies recommended and employed for thousands of years. Many practitioners, indeed, have affirmed with some truth that, in some cases, it is easier to kill the man than the worm. In adopting the treatment for expulsion shortly to be described, certain contra-indications are to be borne in mind, which either prohibit its employment altogether, or at least necessitates its postponement; these, for the most part, are included in considerations respecting age, sex, peculiar habit of body, actual or previous disease, with some other circumstances, most of which have been before alluded to. We notice one statement in this chapter, which we have a difficulty in reconciling with another before quoted under the head of prognosis; it is set forth that the cold, wet weather of autumn, and the vigorous cold of winter, are far less favorable to success in treatment, than the mild, serene weather of summer; whereas, a few pages previously, the author had dogmatically announced that success followed treatment more frequently in winter than in summer. The truth very likely is, that temperate weather, as interfering least with the general health, is the most conducive to success; and, it is to be supposed that in the enunciation of the former aphorism, our author contemplated a mild winter in opposition to a high temperature in summer, which latter at all times enfeebles the digestive functions.

The method of treatment proposed by Dr. Wawruch, and pursued by him for twenty years with the happiest results, is exhibited and discussed in three divisions; 1, the *preparatory* or abstinent treatment; 2, the *curative* treatment proper, and 3, the *after* treatment.

1. In preparing the patient for the administration of remedies destructive to the worm, our author for three or four days previously withdraws all solid food, allowing a little mild soup several times in the day. In order to render the alimentary canal more susceptible to the coming treatment, the patient, during this period, takes the following solvent:

℞ Radicis Cichorei et Taraxaci ää unciam unam coque in sufficiente quantitate aquæ per mediam horam; Colaturæ fortiter expressæ unciarum sex adde
Salis ammoniaci depurati scruplum,
Syrupi Cichorei cum Rheo unciam semis.
Detur usui, signetur: "Two tablespoonfuls to be taken every two hours."

The object in the above prescription is to carry away by gentle means all impurities in the *primæ viæ*, at the same time that a more suitable predisposition is obtained for the next step. When, after a careful consideration of all the possible contra-indications, the time for proceeding becomes fixed, the patient, on the evening previous, must take a rich panado, made of bread-crumb, water, and several ounces of butter; and, at the same time, he must have, within the space of an hour or two, three or four enemata administered, constituted of linseed tea with equal parts of milk and oil. On the following morning, that of the appointed day, there must be a repetition of the same clysters and the same soup.

To this preliminary treatment, Dr. Wawruch attaches a very great importance; so much so, that he ventures to affirm that the success or non-success of the entire plan will often depend upon its strict observance; as the temporary withdrawal of the customary nutrition attacks the vitality of the parasite at once; for, according to an old saying of Alexander of Tralles, "hunger is the tapeworm's mightiest foe." Abstinence from food ever aggravates the subjective symptoms, inducing a sort of desperation on the part of the intestinal lodger; and, after this has continued for some time, its rage for food becomes so considerable that whatever comes in its way is greedily consumed, even the medicament administered for its destruction. Another circumstance explaining the utility of the abstinent treatment consists in the fact that, in this way, the excitability of the alimentary canal becomes exalted, and, under the influence of the appropriate drug, its peristaltic action determines more powerfully downwards, expelling the entire contents of the intestines, inclusive of the worm rolled up. Failure in most cases is to be attributed, when it occurs, to insufficient preliminary fasting; and, indeed, in many of his own recorded cases, the cure was completed by this alone, the tænia being passed without a grain of medicine having been taken for the purpose. The employment of the mucilaginous and oily panado and enemata, whilst it enables the patient to endure the abstinence from other matters, has its use also in protecting him from being too powerfully influenced by the succeeding remedies, which otherwise might excite hyperemesis or hypercatharsis.

2. The measures for bringing about the expulsion of the tapeworm must begin early on the fourth day, just on the completion of the preparatory discipline before mentioned. The means themselves are of a twofold character, having for their object, first, to destroy the worm, and then to expel it from the body. For the first purpose, castor-oil and the male fern, are the remedies recommended; and, in the case of the former, it is of consequence that it should be recent, and have been expressed from the seeds including the husks, in which latter our author says the anthelmintic property seems to reside. He is accustomed to exhibit this medicine according to the following formula:

℞ Olei Ricini Americani ex seminibus cum pelliculis recenter pressi uncias duas.
Detur usui. Signetur: "Two tablespoonfuls to be taken every hour."

This he alternates with the powder of the male fern, as indicated below:

℞ Pulveris radicis filicis maris drachmas tres. Divide in doses numero tres.
Signetur: "One powder stirred up in tea to be taken every hour alternately with the oil."

These measures, we are informed, tend to the fulfilment of the first intention, that of effecting the destruction of the worm. The actual removal of the tænia, dead or alive, is aimed at by the exhibition of calomel in union with gamboge. The author never, but with the greatest reluctance, substitutes any other drastic, however similar in its operation, as none besides, in his own estimation, act so beneficially or so specifically. The dose is regulated according to the particular diathesis of the patient; but, generally speaking, and on an average, the remedy is taken according to the following formula:

> ℞ Calomelanos.
> Pulveris Gummi guttæ [Cambogiæ.]
> Sacchari Albi aa grana sex.
>
> Exacte terendo fiat pulvis. Dentur tales doses numero tres. Signetur; "A powder to be taken at one, three, and five in the afternoon."

To provide against any possible nausea or disgust at these remedies, the author is in the habit of being provided with some ounces of candied orange-peel to serve as a means of clearing the mouth by mastication, and also with some infusion of lime-tree flowers, to be drunk after the medicines have been taken. It is also advisable to have some emollient fluids in readiness, in case of clysters or fomentations being required for any purpose.

These various means require the strictest regularity and order in their management; and the precise mode prescribed of administering the remedies must be conscientiously adhered to. In support of this injunction, Dr. Wawruch appeals to the recognized practical truth which demands that the physician, in employment of any specific mode of cure, should observe a rigorous punctuality, even in things apparently the most trifling. In particular, the succession of doses should receive the most careful attention; for, if these be given too precipitately, they will assuredly be rejected by vomiting, and the object be frustrated. If the intervals be too protracted, the patient becomes depressed and impatient, and, without purpose, suffers longer than he ought.

To resume; our author commences with the final treatment at 8 a.m. by administering the first dose of castor-oil; at 8½ a.m. the male fern is taken; at 9 a.m. the second dose of castor-oil is received, and in half an hour the next powder of the filix mas. At 10 a.m. the third and last dose of oil is given, and at 10½ the last of the powders; in the hour next ensuing, one or two injections of milk and oil are administered. From this time till about 1 p.m., repose in every respect is conceded to the patient, in order that the medicines received should duly attain the intestinal surface, and be able to exert their influence upon the tapeworm itself.

At this period, nausea, headach, and griping most commonly set in, in such a manner that the patient will often conceive, from the intestinal commotion, that he can point out the precise location of the parasite. If vomiting now arise, the entire process is procrastinated, as in that case the whole of the third dose, or at least one half of the male fern powder must be repeated. If this interval, however, pass without the occurrence of any material accident, the practitioner must proceed to the administration of the gamboge and calomel, selecting, for fear of vomiting, the moment of greatest tranquillity on the part of the patient. It is well to let him have some candied orange-peel just after the dose has been taken, as this often settles the medicine on the stomach. Time must be allowed before a second exhibition of the drastic; if nothing result for an hour or an hour and a half, the next dose may be given. Most commonly, however, if the medicine do not disagree, intestinal action will speedily take place after its administration; and the patient, with violent spasmodic action of the sphincter ani, expels the tapeworm. Of course, after this event, no further drugs are given, even though the head of the parasite be not discovered, as the cure is not the less certain on this account. Soon after the expulsion, all inconvenience to the patient ceases, who feels thoroughly refreshed, and as one regenerated.

The above description of treatment, with its results, is drawn from the more fortunate cases, a period of six hours at the most, being generally consumed in its progress. At times, however, the occurrence of fever, or of undue nervous excitement, during the preliminary abstinence, will necessitate the postponement of the ultimate remedies; at others, an indomitable vomiting has seemed to arise from the bulk of the male fern, which may equally frustrate the intention of the physician. Dr. Wawruch does not approve of the exhibition, in these cases, of the *filicin* as a substitute, according to the suggestion of the Baron von Jacquin, as, in three instances in which he had given this alkaloid, most severe and troublesome symptoms followed its use. The castor-oil also, and the drastic of gamboge and calomel, will occasionally so excite the stomach and bowels, as to render the practitioner's course both difficult and doubtful; and, here, acting upon general principles, he must be guided by judgment and discretion.

Another embarrassing circumstance consists in the expulsion of the worm not taking place somewhere about the customary period, since it creates a doubt whether the drastics should be withdrawn, or be continued to an unwonted degree. In these cases, the author's experience leads him to wait, and generally in a day or two, probably after the idea of cure has been given up, the wished-for result occurs without any medicine being given anew.

Whether an attempt, as here described, at the effective cure of tænia be successful or otherwise, the after-treatment must be decidedly such as shall tend to obviate the fear of intestinal inflammation, a state of things to be guarded against on account of the excitability induced by the previous proceedings. Our author, in this case, directs the treatment commonly recommended and employed in ordinary irritation of the gastro-intestinal mucous membrane. Some hours after the expulsion of the tapeworm, occasionally in a few minutes, a violent hunger sometimes arises, which must be carefully attended to, as the most prejudicial consequences have been seen to follow the indulgence at this period of the inclination. At times, however, no sense of inquietude, or irregularity of any kind, succeeds the treatment; in these cases, it is desirable not the less to warn the patient to be careful respecting diet for at least a few days.

We now conclude our analysis of this very interesting and valuable monograph. Before closing this article, we may repeat what was stated at the onset, that the doctrines inculcated are illustrated by an appendix containing accounts of 206 cases. These are concisely related, the main features of each being given without that troublesome repetition and detail which often characterize clinical histories. We have looked here and there into the appendix, without, however, having systematically gone through with it; and the published results, so far as we can judge, would seem to coroborrate the estimate which Dr. Wawruch attaches to his own plan. As stated in the title-page, the work is prefaced by Professor Bischoff, in terms highly laudatory of its contents and its author; and, certainly, as a contribution to practical medicine, its commendation is only an act of justice. We suspect, however, that the plan of treatment will hardly ever be likely to become general, at any rate out of the wards of an hospital. Many cases will undoubtedly be cured by a much more simple, and, we may add, less perilous process; and then again, the difficulty of accomplishing the plan in all its integrity would be found insufferably great in a majority of instances. For our own parts, we feel disposed to give the method a trial, in cases that shall resist and baffle common treatment, especially where the patients' anxiety or intelligence would furnish a reasonable assurance of his own cooperation.

To such of our readers as are familiar with the German language, we strongly recommend a perusal of the book itself; it is written in a pleasing and even elegant style it abounds; with anecdote and lively illustration; and it is well calculated in this way, as well as by its practical worth, to recommend its doctrines to the favorable attention of the profession.

ART. V.

Guy's Hospital Reports. Second Series. Nos. I, II, and III. April, 1843, to April, 1844. *London*, 8vo.

AFTER a prosperous course of seven years, the editors of these valuable Reports, having added to their own numbers, have determined upon extending the field of their labours also. In addition to the finished treatises, the works of individuals, which have chiefly occupied the former volumes, they now propose to illustrate the different classes of disease by the aid of a series of Reports, collected within the walls of the hospital, and furnished by the books of the Clinical Society, (attached to the institution;) and likewise, to apportion a part of each number to the consideration of anomalous cases from the same source. Original views may also be occasionally propounded concerning some of the sciences connected with medicine. We regard this plan as one that is calculated to increase the utility of the work in no small degree, if it be fairly and fully developed; and we therefore cheerfully lend ourselves to the task of analysing, after our accustomed manner, the portions which are now before us.

No. I. 1. The first paper which claims our notice is a *Case of suspected irritant poisoning*, by Mr. A. S. Taylor, which, like everything proceeding from the pen of that author, is full of interest. Three members of the family of a shepherd—the wife, son, and daughter, the two latter being young children—were taken ill on Sunday, after dining on some mutton from a sheep which had died of the '*staggers*.' They were better next day, and again partook, with the father, of the same food. In *less than three hours* the boy died; the mother and daughter were found, at the same time, in a state of insensibility; and the father felt, while at work, a sharp burning pain in his inside. No very accurate account of the symptoms could be procured; the mother foamed at the mouth, and was in a state of great nervous excitement; both the children, however, suffered from vomiting and purging, the boy especially. His motions were of a dark-green colour; the matters vomited were very copious, and striated with a yellow-coloured substance: they were, unfortunately, thrown away. On examination of the body after death, the lungs were found loaded with scarlet blood; the liver of a pink colour, and congested with liquid blood. On the posterior part of the stomach there were several inflamed and prominent rugæ, and other parts of the lining membrane presented traces of inflammation. The upper part of the small intestines was inflamed, and they contained a liquid mixed with blood. The peritoneum was highly inflamed; and there were two spots of well-defined inflammation on the posterior wall of the contracted bladder. The cavity of the abdomen contained about two ounces of bloody serum. The upper part of the larynx and lower part of the pharynx were inflamed. The vessels of the brain were congested. It is clear that many of these appearances were unconnected with the action of the deleterious agent, whatever that might be. No trace of poison could be detected in the contents of the stomach and viscera. Suspicion rested upon the father, but the evidence, both in a medical and moral point of view, was altogether inconclusive as to the administration, either wilfully or accidentally, of any mineral irritant poison; and we quite agree with Mr. Taylor in ascribing the death in this instance, and the illness in reference to the other parties, to the use of the diseased mutton. It is, however, a curious and not easily explained fact, that other families had partaken of the same meat without the production of any injurious effects. This may appear to militate against the view adopted, but it is not without parallel, for the author quotes an instance of poisoning by bacon, in which the same thing was observed. We believe this is the only example on record of poisoning by diseased or putrefied mutton, and it is also valuable as showing that death may occur in a much shorter time than has been hitherto imagined. We strongly recommend a careful perusal of the original paper, which contains, in addition to a minute analysis of the whole evidence bearing on this one particular point, a brief but interesting notice of all that is at present known in regard to animal poisoning.

2. *Observations on pelvic tumours obstructing parturition*, by S. C. W. Lever, M.D. This is a continuation of a paper in the seventh volume, and, like it, unsusceptible of condensation. It well merits perusal.

3. *Inquiry into the causes of death after injuries and surgical operations*, by Norman Chevers, M.D. A most important subject, especially when treated, as in this instance, with a view to the adoption of preventive means. There is great variety in the nature of these fatal actions, but they may be arranged in three general groups: viz. 1. The *primary*, occurring either during or very shortly after operations or injuries, such as collapse, hemorrhage, the admission of air into the veins, or the sudden occurrence of any internal lesion. 2. The *secondary*, including those which are developed within a few hours or days, such as tetanus, delirium tremens, irritative fever, secondary hemorrhage, sloughing, erysipelas, and acute inflammations of the various internal organs, of the veins and arteries. 3. The *remote*, which do not cause death until after an interval of some weeks or months, such as profuse suppuration, secondary fever, caries of injured bone, phthisis.

Dr. Chevers's remarks are chiefly confined to the inflammatory diseases included in the second class; and his object is to prove that, in a large proportion of cases, they may be traced to previously existing disease, or a predisposition to disease, in the kidneys, liver, and spleen, but especially in the former. His suspicions were first excited by observing that the morbid characters presented by the serous membranes and other structures, together with the appearances of the effused fluids, &c. in those who died of acute internal inflammatory attacks consequent upon operations or injuries, (especially where the primary wounds were at a distance from the parts afterwards involved,) almost invariably bore a precise resemblance to those which so characteristically distinguish the inflammatory affections of the same parts which are known to result from Bright's disease of the kidney. Subsequent inquiries demonstrated the correctness of this idea. Out of 134 cases in which the patients died of internal inflammations, there was also superadded marked disease of the kidneys, liver or spleen, or of all these organs combined, in 90; and from the character of the symptoms, &c. in many, of the whole number, in which the kidneys were not examined, Dr. Chevers feels confident that the proportion would have been still greater had the post-mortem scrutiny been more complete. In rather a large proportion of these cases the diseases above mentioned had evidently existed for a considerable period before the reception of the wounds or injuries which became the apparent primary causes of death: but in very many (and this was especially observable in the renal cases) the changes were manifestly of so recent a nature as to render it probable that almost immediately after the operations or accidents, either visceral disease had been excited from a latent to an active condition, or that a state of acute congestion had suddenly been established in organs which had hitherto been suffering merely from chronic disorder.

The practical inference to be derived from the above facts is sufficiently obvious, viz. that no operation should be performed upon any patient (excepting, of course, in those cases in which delay would be death), until the state of these important organs is satisfactorily ascertained, and any diseased action which may exist is, as far as possible, subdued. And though the observations here recorded are chiefly applicable to patients similarly situated with those who are the inmates of London Hospitals, yet we cannot but strongly recommend to *all* surgeons a most careful and anxious out-looking in the same direction.

4. *On the structure, functions, and diseases of the coronary arteries of the heart*, by Norman Chevers, M.D. There is considerable peculiarity in the structure of these vessels. The lining membrane is, of course, a prolongation of the internal tunic of the aorta, and like it has a minutely granular surface, when the secretion which maintains its polish is removed by a sponge or by drying. It cannot be separated from the sub-serous tissue. This tissue is a continuation of the very strong fibrous tunic of the aorta. At the commencement of the vessels the fibres form rather thin laminæ; but immediately upon reaching the interior of the arteries, they undergo a great increase of development, spreading out into several layers (three of which, at least, may be discerned upon very cursory examination), and, in short, forming the main tunics of the vessels. In extending along the tubes, these fibres do not take a perfectly longitudinal course; but those of the different layers, spreading out as they descend, cross the fibres of the laminæ above and below them

at very acute angles. This arrangement is also found in the aorta, and greatly adds to the strength of the tubes.

The middle elastic coat is very thin, consisting, apparently, of a single layer of circular fibres, slightly interlaced. External to this, and easily separable from it, is the fourth tunic, strongly reticulated, and plentifully supplied with vessels, and surrounded, throughout the whole of its course, by the intricate network of nerves, from the cardiac plexus, which may almost be regarded as intended to form another covering for the defence of each vessel under distension. Dr. Chevers has observed a very similar arrangement of parts in the superficial veins of the extremities. The structure above described is admirably calculated to enable these vessels to sustain the impetus of the downward current of the blood, and to recover from the elongation which, at every injection, that current must produce in their canals; while at the same time it confers upon them sufficient lateral extensibility to enable them to bear those degrees of distension to which they must be frequently exposed, both when an increased quantity of blood is thrown into them under inordinate action of the heart, and when the passage of their contents into the coronary veins is obstructed by any cause. In reference to the *diseases* of these arteries, Dr. Chevers does not advance anything very new.

5. *On digestive solution of the œsophagus, with a case,* by Mr. T. Wilkinson King. A young man, who had been a hard drinker, had complained of epigastric pain, sickness, loss of appetite, and flatulence for some months. He was compelled to leave a public supper about nine o'clock on account of sickness; vomited slightly, and returned home, but could not walk without assistance. His wife then gave him some castor-oil. He was seen by a medical gentleman about three a.m. He complained of severe pain about the epigastrium and great difficulty of breathing; the abdominal muscles were very rigidly contracted; he was sitting up in bed, and leaning forward on his hands; countenance anxious and pulse soft. Remedies were of no avail. Emetics were administered, but no vomiting took place. The dyspnea increased; the face, throat, and chest were distended with emphysema, and he died at noon. On dividing the cartilages of the ribs on the left side, fluid of a dark and offensive character issued from the chest; the lung on the same side was collapsed, and castor-oil floated on the surface of the fluid in the pleural cavity. There were marks of inflammation on the outside of and within the pericardium. There was a large rent in the œsophagus, where it passes through the diaphragm, filled up with food; portions were also found at the posterior part of the chest. The left end of the stomach was softened by digestion.

We leave our readers to form their own opinion upon this case. To us it appears that the whole phenomena are most easily explained on the supposition of a rupture having taken place during life. Mr. King is of a different opinion, and rather imagines the death to have been caused by sudden inflammatory tumefaction of the larynx, of which we confess ourselves unable to detect any evidence. Nor do we see how the case is corroborative of his views of the different actions of the two ends of the stomach.

6. An interesting *case of glanders in the human subject,* by Dr. H. M. Hughes.

7. *Report of cases of hernia at Guy's Hospital, from Sept.* 1841 *to Dec.* 1842, by Mr. Alfred Poland. This is a lengthy paper, containing the record of forty-five cases; eighteen were the subjects of operation, and of these nine died. Was not too much time lost in the exhibition of remedies before operating in some of the cases? We cannot but think it is the duty of the surgeon to interfere within a very few hours, when the symptoms show no tendency to yield under the employment of the ordinary means. A reference to cases 8, 11, and 25 will, we think, bear us out in these remarks. Appended to the paper is an account of four anomalous cases, which are well deserving of a perusal. In one, fungoid disease of the testis and cord was mistaken for an omental hernia, and the fatal event, in all probability, much hastened by the vain efforts at reduction.

8. *Observations on patients whose urine was albuminous,* by Drs. Bright and Barlow. A valuable paper, well deserving an attentive perusal. Its great length, however, (upwards of 100 pages,) and the nature of its contents, which are for the most part the mere reports of cases, preclude the possibility of analysis within any reasonable limits. A number of the cases are so arranged as to exhibit the influence of particular remedies or courses of treatment; this plan we regard as a most excellent one, and shall be glad to see it more extensively followed. We have risen from the perusal, with a deepened conviction that a *remedy* for these diseases is yet among the things to be desired, and with our previously formed opinion, that *mercury* is decidedly unsafe, not a little strengthened.

How have the editors allowed so many typographical errors to escape their notice?

9. *Observations on the blood, with reference to its peculiar condition in the morbus Brightii,* by Dr. G. O. Rees. In a communication formerly noticed by us (Br. and For. Med. Rev., No. XIV, p. 133,) Dr. Rees explained his views of the structure of the red corpuscle, which he believes to consist of a closed sac containing a fluid, the sac being capable of exhibiting the phenomena of endosmosis and exosmosis. But he had not then determined whether the red colour existed in the sac or in the fluid. The following experiment shows that the latter is the case:

> "The serum was decanted from a specimen of coagulated blood: the clot was next carefully washed in the serum, in order to get as many red corpuscles as possible into the liquor; it was then removed, and the serum set aside, to allow the red corpuscles to subside. Subsidence being complete, the supernatant fluid was decanted, and the deep red, thick mixture at bottom poured into distilled water, in order to burst the membranes of the corpuscles. This aqueous mixture was then well stirred and set aside; when, after a few hours had elapsed, I observed a white deposit at the bottom of the containing vessel, while the supernatant liquor remained clear, and of a fine red colour."

The white deposit, examined under the microscope, appeared to be made up of burst membranes of corpuscles, and of nuclei; and when the supernatant fluid was evaporated to dryness, and the dry mass incinerated, it was found to contain all the iron.

Dr. Rees believes that the iron, which gives colour to the fluid of the corpuscles, is communicated by what is called the *aqueous extractive* of the chyle, and which passes into the corpuscles by endosmosis; the specific gravity of the contents of the thoracic duct in the human subject being 1024, while that of the liquor sanguinis is about 1052, at least. If such be the case, it is quite clear that anything which alters the relative specific gravities of these two fluids must have an influence over the process above mentioned, and, consequently, affect the quantity of colouring matter in the blood. Thus, if the specific gravity of the liquor sanguinis be diminished, (as it undoubtedly is in the morbus Brightii,) there will be less endosmosis of the fluid matter of the chyle, and, by consequence, a deficiency of iron in the corpuscles. In this way Dr. Rees explains the diminution of red corpuscles in these renal diseases. The subject is one of much interest, and deserves further investigation.

10. *Report of cases of fever*, by Mr. J. H. Browne. From this paper we have derived no new information of any value; and we have again to complain of the carelessness with which the text is prepared. There are numerous grammatical blunders, (e. g. " the pulse *were*," in about half the number of cases,) and the style is altogether inelegant and devoid of precision.

No. II. 1. *Observations on pneumonia and its consequences*, by T. Addison, M.D. In this valuable and interesting communication the author confines his attention to certain points which appear to him to require or admit of further elucidation, passing over the general history of the disease. And, in the first place, he expresses doubts whether what is generally denominated *simple pneumonia* be in reality so uncomplicated an affection as is supposed. His own experience leads him to regard this form of the disease as a *broncho-pneumonia;* but he believes that a truly simple pneumonia is not very unfrequently met with in young patients, and in good constitutions, unattended by either cough, expectoration, or pain—at least, in such a degree as to attract particular attention. This important fact will be more fully noticed on a future occasion.

Seat of pneumonia. Our readers are doubtless aware that Dr. Addison dissents from the common opinion, that the inflammatory deposits are poured into an interstitial tissue, the very existence of which he has been unable to discover; and believes that the original and essential seat of pneumonia is in the air-cells. The question yet remains an open one, but we confess these views appear to us the most sound and satisfactory.

Effects of pneumonia. The first effect is probably a preternatural dryness of the air-cells, from an arrest of their natural secretions, which (also *probably*,) is characterized physically by an excited state of the respiratory act, and a murmur louder than natural. This stage is soon succeeded by an effusion of serum into the air-cells themselves—not into the intestinal tissue, as may be shown by subjecting a portion of lung thus affected to pressure, when the fluid will exude from the truncated bronchial tubes, and may be collected for examination. In this stage the cells are highly red and vascular, and already feel more substantial and dense than natural, though still crepitating and containing air. Sooner or later, however, the turgid parietes of the cells lose their natural cohesion, swell, and, encroaching on the cavities, cause the absorption of the serum previously effused; and thus occasion, first, a dryness and brittleness, and afterwards the complete consolidation which constitutes the red hepatization of authors, and which is accompanied by such a loss of cohesion as causes the lung readily to break down under pressure. As the inflammation proceeds, the parietes of the cells become more and more opaque and thickened; the minute blood-vessels are no longer visible; the tissues become exceedingly softened; and, with this extreme loss of cohesion and apparently diminished vascular turgescence, we have undeniable proof that the cells now admit of an albuminous matter being poured into their cavities. This constitutes the gray hepatization of authors. The degree of solidity of this effused matter will depend upon the strength of the constitution, and the sthenic form of the inflammation. These changes may occupy a large extent of the lung, or be limited to separate lobules, which latter most commonly occurs in cachectic subjects.

What is called *carnification* of the lung, and generally believed to be the result of pneumonic inflammation, modified by pleuritic effusion, Dr. Addison considers to be merely the effect of pressure sufficient to force out all the contained air; having ascertained that a portion of lung in this state, which instantly sunk in water, could be perfectly inflated, and then presented a natural and healthy appearance with the single exception of slight redness.

The above may be regarded as the more *immediate* changes produced by inflammation. The *permanent* effects, according to our author, present three varieties: 1, the uniform albuminous induration; 2, the granular induration; 3, the gray induration.

a. The uniform albuminous induration is the least frequent: it occurs occasionally as the result of acute pneumonia in good constitutions, the softened tissues appearing to become so blended with, or assimilated to, the permanent albuminous deposit, that the whole is converted ultimately into a uniform homogeneous, semi-transparent, or opaque and yellowish material, in which not the slightest trace of either the aërial cellular structure or of the common interlobular cellular membrane can be discovered. Most frequently a considerable portion of lung is affected in this way; but occasionally the change is limited to one, or a very few lobules.

b. The granular induration is caused by the effusion of a less organizable albumen. The interlobular cellular tissue often, but not necessarily, remains perfectly distinct; and a solid, pale, or yellowish and friable albuminous matter occupies the lobules, apparently without having assimilated the parietes of the cells; so that the lobules, with their still separate cells filled with this substance, have something of the configuration of a raspberry. To this change the term *inflammatory tubercle* is sometimes applied.

c. The gray induration is made up of a mixture of dull or yellowish-white and black matter, in variable proportions, the density increasing with the darkness of colour. This change differs from the uniform albuminous induration in the albuminization of the tissues, being much less complete, and from the granular matter in the deposit being of a more plastic or organizable kind. In short, the pneumonic inflammation may be said to have terminated in adhesion; the albuminous effusion having partially undergone organization and contraction, and thereby glueing together and hardening the aërial cellular tissue—the whole of the appearances being, consequently the result of permanent albuminous deposit, obstructed cells, and black pulmonary matter. The interlobular cellular membrane probably undergoes a similar change. All these morbid structures or deposits maintain their integrity by a very slender power; and, under the influence of slight disturbing or devitalizing causes, are liable to undergo softening, and thus give rise to abscesses of various characters and extent.

These morbid changes are frequently regarded as forms of tubercular infiltration, and many will therefore doubt their inflammatory origin; but that the above is the true view of their nature, Dr. Addison is convinced, for the following, among other reasons. We occasionally have direct proofs of a previous attack of acute inflammation within the chest. They are pretty uniformly accompanied by the clearest evidences of former inflammation, viz. old deposit on the pleura, immediately above the affected portion of lung; adhesions between the pleuræ in that situation; considerable puckering of the pleura, with accompanying contraction and diminution of size of the lung itself, sufficient, in some instances, to cause actual deformity of the thoracic cavity. They are more frequently than otherwise found in the middle and inferior lobes of the lung. And, lastly, there is often the total absence of a vestige of tubercle in other parts of the lung.

Into the general and important question of the inflammatory or non-inflammatory origin of tubercle, which is incidentally noticed by Dr. Addison, we have not space to enter; but we recommend to our readers the attentive consideration of his remarks, assuring them that in this, as in every other part of the paper, there is food for much and improving meditation.

The paper is illustrated by seven admirable coloured drawings.

2, (and 5,—No. III.) *Observations on lithotomy*, by Bransby B. Cooper, Esq. This paper, though somewhat carelessly written, is characterized by much sound sense and judicious surgery; and deserves a careful perusal.

3. *On the pathology of cells*, by Thomas Williams, M.B. This article is the commencement of a series of Reports of facts and observations obtained in the microscopical department of the hospital. It contains much that is interesting, though well known, and some few things that are new. On the present occasion we shall confine ourselves to the notice of certain points that have been ascertained regarding the *cells of the liver*.

The liver abounds in large, well-defined, spheroidal cells, having a distinctly biliary tinge, and a considerable quantity of granular amorphous material. They also, as was determined independently by Henle and Erasmus Wilson, present the peculiarity of having in their interior, as one of their normal constituents, adipose particles, which vary remarkably in number and size. In cases of *fatty degeneration* Mr. Bowman first pointed out that the morbid change was caused by an unnatural accumulation of these particles in the interior of the cells; and the researches reported by our author have brought to light the following additional circumstances.

No fat-globules have ever yet been detected in the interior of the *nucleus;* they are invariably situated between it and the outer or cell-membrane. As they accumulate, the nucleus completely disappears; the granular or primitive molecules undergo absorption; and with their removal the *yellow* tinge, or *bile-colour*, of the cell also disappears.

A patient died in the hospital from malignant disease of the duodenal extremity of the pancreas, which had so pressed upon the common excretory duct, that the passage of the bile into the duodenum had become almost impossible. The liver was considerably enlarged; it had become soft and flabby, and not capable of being easily broken down by pressure. On repeated examination of different portions of the organ with the microscope, scarcely a single nucleated glandular cell, in a perfect state, could be found. In each portion nothing more than minute, *free*, fatty particles, and equally free, floating, amorphous, granular matter could be discovered: it was very seldom that a *whole* nucleated cell could be seen. It would seem, therefore, that the pressure of the constantly accumulating bile had caused rupture of the cell-membranes.

In *fever* the adipose globules almost completely disappear from the cells of the liver.

In cases of *granular liver* the cells of the affected parts were found transparent and *colourless*, with few granules, but still containing adipose particles.

We shall be glad to meet with Dr. Williams again, but will venture to give him a gentle hint, (and it is meant in kindness,) that the value of his communications would be considerably enhanced by the adoption of a more *simple* style of writing; grandiloquence is altogether out of place in a professedly scientific paper.

4. Mr. Williams relates a successful case of *extirpation of the superior maxillary bone.*

5. *On a feculent discharge at the umbilicus, from communication with the diverticulum ilei*, by T. Wilkinson King, Esq. The author is of opinion that the diverticulum ilei is the remains of the canal which, in the earlier periods of fetal life, forms a communication between the umbilical vesicle and the parts within the abdomen; and which is described by Michel and others as the starting-point for the development of the bowels, both upwards and downwards. When the opening at the umbilicus remains patent, the object to be aimed at in treatment is to secure its closure, and to avoid cicatrization between the skin and mucous tube. This was effected, in the case before us, by making an incision of an ovoid shape round the orifice, and bringing the cut parts into apposition; caustic having been previously tried with only partial success.

6. *On the extirpation of ovarian cysts*, by Mr. C. Aston Key. Mr. Key advocates the long incision; but the only case in which he has operated, and the details of which are given in this place, was unsuccessful.

7. *Case of extra-uterine fetation*, by Mr. B. C. B. Rose, with a description of the parts, by Dr. Oldham.

8. *Cases of puerperal convulsions*, by Dr. S. C. W. Lever. The most interesting circumstance noted in this paper is the fact that, in nine out of ten cases, in which the condition of the urine was examined, it was found to contain *albumen* in variable proportions. The water, in every

instance, was carefully drawn off by the catheter, so as to avoid mixture with the vaginal discharges. Dr. Lever has also examined the urine, obtained in the same way, from upwards of fifty women during labour, and with the following result: *in* NO *cases was albumen detected, except in those in which there were convulsions, or in which symptoms presented themselves which are readily recognized as the precursors of puerperal fits.*

Cases of convulsions thus complicated are divisible into two forms. In the one the urine is *albuminous during pregnancy;* and there are external evidences, as shown in œdema of the face, eyelids, hands, &c. In these cases the convulsions are more violent, and last for a longer time after delivery than in the other variety. The urine also retains its albuminous properties longer. In the second form the urine only becomes albuminous *during the labour;* the abnormal ingredient is less in quantity, and speedily disappears, and the fits seldom recur after delivery. Dr. Lever believes that the *causa mali* is the pressure of the gravid uterus upon the emulgent veins. The treatment advocated is, active depletion, when the patient will bear it, the free use of tartar-emetic, purgation, and as speedy a delivery as can be safely effected. He only gives mercury as a purgative; and remarks that its exhibition, even in this way, is not unattended with risk.

9, (and 6,—No. III.) *Report of cases of stricture of the urethra, retention, and extravasation,* treated from October, 1841, to October, 1842. We recommend this instructive record to all our surgical readers, and more especially to such of them as may be inclined to advocate the *heroic* practice of catheterism. The appended plate is full of solemn warning.

No. III. 1. *Cases of pelvic inflammation, with abscess, occurring after delivery,* by Dr. Lever. We have only room for a short abstract of the remarks made by the author on this affection, which has not received as much notice from obstetric writers as its importance demands. From the cases narrated, it would seem that the *seat* of the disease is the uterine appendages and the cellular tissue of the pelvis, but in which of these it originates does not appear so clearly. It may follow an attack of acute inflammation; or remain as the sequent of puerperal fever; or be caused by cold, falls, blows, &c. In only two of Dr. Lever's cases were the labours unnatural. The *symptoms* may commence a day or two after delivery, or a much longer period; some weeks even may intervene. The attack is generally preceded by rigors, or a sensation of coldness over the surface, followed by heat of skin, quickened circulation, and pain in the region of the pelvis. The febrile paroxysms are sometimes irregularly remittent. As the disease advances the pelvic pain increases; there is usually some degree of stiffness in the side affected, and, not unfrequently, pain in the course of the vessels of the thigh and leg: this may proceed to the development of phlegmasia dolens. The pulse is seldom below 100 or 110; the tongue loaded; urine scanty and high coloured, and passed at short intervals; bowels constipated at one time, and relaxed, with tenesmus, at another. In some cases a swelling is readily seen at the seat of the affection; in others there is merely an appearance of fulness on one or both sides. The parts are very sensitive, and feel hard. This hardness may extend as high as the umbilicus, or be confined to the pelvis. On examining *per vaginam*, in some cases, nothing abnormal may be detected, beyond increased size of the uterus. But in others the upper part and side of the vagina will be found hard, tender, firm, and inelastic; and by pressing upon the swelling felt through the abdominal parietes with one hand, and keeping the forefinger of the other in the canal, it will be found that the hardness and swelling in both situations arise from the same cause. Frequently there is some lateral displacement of the uterus. In some cases the swelling may be felt, *per rectum*, encroaching upon the bowels. The disease may terminate by *resolution* or, what is unfortunately more common, by *suppuration;* in which case the abscesses have been found to evacuate themselves, 1, externally; 2, into the cavity of the peritoneum; 3, into the vagina—the most frequent event; 4, into the uterus; 5, into the bladder; 6, into the intestines; 7, into the surrounding cellular tissue. The *sequelæ* may be, immobility of the uterus, leading to abortions in any subsequent gestations; an impervious condition of the Fallopian tube; and, probably, ovarian disease.

In the *treatment* heroic measures are neither required nor advisable, as the patients are generally those whose constitutional powers are much depressed. Blood should be taken by leeches as often as may be found necessary, encouraging the bleeding by warm sedative cataplasms, and vaginal injections of a similar nature, as the decoct. conii. Mild mercurials should be cautiously exhibited, to affect the system slightly; and the secretions carefully attended to, The distressing tenesmus may be relieved by the enema amyli, with syrup. papaveris, or tinct. opii, or by opiate suppositories. When it appears evident that suppuration is taking place, that process should be hastened by medicated poultices and fomentations; and, as soon as practicable, the abscess should be opened. M. Martin recommends the application of caustic potass to the abdominal parietes, for the double purpose of having an external opening, and securing previous adhesion of the containing sac to the abdominal walls. At the same time, the patient's strength must be supported by generous diet. If nursing appears to do injury, the child should be at once removed.

2. *Cases of poisoning*, by Mr. A. S. Taylor. The first case is that of a man who voluntarily swallowed about two drachms of corrosive sublimate. He presented the usual symptoms, but with some peculiarities. Thus, salivation began to make its appearance in about four hours after the poison had been taken, which is an unusually early period. He had also complete suppression of urine during the whole period that he survived—upwards of three days. The post-mortem appearances were also in some degree remarkable. Though life was so long protracted, there were no marks of corrosion, ulceration, or any tendency to perforation of the stomach, which was simply inflamed, very much as it appears in cases of arsenical poisoning. The slate-gray colour which the mucous membrane sometimes presents was entirely wanting. The duodenum and jejunum had entirely escaped the action of the poison, the chief effect produced on the small intestines being seen on the lower part of the ileum. The patient was subjected to most active antidotal treatment, but, as the

above shows, without effect. *No trace of the poison could be detected upon the most careful chemical analysis.* The importance of this fact, in its bearing upon legal medicine, is self-apparent. It may be well to state, that, according to Mr. Taylor's experience, the most delicate test of corrosive sublimate is a coil of gold and zinc suspended for some hours in the suspected liquid, previously slightly acidulated with muriatic acid. If the poison be present, the gold will become tarnished, and from it metallic mercury may be obtained by heating in a reduction-tube. In this way Mr. Taylor has detected the $\frac{1}{144}$ of a grain, dissolved in one fluid drachm; that is, in more than eight thousand times its weight of water. About twenty-four hours is necessary for the production of the change when the quantity is so small.

The second case is an exceedingly instructive example of the dangerous effects of opium in young subjects. Death in this instance was caused by the administration of paregoric elixir to a girl aged five and half years, who was labouring under slight indisposition from a cough. We are unable to condense the reporter's valuable remarks, but would strongly recommend them to our readers.

3. *On paracentesis thoracis*, by Dr. Hughes and Mr. Cock. In the experience of these gentlemen, this operation has proved much more successful than other observers have found it. A table at the end of the paper gives an abstract of twenty cases of empyema, chronic pleuritic effusion, pneumothorax, &c. occurring in the practice of several gentlemen, and of which some records have been preserved. From this it appears that in ten the operation was followed immediately by great relief; in six by relief; in three there were no immediate effects; and in one the patient appeared in danger of suffocation, but ultimately recovered perfectly. Of the whole number of cases, seven were completely cured, and three recovered partially. Nine of the patients died; but of these six were phthisical, and no permanent good was anticipated from the operation; in one the fluid was not reached by the trochar; another was already sinking, under the complication of pneumonia and pericarditis, when the chest was opened; and in the remaining case, in which there was hydrothorax of the other side, death took place, rather suddenly, six days after the operation. One case still remained under treatment when the table was drawn up. In no instance could the fatal event be said to have been hastened by the operation.

We have not space to enter into the diagnosis of fluid in the cavity of the pleura, which is fully discussed in the paper before us, but must say a few words upon the manner in which they recommend the operation to be performed, and which, in some measure, differs from the more common method. The best spot for the puncture is below the angle of the scapula, between either the seventh and eighth or the eighth and ninth ribs, and at a point distant from one to three inches from the angle of the bone. In this situation there is least danger of wounding the diaphragm, and here we are most likely to procure a free discharge of the effusion. But, in many cases, it is advisable to make an exploratory attempt before proceeding to open the chest; and for this purpose a contrivance invented by Dr. Babington is admirably adapted. It consists of a needle contained in the smallest-sized canula; this is passed between the ribs into the suspected spot; the needle is then withdrawn, and the escape of fluid from the tube at once indicates the existence and the nature of the abnormal secretion. This instrument appears to us to possess many advantages over the ordinary grooved needle. Having thus determined upon the propriety of further proceedings, the trochar and canula are to be introduced in the same spot. Mr. Cock uses a circular instrument, which is about $\frac{1}{12}$ inch in diameter, and about two inches in length, exclusive of the handle. This is less than the usual size, but it has the advantage of being more easily introduced, of giving very slight pain, of ensuring a slow and gradual evacuation of the fluid, and of enabling us to avoid the admission of air; all which, but especially the last two, are matters of great importance. The patient should sit *across* the bed, so that his body may be readily lowered and supported over its edge. The introduction of the trochar will be much facilitated by making a small puncture in the skin with a narrow-bladed lancet. When the trochar is first withdrawn the fluid will issue in a steady and equable stream, but after a time the flow will be checked at each inspiration, and then the body of the patient should be gently lowered into a horizontal posture, and turned slightly to the affected side, so as to bring the cavity directly over the opening. Should the stream again begin to flag, steady pressure may be made on the lower part of the chest, by an assistant grasping it on either side with the hand; but as soon as these means fail in procuring a continuous flow the canula should be immediately withdrawn, while the chest is still compressed, that so the entrance of air may be prevented. The occurrence of this accident is known at once by the production of a peculiar sucking noise; and at whatever stage that sound may be heard, it should be the signal for the prompt removal of the instrument. The wound requires nothing but the application of a small dossil of lint and a strip of plaster.

4. *On polypus uteri, and its coexistence with pregnancy*, by Dr. Oldham. Uterine polypi are of various kinds. The most common is the *fibrous*. Such tumours may originate in any portion of the uterus; but they usually spring from some part of the body, its sides, or fundus. They consist of a fibrous growth, with more or less of uterine structure, (the latter being most abundant in the stalk,) covered by the mucous membrane of the womb. The anatomical elements are a clear, unstriped fibre, closely packed, interspersed in some instances with crystalline calcareous grains, and minutely divided arteries. The veins, though closely collected around the growth, do not appear to enter it. When these polypi descend, the walls of the uterus, to which they are attached, sometimes become inverted, and the depressed portion may be mistaken for the pedicle and tied, a circumstance full of danger. The uterine sound, as recommended by Dr. Simpson, of Edinburgh, will afford valuable aid in the diagnosis of such cases. Dr. Oldham relates a very remarkable case, which occurred lately to Dr. Rigby, in which the entire uterus was thus inverted, and removed by ligature, *without injury to the patient.*

Another variety of polypus is that which is called *spongy*, *cellular*, or *fibro-cellular*. The latter term correctly expresses its appearance when bisected; but, according to our author, gives an erroneous idea of its true nature; the void spaces or cells being, in most instances, truncated and divided veins, For in this species, the veins are not only collected in large trunks around the growth, but penetrate the centre, and are distributed through it in large channels, having a free communication with each other. In this situation their coats are supported by an unstriped fibre, closely resembling, if not identical with, the muscular tissue of the uterus.

Dr. Lee has described a form of polypus which is strictly *cellular.* Dr. Oldham has met with the same, and believes that it is formed by an expansion of the uterine glands.

The crypts of the cervix uteri furnish, in their enlargement, another variety; and the author has met with two specimens, situated in the same locality, which he designates the *channeled polypus of the cervix*, from the fact that its interior is made up of numerous large channels, with occasional communications between them, and opening, by large orifices, on the free surface of the growth. The channels are filled with transparent, sticky mucus.

Dr. Oldham believes that the hemorrhages in polypi are from the tumours themselves, and principally from the veins on their surface or pedicle; that sometimes the veins are lacerated, and that at others they open, under the accumulation of blood in them; as they do during menstruation.

The remainder of the paper is occupied by a narration of cases illustrating the coexistence of polypus with pregnancy, in reference to which we find the following summary:

"1. That various sized polypi, and some very large, may be developed in the uterus during pregnancy, without at all interfering with gestation, or in any way impeding parturition.

"2. That they are to be suspected when the uterus remains larger than usual after parturition, although hard and contracted, and when ineffective uterine efforts continue, attended by hemorrhage; and that they are liable to be mistaken for a secondary fœtus.

"3. That the distinction between a polypus and a second fœtus ought to be carefully made out before any attempts to deliver are begun; as, by confounding them, a polypus has been perforated, and the uterus rent and injured both by the hand and the forceps.

"4. That the hemorrhage they occasion is sometimes slight; at others, sudden and overwhelming: sometimes it occurs immediately after the birth of the child, or placenta; at others, it is delayed for a fortnight or three weeks; but it is frequently a harassing daily loss, which defies ordinary remedies to arrest it, and usually occurs in gushes.

"5. That the uterus may propel the polypus before the child's head; or, after delivery, it may close over the growth, and remain quiescent; or paroxysms of uterine pain may continue, with bleeding, or constant unremitting action of the womb, causing severe distress, exhaustion, and death.

"6. That the uterine action may occasion, in some instances, the separation of the growth; in others, the partial or complete inversion of the womb. If the polypi be attached to the cervix, or immediately above it, it may propel it into the vagina, separating perhaps some cellular adhesions; or, lastly, that its constant forcings with the weight of the polypus, may prolapse the uterus, and force the polypus beyond the external parts.

"7. That the treatment must depend on the prominent symptoms, and on the practicability of applying mechanical means: that if no hemorrhage be present, or it be but very slight, every endeavour, by opiates and rest, should be made to quiet the womb, until the shock of parturition, the uterine circulation, and that of the polypus, have diminished; that, if the hemorrhage be violent or continuous, and intractable, or if the incessant contractions of the womb threaten the life of the patient, the polypus is to be removed.

"8. That a polypus may be encircled by a ligature, or after that, be excised, or primarily twisted off, both during, immediately after, or at any time succeeding parturition, without the necessary production of bad symptoms; but that the ligature alone, or ligature and excision, are the preferable plans of operating.

"9. That a polypus thus removed is not regenerated; nor does it necessarily cause sterility, or occasion untoward symptoms in succeeding labours."

7. *Report of cases illustrating diseases of the brain and nervous system.* This paper is interesting, but unfitted for analysis.

Art. VI.

1. *Om den spedalske Sygdom (Elephantiasis Græcorum.)* Af Berglaege C. W. Boeck.

On the Elephantiasis Græcorum, or Spedalskhed of Norway, &c. By C. W. Boeck, Physician to the Mines at Kongsberg.

2. *Iagttagelser om Spedalske i St. Jörgens Hospital i Bergen i* 1841. Ved Corpslaege D. C. Danielsen i Bergen.

Observations on the Patients affected with Lepra in St. George's Hospital in Bergen. By Dr. Danielsen. (*Both Essays are published in the 'Norsk Magazin for Laegevidenskaben.'—Christiania,* 1842.)

The Norwegian government have now for some years directed the attention of the medical profession in that country to a thorough investigation of the diseases which appear to be almost peculiar to the high northern regions. Not contented with the consideration of these disorders at home, several enterprising and public-spirited individuals have undertaken long and arduous journeys to the different parts of Europe where maladies analogous to those of Norway are reported to prevail. The result of one of these scientific expeditions—for such we may truly call them—is here presented in the report of Dr. Boeck, and the admirable essay by Dr. Danielsen, Physician to St. George's Hospital at Bergen, is another proof of the interest that is now excited in Norway regarding these two diseases, the radesyge and the spedalskhed or leprosy. Nor is the study of these maladies—though they are now fortunately confined to a very few isolated spots in Europe—without interest to the practitioner in more favoured lands. In a historical point of view, they are of the utmost importance, as there seems now no doubt of the fact, that the ancient leprosy, which formerly prevailed to so fearful an extent in Britain, is still to be found under the name of the spedalskhed, liktraa, and radesyge in the remote regions of Iceland and of Norway. In a former volume of this Review (April 1842,) we gave a detailed analysis of Dr. Hjort's description of the last-named malady. Dr. Hjort was sent, in 1838, to investigate the disorders analogous to radesyge in the southern parts of Europe; and in like manner his talented colleague, Dr. Boeck, has now recorded his researches concerning the prevalence of the spedalskhed on the western coast of Norway, and his experience of the same disease, as it exists on the shores of the Mediterranean and in the Grecian Archipelago. Dr. Danielsen's paper is not an essay upon the disease, but merely professes to be a report of the patients treated in St. George's Hospital at Bergen during the year 1841. The number of these patients amounted to 155, of which 79 were females and 76 were males. The deaths during the year were 16 in number; and 13 post-mortem examinations were made, which last, we believe, are the first records of the state of the internal organs in this disease.

Both Dr. Danielsen and Dr. Boeck admit two varieties of elephantiasis Græcorum, viz. elephantiasis anaisthetos (*αναισθητος*,) or glabra,* and elephantiasis tuberculosa. We shall endeavour to lay before the reader a succint history of both these forms of spedalskhed, as they are described in the two works at the head of this article.

First form. *Elephantiasis anaisthetos seu glabra.* This form of the malady is occasionally preceded by a certain train of symptoms, such as anorexia, general debility, and a tendency to sleep; or occasionally the patients experience a sensation of intense cold, which may pervade the whole body or be confined merely to a single member. But of all indications of the speedy invasion of the disease, Dr. Boeck regards the appearance of *pemphigus*, chiefly upon the elbows or knees, as one of the most certain. The patients so affected generally affirm that they went to bed in good health, and in the morning were surprised to find one or more large vesicles, filled with a clear fluid, upon the above-mentioned parts. After the lapse of a few hours, these vesicles burst, and left a superficial ulcer, which healed in a few weeks, forming a shining, white, circular cicatrix. This vesicular eruption may recur several times during the course of the malady, and in some instances it has not appeared till long after the disease had become fully developed in the constitution. In some cases—but by no means in the majority—an increase of sensibility of the skin has been observed before the opposite state, which has given the distinctive appellation to the disorder, had supervened.

Sooner or later after the above premonitory symptoms, the skin becomes sallow, and often almost of a violet hue; the integument is dry and loose, and its sensibility greatly diminishes, until, in the fully-developed form, it is entirely lost in the extremities, and also upon the face. In one instance, Dr. Boeck found the whole surface of the body equally destitute of feeling. Animal life may, indeed, be said to be extinguished in the affected portions; the limbs may be amputated without the least pain to the patient, who often is not aware that the operation has commenced, till he hears the grating of the saw upon the bone; nor is it rare that these unfortunate individuals, who constantly complain of intense cold, when standing at the heated stoves with their hands behind them, are severely burned in the fingers or legs, without being themselves aware of the injury, till the odour of the burned flesh reaches their nostrils, or till they are apprized of it by their companions.

In this form of elephantiasis the functions of the skin are nearly annihilated, perspiration never is observed, and the integument becomes like half-tanned leather to the touch. But, in some instances, a viscous fluid exudes from the skin, and, concreting, gives rise to vesicles and pustules in the neighbourhood of the parts affected. In such cases the epidermis becomes thickened, and subsequently the entire skin, which then becomes exceedingly prone to a peculiar form of ulceration. A pale, fluctuating, painless spot or point appears in the integument, generally in the soles of the feet; the epidermis splits, and an ichorous fluid is poured out. The ulcer thus formed soon penetrates and undermines the skin, destroying the subcutaneous cellular tissue, and laying the muscles bare; the edges of the sore are sharp and hard, the surface red and dry, with an occasional secretion of an opaque viscous fluid. It is almost impossible to excite suppuration on this surface. Dr. Danielsen has filled the ulcer for many weeks in succession with powdered cantharides, and always failed in producing the slightest suppuration. At times the whole of the soft parts are destroyed, the ligaments and bones are laid bare, and the latter may be destroyed by necrosis, and then thrown off. The tendons of the feet may be thus attacked, the ligaments of the joints bored through, till the joint itself is dislocated, and hangs useless within the integument, while the ulcerated and ruptured tendons protrude through the open sore. Where this destructive process is rapid it is accompanied by great general reaction, but it is more usually chronic in its advances: the patient suffers for a long time previously from pain in the legs, and especially at night, depriving him of sleep; but these nocturnal pains usually cease as soon as the ulcer shows itself upon the surface. Dr. Boeck lays more stress upon these nocturnal pains, and connects them with insidious inflammation and ulceration of the joint, and he also regards the ulcerative process as of a more active kind than Dr. Danielsen represents. But perhaps Dr. Boeck has principally examined those cases where necrosis of the fingers, toes, and metacarpal bones occur with much pain and swelling, and with great general reaction of the system. In such cases the parts about to be affected become hard, red, swollen, and very painful; the tumefaction extends up the limbs towards the groin or the axilla; the glands in these parts inflame, and the whole progress of the malady is marked by much fever, vomiting, headach, and a small and frequent pulse. The original tumour becomes gradually paler, and a fluctuating point is observed in it, which speedily bursts, and a quantity of bloody ichorous fluid is discharged, along with dead cellular tissue. On exploring the ulcer, the necrosed bone is discovered beneath. The general symptoms now decline, the swelling diminishes, and after some time the whole phalanx is thrown off, and the ulcer heals. This process may be frequently repeated in the same subject, until all the fingers or toes are destroyed, and even the metacarpal and metatarsal bones are occasionally lost in this manner.

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* The appellation "glabra" is preferred by Dr. Boeck, as he says that the insensibility of the skin, which has given rise to the name of anaisthetos, is sometimes wanting in this form of elephantiasis.

A species of paralysis of the muscles of the face is noticed by all writers upon the spedalskhed. It produces a peculiarly idiotic cast of countenance; as the orbicularis oris and palpebrarum no longer perform their office, a constant stream of tears furrows the cheeks, while a similar flow of saliva takes place from the dependent corners of the mouth. Ectropion of the lower eyelid frequently occurs, and successive attacks of inflammation, from insufficient covering of the eyeball, at length entirely destroy the functions of that organ.

Contraction of the fingers is also not uncommon, and Dr. Boeck has never seen this annoying symptom yield to any application.

Few of the patients that entered St. George's Hospital in Bergen in 1841 were free from other skin diseases. Many suffered from prurigo, lichen, and eczema at one and the same time; so that the skin of individuals affected with elephantiasis would seem to be a perfect garden for other diseases of the integument. Elephantiasis glabra does not cause the loss of hair from the surface of the body, as is constantly the case in El. tuberculosa; nor does it appear to attack much the internal organs. The uvula is occasionally slightly ulcerated, while the general health of the patient may not be much disturbed, and the secretions, the menstruation, &c. may proceed in their regular course.

The medium age at which El. glabra has been observed to commence is from twenty-three to twenty-five; but from Dr. Boeck's tables we learn, that that malady may commence in the tenderest years, or may not be developed till the patient is in the decline of life. Of the 42 cases observed by Dr. Boeck, 22 were males and 20 females. He assures us that the malady on the coasts of the Mediterranean is identical with that which he has found much more frequent in Norway. In the former localities this form of elephantiasis occurred only in one tenth of the patients; in Norway one third were thus affected.

SECOND FORM. *Elephantiasis tuberculosa.* This form of the disease is frequently preceded by a sensation of cold, either general over the whole body, or confined to the part about to become the seat of the malady. Along with this we usually find anorexia, and occasionally an acute febrile paroxysm occurs, accompanied with swelling and redness of the limbs, which last is of so peculiar a character as to indicate to practised eyes the near approach of the disease; but more generally elephantiasis tuberculosa is developed in a gradual manner. The eyelashes, eyebrows, and beard fall off, and sometimes also the hair upon the scalp; but the chief and characteristic symptom are the bluish-red elevations or tubercles, which form in the skin and over all parts of the body, thickening it, and giving to the patient a most repulsive and scarcely human appearance. These tubercles generally appear first upon the face;* the skin of the affected portions soon alters in colour, becoming of a dark bronzed hue, and at the same time it becomes shining, as though it were smeared with oil. If the disease continues to make progress, the tubercles sooner or later soften, and form superficial ulcerations, or else they excrete a thick viscous matter, which hardens into crusts upon the surrounding skin.

After some time the eyes also suffer from the malady. Small elevations, closely resembling the tubercles above described upon the skin, form upon the conjunctiva, and an insidious form of iritis goes on within the eyeball, which often causes the total loss of the sight. The tubercles which form on the conjunctiva commence, without any pain, as a minute yellowish-brown mass, which is surrounded by a small network of blood-vessels. These tubercles are not more than two or three lines in diameter at first, and their general position is on the outside of the cornea. Along with these we find frequently an opacity and thickening of the

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* In Rayer's Plates of Diseases of the Skin, there is a drawing of a case of this kind, which exactly resembles one that we saw in Norway, under Dr. Boeck's care at Kongsberg, in 1836.

conjunctiva corneæ, which in general obscures only the superior half of the pupil, but may extend over the whole, and totally obstruct vision.

The mucous membrane of the nose swells and ulcerates; at first, the sores are merely superficial, but they often subsequently penetrate the septum narium, and this sometimes is destroyed, and the point of the nose falls down. The hard crusts that form in the nasal cavity from the secretion of these ulcers, is complained of by the patients, as one of the most tormenting symptoms of their disease.

Tubercles also occur in the larynx, but they are seldom of large size, though, from their number, they may seriously interfere with respiration, and the voice is always hoarse, and sometimes almost inaudible. The tonsils, the uvula, and the epiglottis are sometimes nearly destroyed by the ulceration succeeding upon the development of tubercles within their substance. Ulcers in this situation have sharp edges, of a pale red, and the surface of the sore is smooth, and secretes a thick yellowish white fluid.

Occasionally tubercles are discovered in the lungs of patients on dissection; but they are almost always the common pulmonary tubercle, an inorganic, granular, hard, grayish mass, very distinct from the tubercles of elephantiasis, which are fatty and yellowish, tend constantly to ulceration, and bear undoubted marks of being organized products. But though thus rarely seen in the parenchyma of the lungs, they are frequent in the pericardium and in the pleura, and present there characters identical with those found beneath the skin. The abdominal organs may also contain true leprous tubercles, but it is singular how little the appetite or the general health is impaired in such cases, though febrile reaction sometimes is seen at the commencement of the disorder.

So widely different in external character, are the two forms of elephantiasis we have now described, that we should be inclined to consider them as separate maladies, were not both well known to arise from the same causes; which in one individual of a family may give rise to elephantiasis glabra, and in another, under exactly similar circumstances, may occasion the development of the tubercular species. The one, too, often passes into the other, and it is chiefly the elephantiasis tuberculosa that undergoes this transformation. The patient becomes feverish, he complains of a burning sensation in his skin, which becomes red and swollen, and the limbs are peculiarly tender and sensible to the slightest touch. These pains gradually cease, the tubercles disappear, the skin becomes smooth and of a pale yellow hue, and the sensibility of the affected parts diminishes, until all feeling in them is entirely lost.

The spedalskhed is undoubtedly a skin disease in its most perfect form.* The integuments in the tubercular species are totally altered in structure; the skin loses its lamellar structure from the deposition of tubercle or tuberculous matter between the layers, and the whole becomes a homogeneous mass with a few interspersed fibres. Perspiration can hardly be said to exist, while in its place there exudes a thin viscous fluid, which concretes upon exposure to the atmosphere. Nor do the

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[*] Dr. Boeck doubts this assertion, and suggests that the disease cannot be looked upon as one purely of the integument. No doubt many maladies, at present classed as simple skin diseases, will be afterwards proved to arise from an altered condition (dyscrasis) of the blood.

nerves and arteries ramifying in the cellular tissue escape uninjured; the tubercular matter is at first deposited merely upon their circumference, but subsequently it penetrates them entirely, and totally changes their structure; they become homogeneous cords; the nerves no longer contain the cells so characteristic of their nature, and the smaller arterial or venous trunks are totally obliterated, while the muscles usually remain uninjured.

The tubercular form of elephantiasis may occur in any age from infancy to sixty or sixty-five, but it most frequently makes its appearance during the period of manhood.

As to the proportion of the sexes affected by this disease, all agree that it more frequently occurs in males than in females. Of the 90 Norwegian patients whose cases are detailed by Dr. Boeck, there were 47 men. Of the 21 cases seen by him in Italy, 16 were males; and of the 160 observed in Greece, there were only 52 females. The malady everywhere affects those *dwelling on the sea coast.* All the Norwegian male patients were fishermen and sailors, as is universally the case in the Bergen district; and the disorder in Norway, Greece, Italy and France was entirely confined to the lower classes of society. The only difference observed by Dr. Boeck, between the Norwegian disorder, and the form under which it appears in more southern climates, was that the parts of generation were more frequently affected by ulcers and tubercles in the latter, while in Norway, eczema impetiginoides was a very frequent complication.

That the disorder, under both forms, is hereditary, is, we think, amply proved by the elaborate statistical tables of Dr. Boeck; while, like other maladies which are thus transmitted, it appears frequently to pass over a generation or two, and then to recur with increased violence. But the question of its being propagated by contagion is still undetermined. In Bergenstift, the spedalskhed is not looked upon as contagious; the healthy and the afflicted frequently inhabit the same chamber; the married continue their relations, and seek no separation of bed or board, should either party become the subject of the disease; while in the northern parts of Norway, the wretched leper is thrust forth, as of old, from society, and is compelled to drag out a miserable existence in his solitary hut. In Greece, according to Dr. Boeck, the doctrine of contagion is fully received, both by the people and by medical men; nay, even a German physician of eminence, Dr. Roeser, refused to touch Dr. Boeck's hand when he returned from visiting the lepers near Athens, saying that he had practised too long in Greece to remain in doubt of the contagious nature of the disease.

As far as has been hitherto ascertained, the entire number of persons afflicted with the spedalskhed in Norway, amounts to 659; the disease prevails in 1 individual in 511 in Bergenstift; in 1 in 947 in Norland and Finmark, and in 1 in 2505 in Greece. These proportions certainly favour not a little the doctrine of contagion, and the necessity of separating the afflicted from healthy individuals.

It has been maintained that a diet of half-rotten fish and of the oily bodies of sea-fowl is the chief cause of the disease. But the inhabitants of the sea coast in Norway live chiefly upon fish, *either fresh or well salted,* and rarely eat sea-fowl; and when they do so the skin and its accompanying layers of fat are generally removed. Besides, many suffer from the disease, who rarely eat fish or sea-fowl, but who live throughout the year upon flesh-meat, and bread or puddings. The patients themselves invariably ascribe their malady to the daily cold they suffer from wading in the rivers, and among the melting snow in spring, and to the being obliged often to sleep in their wet and frozen clothes. Dr. Boeck thinks that the exhaustion and cold to which the fishermen on the coast are so exposed, contribute greatly to the prevalence of disease in these localities; but all agree that its development is greatly favoured by a damp atmosphere, ill-constructed and ill-ventilated dwellings, the abuse of ardent spirits, and by the filthy habits and total neglect of all personal cleanliness which prevail among the lower classes in Norway.

In France, elephantiasis is to be found upon the southern coast in the neighbourhood of Marseilles, especially in the wretched villages of Vitrol, Rognac, Baer, and Maitigue; but as the malady is there looked upon as a special infliction of Providence, the sufferers carefully conceal themselves from the public view, so much so, that in Vitrol, where the greatest number are to be found, Dr. Boeck could not obtain sight of a single case.

In Italy the disease prevails under the name of "la lebbra," on the coast of Piedmont, and especially in Varaze, a small town near Genoa; but, according to Dr. Granetti, the whole number of the affected does not exceed sixty. Probably the commission just established by the Sardinian government to investigate the disease, will develop a much greater extension of it. On the eastern coast of Italy, elephantiasis also exists, and is there called "male del fegato." In Greece it is known by the name of lova, and is entirely confined to the islands, or to those who dwell on the sea coasts of the continent: the number of sufferers is computed by Dr. Bouros at 160.

Treatment. Dr. Boeck's stay in most places that he visited was too short to enable him to follow out the results of any particular plan; we therefore confine ourselves chiefly to the experience of Dr. Danielsen in this respect.

During the year 1841 every possible mode of treatment was tried at St. George's Hospital in Bergen, but, with the exception of two, they were all of no avail. Mercurials gave rise to vomiting and to purging, but never produced salivation, while iodine and the salts of potash were totally unavailing. The preparations of arsenic, consisting of the arseniate of copper, Scheele's green, and Fowler's solution, induced, after a short period, violent reaction in the system, especially inflammatory affections of the bowels, as enteritis and peritonitis, in the presence of which inflammations the tubercles evidently diminished in size, and the thickened skin returned to its normal density; but as soon as the inflammation had subsided the tubercles again increased, and the patient's condition became much worse than before, from the anorexia, vomiting, debility, and flying pains in the bowels, which tormented the sufferers for a long time after. Dr. Danielsen states that he has frequently observed the tubercular masses to disappear, for a time, in the way above described, but they showed themselves again as soon as the acute attack was passed, though upon some fortunate occasions the patient was actually freed from his leprosy by rapidly-succeeding attacks of acute disease. Dr. Danielsen believes in the curability of the malady in its earliest stage; but when once the tubercles have become fully developed, or the paralysed condition of the skin has appeared, then all remedies seem at the most to palliate the disorder. Of all modes of treatment, Dr. Danielsen gives the preference to venesection: it appears, he says, to check that plasticity of the blood which is so characteristic of elephantiasis tuberculosa, and not only is the tendency to tubercular formation diminished thereby, but all the symptoms that precede and accompany it are greatly ameliorated. Dr. Danielsen has found the greatest benefit from venesection, repeated for ten, twelve, or fourteen days in succession; and though the patients were, of course, by such active treatment, greatly reduced in strength, still they often earnestly demanded it to be repeated, from the great alleviation it brought to their sufferings. The blood has always presented a distinct inflammatory crust, of a greenish hue, but no chemical or microscopical investigation of it has hitherto been attempted. As to the elephantiasis glabra, Dr. Danielsen avows that every plan of treatment hitherto attempted has proved totally unavailing.

The result of these commissions of inquiry, established by the governments of the kingdoms where the disease prevails, will no doubt lead to an amelioration and thorough reform of the charitable institutions for the reception of lepers. St. George's Hospital at Bergen scarcely deserves the name: it is a receptacle for the afflicted, who seem to be under no rule or government; and the medicines, as Dr. Danielsen justly complains, are constantly thrown aside or neglected, according to the humour of the patients. It is only by the strictest sanitary regulations that we can expect this terrible disorder to be finally extirpated from every part of Europe.

Art. VII.

1. *On Ankylosis or Stiff-joint; a Practical Treatise on the Contractions and Deformities resulting from Diseases of the Joints.* By W. J. Little, m.d.—*London*, 1843. 8vo, pp. 146.

2. *Traité Pratique du Pied-bot, de la fausse Ankylose du Genou, et du Torticolis.* Par V. Duval.—*Paris*, 1843.

A Practical Treatise on Club-foot, False Anchylosis of the Knee, and Wry-neck. By V. Duval.—*Paris*, 1843. Second Edition, 8vo, pp. 542.

3. *Du Traitement des fausses Ankyloses et de la Contracture des Membres par la Compression, aidée de l'Extension, sans l'Emploi de la Tenotomie, avec quelques Reflexions sur ce dernier mode opèratoire.* Par M. Dancel.—*Paris*, 1843.

On the Treatment of False Anchylosis and Contraction of the Limbs by Compression aided with Extension, without the performance of Tenotomy, with some Reflections on the latter operation. By M. Dancel.—*Paris*, 1843. 8vo, pp. 76.

In several former Numbers of this Journal, (XVI, Oct. 1839, XXV, Jan. 1842, XXVII, July 1842,) the general principles and most of the special applications of subcutaneous tenotomy have been fully examined; but we have not yet had an opportunity of sufficiently considering the employment of that method in the treatment of anchylosis, and take advantage of the present occasion to draw attention to that most important subject.

It would be an unnecessary waste of time and space to dwell on the inefficiency of all the older methods recommended for the treatment of confirmed false anchylosis. Occasionally no doubt, when the affection existed but in a slight degree, it was alleviated or even removed, but as a general rule, confirmed anchylosis was abandoned as incurable, and when the knee was implicated, not unfrequently led to amputation of the limb. Scarcely seven years have elapsed since M. Duval first applied tenotomy to the cure of this intractable deformity, and so brilliant has been the progress of this branch of surgery within that limited period, that Dr. Little has fallen into little if any exaggeration in saying that few cases of false anchylosis are incapable of alleviation or cure. We feel it to be our duty therefore to notice the works enumerated at the head of this article, (especially those of Dr. Little, and of M. Duval,) as briefly as is consistent with clearness, for "although," to use Dr. Little's words, "the method in question has now been frequently practised in this country, and occasionally a successful case has been announced in the medical periodicals, no complete exposition of the forms of anchylosis in which it is especially useful, or the details necessary to enable the competent practitioner successfully to apply it, has been hitherto published." (p. 1.) For the sake of convenience and brevity, we shall take false angular anchylosis of the knee-joint as the type of the affection, and append any necessary observations relative to the deformity as it occurs in other joints.

Long-continued inflammation, no matter what its origin, is universally admitted to be the exciting cause of anchylosis in the vast majority of cases, but it is still a disputed point whether protracted immobility of a limb can produce anchylosis independent of an antecedent disease in the contracted joint. M. Duval, with Petit, Hunter, Boyer, &c., admits mere immobility to be a cause of anchylosis, but Dr. Little rejects "the simple *state* of *rest* or *disuse*, as a cause of true anchylosis," (p. 11, Note,) and by implication rejects it as a cause of *false* anchylosis also, as he does not enumerate it in his table of the causes of false anchylosis, (pp. 12-13;) and at p. 2, states that "inflammation is the cause of each variety and grade of anchylosis." Dr. Little supports this opinion, which has been previously advanced by Cruveilhier, Kunholtz, and other authorities, by an appeal to the fact that, in the most aggravated cases of congenital club-foot of thirty or forty years' standing, the tarsal joints, some of which have probably been motionless for many years, may be restored to their natural function, and by conjecturing that Cloquet was deceived in supposing that he had witnessed bony anchylosis in the joints of individuals who had long remained motionless in consequence of paralysis; on this subject we shall merely observe that though the two remarkable cases published by Cruveilhier and Kunholtz, in which complete immobility of the lower jaw, for the respective periods of sixty and eighty-three years, failed to cause anchylosis, come in aid of Dr. Little's argument, yet such cases merely prove that anchylosis is not a constant, probably not a frequent effect of continued rest, but they do not warrant us in setting aside the positive testimony of so competent an observer as M. Cloquet, on conjectural grounds, especially when the more recent investigations of M. Teissier (Gazette Méd. de Paris, t. ix, pp. 609-26,) go so directly to prove that a long-continued state of rest may cause vascularity of the synovial membrane, with formation of false membranes, absorption of the cartilages, and finally true anchylosis.

In the vast majority of cases, however, anchylosis, as already said, originates in some inflammatory disease of a joint; but how is it that such inflammatory affection in the first instance so frequently causes flexion of the joint, and that the flexion ultimately becomes permanent? M. Duval adopts the usual explanation that the inflammation in or about the joint, excites the irritability of the flexor muscles, and consequently the patient relaxes them by bending the knee. To this Dr. Little objects that there is no reason why the extensor muscles should not be as much irritated to preternatural action as the flexors, and he explains the preference given in most cases to the bent position of the knee: 1st, by the fact that *most* of the muscles of the extremity are then relaxed, because though some of the extensor fibres may then be rendered tense, yet as the thigh is also flexed, the rectus femoris being attached to the pelvis, is relaxed; and 2dly, by the circumstances, that the amplitude of the synovial sac being greater during flexion than extension, its fluid contents exert less pressure on the inflamed tissues. (pp. 4-5.) We believe Dr. Little's views on this subject are correct at least in part, but they have been anticipated by M. Bonnet, (Gaz. Méd. de Paris, 1841, Nos. xlvi and xlvii.) who points out some other circumstances which more constantly tend to cause flexion of the joint, the chief of which is simply the necessity experienced of keeping the limb as motionless as possible, which can only be done with facility when the limb is extended so long as the patient lies on his back, for the moment he turns even partially on the side, the entire body has an insufficient basis of support and steadiness of posture, can only be maintained without a constant and fatiguing exertion by flexing both the leg and the thigh, by assuming in fact the position which we almost constantly adopt in perfect health when lying on the side in bed. That the capacity of the synovial membrane is greatest when the joint is flexed has been proved by M. J. Guerin, and by M. Bonnet, who has, however, also shown that probably this circumstance has no influence on the position of the limb so long as the quantity of fluid contained in the joint is small: if indeed the accumulation is sufficient to produce extreme distension, flexion of the joint follows as a matter of physical necessity, for even in the dead subject if fluid is forcibly injected into the cavity of the knee-joint, the leg becomes bent to a right angle with the thigh.

But though the muscles it would appear are not the primary agents in flexing a diseased joint, they unquestionably exert a most important influence in keeping it permanently flexed after the bent position has been maintained for a certain time, and when perhaps the original disease may have completely subsided. No doubt any alteration in the shape or relative position of the articular extremities of the bones, or the effects of inflammation, such as the effusion of lymph, and the thickening of the structures while the joint was flexed, tend to keep it in that posture, but whenever the deformity has existed for any considerable period, contraction of the flexor muscles usually constitutes the chief obstacle to straightening the limb. This contraction is usually attributed merely to the continued operation of the organic contractibility of muscular fibre consequent on the permanent approximation of the fixed extremities of the relaxed muscles; but Dr. Little maintains that "after the lapse of some time, the shortening from this cause attains the maximum, and is followed by a change in the structure of the muscular fibre, which I have named structural shortening," (p. 7,) and which consists in interstitial shortening of the muscle in consequence of its molecules being renewed while it is contracted, so that "the muscular fibres become permanently shortened and inelastic; and provided a period sufficient for the complete renewal of their integrant parts has elapsed, they are necessarily redeposited shorter, to accommodate them to the altered relation of the surrounding parts." (p. 8.) This latter change, Dr. Little says, is shown by observation, as might be expected, to occur more rapidly in children than in adults, and the relative shortness of the muscles is further increased in early life, because "the bones, gradually advancing towards their full development, are elongated, the disproportion between them and the contracted muscles is augmented, and the deformity of the limb necessarily aggravated." (p. 8.) Thence it is that "in a favorable case of false anchylosis in an adult, we may succeed in effectually straightening the limb after the lapse of five years, (without tenotomy;) but it is rarely possible in a child, unless of very lax fibre, permanently to relieve by mechanical means, a severe contraction of similar duration." (p. 31.)

The diagnosis of anchylosis from other affections of the joints can rarely indeed be attended with any difficulty. Dr. Little says a mistake is impossible, and we should have been inclined to think so too, were it not for the case mentioned by Sanson (Dict. de Méd. et Chirurg. Prat., art. Ankylose,) of a child whose hip-joint, though affected with coxalgia, was supposed by several eminent practitioners to be anchylosed, the thigh being so immoveably flexed on the pelvis that when the patient was raised by the limb, the weight of the body failed to cause any motion in the hip-joint; on diverting the child's attention, however, the limb could be momentarily extended, but directly this was perceived, the thigh was again rigidly fixed in its former position. But though it is almost uniformly very easy to detect the existence of anchylosis, it is sometimes extremely difficult to determine whether the anchylosis is true or false. With respect to the diagnostic signs between those affections, Dr. Little attaches little importance to the statement that in true anchylosis the patient apprehends no pain from an examination of the joint, but that the reverse is the case in false anchylosis. A sense of pain or tightness produced on the flexed side of the limb, by attempts to straighten the joint, often indicates incomplete anchylosis. "But," in Dr. Little's opinion, "a much more delicate test is the production of pain on the opposite side," (p. 28,) though in two cases of bony union of the patella to the femur, he found this test fallacious. The condition of the muscles on attempting to straighten an anchylosed joint, is an important index of the nature of the case, for in false anchylosis, even when no motion is visible, "an alternate tension and relaxation of the muscles may be produced by forcible attempts made to straighten and bend the knee," (p. 27,) while no such tension of the muscles can be mechanically produced in true anchylosis. Dr. Little, however, warns us that "muscular tension is a symptom of anchylosis which may occasion an erroneous diagnosis," (p. 27,) as the patient while the limb is being examined, is apt to throw the muscles into voluntary action, which may be mistaken for tension consequent on the attempt to straighten the joint; and in order to avoid this source of error, "the practitioner will observe whether the muscular tension be synchronous with his manual attempts to move the joint," (p. 27,) and will take care that the patient's mind is diverted during the examination. (p. 46.) Dr. Little very candidly states, (pp. 123-4, Note,) that he fruitlessly performed tenotomy in one case before he "*discovered* the source of error in diagnosis" now adverted to. It is, however, right to say that this discovery does not appertain to Dr. Little; it was long since distinctly stated by M. Duval, who recommends us, in order to avoid being thereby misled, to place the patient on a sofa, and measure the space between the ischium and heel, first while the limb is at rest, and then when it is extended, the pelvis having been firmly fixed. If the two measurements are equal, the anchylosis is complete, if the latter is the longer, the case is one of false anchylosis.

Dr. Little and M. Duval, agree that the prognosis of anchylosis is in the vast majority of cases favorable, inasmuch as true anchylosis occurs but very rarely, and false anchylosis is almost uniformly remediable, whatever may be its duration. When the disease indeed has existed for a very long period, the treatment is both tedious and difficult, and tests both the resources of the surgeon and endurance of the patient, but still both authors agree that success more or less complete may be calculated on, as they have each effected cures where the deformity existed for periods varying from ten to twenty years, and in one instance Dr. Little achieved a cure after twenty-six years. But though so far agreed, Dr. Little and M. Duval differ very materially as to the *amount* of success obtainable at least in aggravated cases. Dr. Little teaches that the result is generally perfect, or as near thereto as may be, that not only can the limb be straightened, but the motions of the joint can in most cases be restored; but M. Duval, on the contrary, expressly and repeatedly warns us to be most guarded in promising the patient more than that the limb shall be straightened, as "most frequently when the leg is extended on the thigh, the limb remains in that state, that is, the leg cannot be flexed, the patient walks with a stiff joint," (p. 367,) and a little further he adds, "unfortunately, *six patients out of ten* have *lost for ever* the faculty of *flexing* the leg after it has been extended, although the intermediate substance has been perfectly developed between the extremities of the divided tendons. The same thing it is true occurs when a limb has been straightened by mechanical means, without preliminary subcutaneous section of the tendons. When the leg is extended it cannot be flexed, *it remains stiff for ever.*" (p. 368.) But if M. Duval differs in this respect from Dr. Little, we must add that he also differs from himself, for in another passage we find him saying, "the *vast majority* of patients on whom I have operated, already exceeding 150 in number, have been able after the complete extension of the previously flexed leg, to perform *a part or the whole of the natural motions of the knee*," (p. 371;) but in not one of the numerous cases detailed in his work, does he state whether the joint enjoyed any degree of motion after the treatment was concluded. As regards the facility of extending the limb, M. Duval states that, generally speaking, the more moveable the joint, the more favorable is the prognosis in this respect, but that the condition of the articular surfaces is a more important consideration, for if the tibia is either much rotated or displaced backwards on the femur, it may be very difficult to effect extension, though a considerable degree of motion exists in the joint, while an almost immoveable joint may be easily straightened if the relations of the articular surfaces are not disturbed. (pp. 368-9.) M. Duval says that the difficulty of cure generally increases with the age of the patient, and the duration of the disease; we have already seen that so far as the mere influence of age is concerned, Dr. Little comes to the directly opposite conclusion. As respects the *cause* of the affection, Dr. Little considers that rheumatic false anchylosis demands a very "cautious prognosis," for though there is little absolute disorganization of the structures, and still less change in the form of the knee, yet the tissues are "often soldered as it were into a mass by the exudation of plastic matters." (pp. 97-8.) Anchylosis after white swelling, on the contrary, though peculiarly prone to be accompanied with great deformity of the joint, "constitutes the most favorable form for division of tendons, the subsequent mechanical treatment being more easy, and the restoration of the limb more complete than in anchylosis from rheumatism, or from extensive suppuration from phlegmon." (p. 115.) We may here mention that though as a general rule, true anchylosis must be deemed incurable, Dr. Little entertains "no doubt of the capability, in some instances, of restoring motion in an articulation between the surfaces of which the deposit of osseous particles has already commenced," (p. 33:) this opinion is founded on two cases given at pp. 92-4, which however, in our opinion, are anything but conclusive; though we must not be understood to deny the possibility of overcoming partial bony anchylosis,—for we are, on the contrary, convinced that it may be occasionally practicable.

Dr. Little and M. Duval agree that friction, manipulation, mechanical extension, &c., are inefficient except in slight cases of anchylosis, unattended with permanent shortening of the flexor muscles, and that when such shortening exists, attempts at mechanical elongation will not only be fruitless, but liable to excite serious mischief in the joint, unless muscular resistance is previously neutralised by the performance of tenotomy. Dr. Little divides the treatment of anchylosis, "by the combined surgical and mechanical method," (p. 32,) into three stages: 1st. The surgical operation, (tenotomy.) 2. "The mechanical reduction of the anchylosis." 3. "The period employed in restoring freedom of movement by passive and active exercises, frictions, manipulations, baths, &c." (p. 32.) M. Duval seems to limit the treatment to the first two of those stages, for neither in the reports of his cases nor elsewhere, does he allude to any treatment for restoring the mobility of the joint after the limb has been straightened. This accounts for the different statements of those authors respecting the amount of benefit derivable from treatment, and also for the great difference in the length of time consumed by them respectively in the treatment. M. Duval says the average period necessary to obtain a "*complete* cure" is six weeks, and seems to fix the maximum time at three or four months, which longer period moreover he has only found necessary in five or six cases. (p. 446.)

Dr. Little, on the contrary, says " the average length of time occupied in the straightening and reduction of the displacement of the articular surfaces after the operation in adults is two months; in children the success is more rapid. The process of restoring mobility . . . . . is seldom completed in severe cases, within a period varying from three to six months," (p. 57,) and occasionally twelve months may elapse " before the patient can be considered fully restored."

Dr. Little considers tenotomy inadmissible, " prior to the complete subsidence of the primary articular disease . . . . until at least two or three years have elapsed since the subsidence of active disease," (p. 117 ;) and having in one case neglected this rule, and performed the operation at an earlier period, the strumous disease was again excited in the joint. We certainly think that to delay two or three years is to wait a period at least double what is necessary, but otherwise the practice here recommended, is we believe so entirely consonant with the views of surgeons in these countries, that we should not have adverted to this point, if it was not for the opposite doctrine inculcated by M. Duval, who, though he at first carefully avoided operating until the disease of the joint had disappeared, has been long in the habit of performing tenotomy, and straightening the limb during the sub-inflammatory period of white swelling. M. Duval appeals to theoretical considerations and clinical facts in support of this practice. He conceives that maintaining the natural position of the limb is as essential in white swelling and in all chronic inflammations in or about joints, as in fractures, (p. 434 ;) that straightening a joint must act favorably on any disease under which it may suffer by bringing the greatest possible extent of the articular surfaces into apposition, " and so preventing the contact of angles and obviating the resulting painful pressure," (p. 433 ;) that while the vicious position of the parts continues, a mechanical cause exists to keep up the disease which antagonises every therapeutic means employed, so that the local disease continues even though the constitution may be improved. (p. 435.) Influenced by such considerations, M. Duval was induced to straighten diseased knees after having divided the ham-string tendons, and states that he has always succeeded in obtaining extension in a period varying from one to two months, not merely without the occurrence of any unpleasant symptoms, but with the happy effect of favorably modifying (p. 436) and ultimately curing the disease of the articulation, (pp. 433-8,) not to mention that the occurrence of anchylosis, of partial luxation of the tibia backwards, &c. is also obviated, and that the more certainly the earlier the diseased joint is operated on. In fine, M. Duval says:

> "Supported by numerous facts, I feel warranted in maintaining the following doctrine: Pain, inflammation, alteration of parts, whether inter-capsular or extra-capsular, alteration of the integuments, inflammatory or œdematous swelling, numerous cicatrices, suppurating ulcers—all circumstances, which seem to be so many contraindications, should not arrest the operator, and are, on the contrary, indications for his interference. The prejudices which induce practitioners to remain quiescent under such circumstances must yield to facts.
>
> "Tenotomy, then, offers great and valuable resources to surgeons in the treatment of those tedious and cruel diseases which, having assumed a chronic form, often resist the most prudent treatment, even when the constitution is not impaired." (pp. 438-9.)

M. Duval does not inculcate this practice as an universal rule; he says he is aware that there are many exceptions; we do not however find any specification of the circumstances which constitute those exceptions; unless, indeed, the existence of tolerably acute inflammation. Several French surgeons have adopted M. Duval's views; for example, while penning these lines, a clinical lecture on the indications for performing Tenotomy, by M. Guersant, surgeon to the Hôpital des Enfans, at Paris, has fallen under our notice, (Gazette de Hôp. de Paris, July 4th, 1844,) in which he lays it down as a rule that "in white swelling of the knee it is almost always advantageous to practice tenotomy the moment circumstances are tolerably favorable for its performance; that is to say, when the tumour is not *extremely* painful, when the inflammatory symptoms *begin* to diminish in intensity." But though we must presume that M. Guersant speaks from experience, and though M. Duval gives several cases in which joints affected with white swelling was straightened with a favorable issue, (pp. 417-39-41,) we cannot but regard the practice as most perilous, and should require no small mass of evidence of its innocence and utility to induce us to adopt it.

M. Duval describes, at considerable length, and with more minuteness than Dr. Little, the details of the surgical and subsequent mechanical treatment of false anchylosis of the knee; we shall refer to a few points respecting which he and Dr. Little disagree, or which the latter author does not notice. M. Duval does not appear to have ever found it necessary to divide any structures save the ham-string tendons, and in one instance only the tendon of the gracilis muscle; occasionally he finds it sufficient to divide the biceps alone, especially when the anchylosis is complicated with deviation of the knee inwards; though sometimes, after the lapse of fourteen or fifteen days, when extension of the leg has made considerable progress, the tendons of the inner ham-strings become prominent and must be divided. This secondary operation we think should always be avoided if possible by dividing, in the first instance, every structure which appeared to contribute in maintaining the deformity. Dr. Little, in common we believe with most operators, has to divide occasionally many more structures than those mentioned by M. Duval; such as the interior fibres of the vastus externus muscles, bands of fascia in the ham and on the outer side of the thigh, and the tendon of the sartorius muscle. Dr. Little, of course, warns us against injuring any important nerve; such as the peroneal, which was severed along with the biceps tendon in a patient whom he subsequently had an opportunity of seeing; but he insists on the necessity of "sometimes" dividing "the more superficial of the nerves which supply the gastrocnemii, or traverse its surface." (p. 52.)

"The section of the nervous filaments in question can only be deemed essential after having divided the muscular structures and fasciæ, when the operator will sometimes discover one or more cords rendered tense, on endeavouring gently to extend the limb. If unaware of their nature, before their section he might suppose them to be fibrous bands; but the peculiar pain complained of by the patient indicates their character. An experience founded on the performance of the operation in numerous cases, and the management of the subsequent mechanical treatment, has shown me the propriety of not leaving these filaments undivided. If stretched during the after treatment, very severe pains may be experienced during weeks or even months. In short, I am convinced that much suffering is spared by the section: the filaments reunite, and recovery from the anchylosis ensues without neuralgia, and without numbness or diminished power in the muscles." (pp. 52-3.)

In dividing the ham-string tendons, M. Duval introduces the knife beneath the tendon and divides it from before backwards or towards the skin: Dr. Little gives no direction on this head, but we presume he follows Bouvier's method of insinuating the tenotome between the skin and the tendon, and cutting from the superficial towards the deep parts, which we think is decidedly the preferable mode. Our readers are aware that considerable difference of opinion and practice exists as to the period when extension should commence after tenotomy has been performed. M. Duval is an advocate for immediate extension; directly after the operation he applies an apparatus to maintain the amount of extension thereby obtained: Dr. Little, on the contrary, and in our opinion most correctly, thinks it most important not to commence the mechanical extension till the punctures are completely cicatrized, which, according to M. Duval, usually occurs in two days; according to Dr. Little, not until the third or fourth (p. 53), or rather, we are told (p. 116), the fourth or fifth day. On M. Duval's own showing, it appears, we think, that the practice of immediate extension is injudicious, as he speaks of pain in the joint requiring intermission of the treatment, &c., as a frequent occurrence. In the only instance in Dr. Little's practice in which the operation was followed by an abscess in the ham, Dr. Little is in doubt whether the suppuration was due to the extension having been commenced previous to the complete cicatrization of the punctures, or to the circumstance that the biceps tendon having been divided at two adjacent points, the portion of the tendon included between the two sections lost its vitality from being insufficiently supplied with blood, and therefore acted as a foreign body. Be this as it may, whenever a tendon requires to be divided a second time, Dr. Little recommends that the second operation should be postponed for two or three weeks in order to allow complete vascular union to be established in the part first divided. (p. 116.)

The mode of conducting extension after the division of the tendons is a matter of great importance, and must vary with the circumstances of the case. Without a very minute description, indeed without figures of the requisite machinery, it would be impossible to enter satisfactorily into details on this head, and we shall limit ourselves to a few general observations. When anchylosis is unaccompanied with displacement of the bones, a very simple apparatus, occasionally even a common splint bandaged on the posterior surface of the joint, may be sufficient to straighten the limb. But when the head of the tibia is rotated, or partially luxated backwards on the condyles of the femur, it is essential to employ an apparatus calculated to remedy such displacement; for not only would the deformity otherwise be very imperfectly removed, but Dr. Little has "ascertained from repeated experience, that when attention has been directed to the removal of the abnormal flexion only, an increased displacement of the head of the tibia sometimes resulted." (p. 541.) And in one case he "found the luxation of the bones complete, and their extremities as moveable as in a recent luxation from injury, the limb presenting to the eye of the almost frightened attendant an appearance of greater deformity and weakness than before the commencement of his well-intentioned but injudicious management." (p. 55.) In order to remedy lateral displacement of the tibia, Dr. Little attaches to his apparatus pads fixed to screws for the purpose of pressing the extremities of the tibia and of the femur in appropriate directions: and when the head of the tibia is thrown backwards the apparatus is so contrived as to constitute the pelvis as a fixed point, and make such extension from the ankle and the foot as will not only *extend* the entire limb " but elongate to such an extent the ligaments of the joint and other structures exterior to it, as will enable the tibia to rise into its position." (p. 55.) When the extremities of the bones are presumed to be sufficiently separated by the extension thus applied, pressure is so directed as to aid in placing them in apposition while the ankle is drawn inwards to remove any eversion of the foot that may exist. M. Duval's practice is in some respects very different: his apparatus is so contrived as to carefully avoid making any pressure on the knee; when the displacement of the head of the tibia is not very great he does not seem to make any attempt to remedy it; and when it is great, he still employs the ordinary extending apparatus; but when the joint has been nearly, or even completely, straightened, he places the patient for a few hours every day on an orthopedic bed, and, the trunk having been fixed, submits the leg to graduated parallel extension by means of a lace passing from the ankle over a pulley and attached to a spiral spring whose force can be regulated at pleasure. To judge both from the text of the cases recorded by Dr. Little and M. Duval and from the figures by which the cases are illustrated, the results obtained by the former practitioner are by much the more satisfactory and perfect.

When extension is completed, M. Duval makes his patients still wear the extending apparatus while walking, the foot-board being replaced by a leather gaiter; and the use of a stick, much more of crutches, is strictly prohibited, as calculated to discourage the patient from exertion and prolong weakness of the limb; after some time the apparatus is removed at night and replaced first every day, then every second day, and so on, at longer intervals, until it is finally discontinued, and then the treatment terminates. With Dr. Little, however, the third, and often the most difficult stage of the treatment now commences; the restoration, namely, of the motions of the joint, which is to be effected by passive movements, frictions, vapour-baths, and the occasional application of the extending apparatus, its action being reversed so as to flex the joint. Dr. Little permits exercise as soon as the joint is straightened, but generally the limb must be supported for weeks or even months by an iron on its outer side with a spring to assist the weak extensor muscles of the knee, and subsequently a lead knee-cap must be worn.

When in deformity resulting from disease of the hip-joint, the resistance arising from muscular contraction cannot be overcome by frictions and passive exercise of the limb, &c.; " an apparatus consisting of a circular bandage fastened to the pelvis, and sometimes to the shoulders, with a steel spring attached behind to the femur, in order to effect extension of the member, may be worn, and will, in a short time, sufficiently remove the contraction to permit the application of the entire sole to the ground." (Little, pp. 37-8.) Should this prove insufficient, dividing the contracted muscles offers the only chance of cure, but should be postponed for two or three years after the complete subsidence of the original disease, to avoid the risk of again exciting the original disease, but in the meantime the apparatus above mentioned should be worn to prevent aggravation of the deformity. The prognosis in contraction of the hip should be more guarded than in the case of other joints, because of the greater difficulty in ascertaining the precise amount of change the structures have undergone. Dr. Little gives five cases of anchylosis of the hip-joint, all of which were materially benefited by the operation except one, in which the deformity was so aggravated, and circumstances otherwise so unfavorable as to leave little to be hoped for. A brief abstract of the third case may illustrate the author's practice. A boy nine years and a half old, had the right hip contracted from the age of three, in consequence of a fall; "the part presents the ordinary appearance of false anchylosis. The knee is much drawn up towards the abdomen, and the thigh is rotated inwardly." (85.) Manipulations and frictions were employed without effect for five weeks, when Dr. Little divided the more prominent muscles; "viz., the tendons of rectus femoris, origin of pectineus, adductor longus, and part of adductor magnus." On the third day the punctures were cicatrized, and the thigh could be depressed about four inches; frictions and manipulations were resumed, and "two months after the operation he had abandoned the crutch, and walked comparatively well; the greater portion of remaining lameness depended on shortening of the limb, and some unnatural flexion, for which the hollowness of the loins compensated." (p. 86.) When, as often occurs, anchylosis of the hip is complicated with contraction of the corresponding knee and ankle, those parts will require a separate appropriate treatment.

We have said enough to convey a sufficient idea of Dr. Little's work, and of that portion of M. Duval's which treats of false anchylosis of the knee. Dr. Little also considers anchylosis of the other joints of the extremities, and of the lower jaw and vertebræ; but we shall not advert to those sections of his book further than to say that we wish he had considered the question of the applicability of tenotomy to the flexor tendons of the fingers at somewhat greater length, but we hope to deal with this subject on another occasion. Before taking leave of Dr. Little's book we must recommend it to our readers, and we trust it will contribute to awaken the attention of the profession *generally* to a class of deformities —those consequent on diseases of the joints—which long were considered incurable, and sometimes even regarded as favorable terminations of the formidable maladies in which they originated. To M. Duval, we believe, the credit is due of having first attempted to remove this opprobrium of surgery, and his present work shows how far and how well he has succeeded. Dr. Little has most successfully pursued the path thus opened and obtained results decidedly superior to those of his predecessor.

M. Dancel's pamphlet need not detain us long; the author's practice can be collected almost entirely from his title-page, which is copied at the head of this article, and the doctrine on which he founds that practice may be briefly explained. M. Dancel denies that the contracted muscles in angular anchylosis are really or organically shortened; however rigid, however permanently contracted muscles may be in consequence of any inflammatory disease of the joint, they are merely, he says, in a state of spasmodic contraction, which is a nervous affection, a spasm capable of being cured by compression and extension; tenotomy he grants will also destroy the spasm, but it is rarely necessary to recur to it; at least, the necessity of so doing arises much less frequently than is commonly supposed. M. Dancel adduces nine cases in support of his views, which we shall not analyse, as none of them really support his doctrine. Some of them, no doubt, are cases of anchylosis; of the knee, for example, after white swelling, cured by splints, bandages, &c., so applied as to effect gradual extension of the leg; but no one, not the most strenuous advocate of tenotomy, denies that this may be occasionally effected; on the contrary, where such means offer a prospect of cure, the tenotomists themselves employ them. Some others of M. Dancel's cases, and those to which he seems to attach most importance, cannot be considered cases of anchylosis; they were clearly cases of permanent muscular spasm arising from various causes, e. g. permanent muscular spasm from exposure to cold, and contraction of the biceps muscle consequent on venesection at the head of the arm. In such cases, however, we think M. Dancel's method a good one, and it is for this reason that we have noticed his work. His method is to powerfully compress the affected muscle by a roller applied round the limb so tightly that it cannot be borne for more than a few minutes; it is not indispensable to compress the entire muscles; acting on even a small portion of it will suspend the spasm: when the bandage is removed, the muscle is partially paralysed as it were, and can be completely or partly extended; splints, &c., are applied to maintain the degree of extension maintained, the greatest care being taken to compress (by bandaging) the muscle as forcibly as can be permanently borne. The mode of action of the very forcible compression first applied is easily understood; and so far M. Dancel's practice is, in fact, an extension to permanent spasm, of a method occasionally useful in intermittent spasm, or convulsions of the muscles of an extremity.

ART. VIII.

Medico-Chirurgical Transactions. Vol. XXVI.—*London,* 1843. 8vo. Three Plates.

I. *Case of paralysis*, by John Webster, M.D. F.R.S. This is an interesting paper. The patient, a gentleman ætat. thirty-six, was for many months utterly unable to move, by voluntary effort, any muscle below the neck, excepting the diaphragm, while sensation remained perfect over the entire surface of the body, and the intellectual faculties and other senses continued unimpaired until death. The progress of the case was very slow, the disease advancing gradually to its climax; and the patient suffered much at times from convulsive spasms in the lower extremities. He had also occasional epileptic fits. The cuticular secretion was entirely suspended. The muscles of deglutition were unaffected throughout. On examination after death, no very remarkable appearances were detected in the cranium. The spinal theca, corresponding to the three or four lower cervical vertebræ, was much distended; and, on being cut into, the arachnoid cavity and sub-arachnoid tissue were found filled with lymph, which united the membranes to each other and to the cord, the adhesions being most firm on the anterior aspect. At this part the medulla was much diseased. Professor Todd, of King's College, examined this portion, and reports as follows: " I find great destruction (from softening) of the medullary substance of the posterior columns, especially that of the right side; the antero-lateral columns seem to have been also the seat of the softening process to a less degree, but I do not find that they have suffered any loss of substance. In examining the softened parts by the microscope, I detected very few of the proper nerve-tubes; and those which I did observe were much altered from their natural appearance; they had become opaque, and had assumed an indistinctly fibrous aspect. I was unable to find any trace of gray matter." The posterior roots of the nerves were unaffected. The preparation, it should be stated, had been preserved some time in spirits, before this examination was made by Dr. Todd.

It is needless to say how perplexing are morbid changes like these, when taken in connexion with the symptoms manifested during life. We remember a case that was brought under our notice some few years ago, the symptoms of which were precisely the converse of the above, viz. the persistence of voluntary motion, with the almost entire loss of sensation; and yet in this instance the morbid appearances were not very dissimilar. The gray matter was altogether destroyed, the interior of the spinal cord (in the lower cervical and upper dorsal regions,) being converted into a cream-like substance. Verily there are mysteries in these things which our philosophy has not yet unravelled.

II. *Case of bronchial calculus*, by Dr. S. C. G. Tice. The symptoms in this case did not lead to any suspicion of the true nature of the disease. They were pain in the right side, increased by pressure over the liver; dyspnea, which was latterly very urgent, amounting to a sense of suffocation upon lying down; the paroxysmal cough; great fetor of breath; deep-seated dysphagia; and ultimately laryngitis. Death occurred suddenly, after an illness of six weeks. The physical signs of disease in the chest were entirely absent. On examination, there was found an abscess, of the size of a pullet's egg, in a mass of enlarged bronchial glands, at the bifurcation of the trachea. It opened into both bronchi and the œsophagus; and contained a quantity of calcareous matter, some of which was very hard, and some of a soft consistence. A hard triangular portion was firmly wedged in the aperture communicating with the right bronchus. It was found to consist of phosphate of lime.

III. *On congestive pneumonia, consequent upon surgical operations, diseases, and injuries*, by Mr. J. E. Erichsen. The form of disease noticed in this communication deserves especial attention, not less from its frequency, than from its very insidious character. Out of sixty-two cases of death after surgical operations and injuries (exclusive of burns), taken indiscriminately from the records of University College Hospital, in no less than twenty-eight there were evident signs of pneumonia, as shown either by the diseased condition being confined to one lung, by its having advanced to solidification, or by the coexistence of marks of inflammatory action in the pleuræ or bronchial mucous membrane. In eleven the lungs were in a doubtful condition, presenting the characters which are common to the first stage of pneumonia and to passive congestion, without there being any collateral circumstances by which the diagnosis could be more clearly established. In nine the lungs were found more or less diseased, but not inflamed or congested; and in fourteen only were they quite healthy. The appearances observed in the affected organs were precisely those which distinguish what is generally denominated *typhoid pneumonia;* and it is a remarkable fact with regard to the location of the disease, that in twenty-two of the cases it existed in both lungs together, though not to the same degree—the right and left lungs being each the sole seat of disease in only three cases. Our readers will at once perceive how very different is this state of things from that which prevails in the more common form.

IV. *Researches into the connexion existing between an unnatural degree of compression of the blood contained in the renal vessels, and the presence of certain abnormal matters in the urine,* by Mr. G. Robinson. This is an account of two series of experiments performed upon the kidneys of rabbits; from the first of which it appears that, when the return of blood is either entirely or partially prevented by ligature of the renal vein, the urine, examined at various periods, from three minutes and half to two days and half, will be found to contain albumen, fibrin, or blood, in variable quantities. In the second series similar results were produced by removing one kidney and tying the abdominal aorta below the point where the renal arteries are given off. We very much doubt the value of these vivisections in determining the point for which they were undertaken, viz. the ascertaining "of the precise cause of the appearance of albumen in the kidney," and, we believe, the rationale of inflammation: for it is quite clear that, excepting in very extraordinary cases, such an amount of *sudden* compression as was produced in them (and which certainly appears to be an essential condition) cannot take place in the human body. And although, in the instances before us, the results were evidently effected by merely mechanical means, is this any proof that in the slower or more complicated actions of inflammation or other disease, we have to look for no further cause?

V. *An account of an unusually large biliary calculus voided from the rectum,* by J. A. Wilson, M.D. The calculus in this case was as large as a full-sized walnut. On its passage to the bowels it induced jaundice, but occasioned *no pain.* The centre consisted of nearly pure cholesterine, and the external part of inspissated and altered bile, with a little cholesterine or fatty matter.

VI. *On fatty degeneration of the arteries,* by G. Gulliver, Esq. F.R.S. From the researches of this gentleman it appears, 1, that the white or buff-coloured opaque spots of the inner membrane of the arteries are of a fatty nature; 2, the soft matter, which has been generally called atheroma, and which often collects between the inner and middle coats, is also fatty; 3, the fatty matter is frequently found in the substance of both these coats; 4, a fatty degeneration of the tunics of the arteries is generally connected with that state of them which is the most frequent cause of aneurism, as well as of their obstruction, occlusion, or wasting in aged people; 5, the matter usually contains cholesterine and oleine, and often some margarine; 6, the tunics of ossified arteries, as well as the bony plates, are often pervaded by the fatty substances just mentioned. In an appended note some valuable observations are made upon fatty degeneration in other organs, as the testis, the kidneys, the lungs, &c. which—as indeed the whole paper—deserve a most attentive perusal, but can be only alluded to in this place.

VII. *Remarks on the calculi in St. George's Hospital,* by Dr. H. Bence Jones. This paper is already so condensed that we are unable to compress it into smaller compass.

VIII. *Case of ulceration of the internal jugular vein, communicating with an abscess*, by Mr. W. Bloxam. The abscess resulted from suppuration of a gland under the angle of the jaw, as a sequela of scarlatina. It opened spontaneously, and the bleeding commenced five days afterwards. The ulcer was on the inner side of the vein, of an oblong shape, and about five lines in its long axis.

IX. *Account of an hysterical affection of the vocal apparatus, with cases*, by Mr. Oscar Clayton. The cases occurred, at three separate periods, in a charitable institution for the maintenance and education of female children from the age of nine to fourteen. In the first attack there were well-marked pyrexial symptoms at the commencement, but in the subsequent ones the hysterical and imitative character developed itself at once. The sounds produced by the different patients were exceedingly various, and very loud, beginning in all with a constant hacking cough. No means of treatment was of any avail, until the children were completely separated from each other.

X. *Case of erectile tumour in the popliteal space*, by Mr. Liston. This is interesting, from its rarity in such a situation. It was removed with perfect success. Appended to this paper is a note of an anomalous growth which the author removed some years ago from the sterno-mastoid muscle.

XI. *Two cases of osteo-sarcoma of the thigh-bone, requiring amputation of the limb in both instances*, by Mr. Frogley.

XII. *Remarks on cancrum oris*, by Dr. H. Hunt. The object of this communication is to point out the efficacy of the chlorate of potash in arresting the progress of this frightful disease, or even effecting a cure, if it has not advanced too far. The quantity of the salt given varies from twenty to sixty grains, according to the age of the child, in divided doses, in twenty-four hours, dissolved in water. The beneficial effect is often observed on the following day, almost always on the second: the disagreeable fetor soon lessens, the sores put on a healthy reparative action, the dribbling of saliva diminishes; if there is mere ulceration it very speedily heals; if there is an eschar, it soon separates; and the sore granulates kindly. Aperients are frequently requisite at the same time.

XIII. *Case of ulceration of the pulmonary artery into an abscess of the lungs*, by W. Crowfoot, Esq. The patient was a medical man, affected with tubercular disease of the left lung. The *active* hemorrhage, which destroyed him, commenced about a month before death. On examination after death, the upper part of the lung was found entirely occupied by a large cavity, containing about half a pound of grumous and coagulated blood. The pulmonary artery was so large as at first to be taken for the aorta, for it had a complete curvature to the right, as high up and upon a line with the clavicle of the left side. It opened into the abscess by a funnel-shaped orifice, about two inches from its bifurcation.

XIV. *Cases of strangulated hernia, reduced "en masse,"* by Mr. Luke. The accident which forms the subject of this communication is one little noticed by British writers on surgery; but, when it does occur, is of such grave importance as well to merit the most serious attention. It consists in the reduction of the hernial tumour through the aperture of the abdominal parietes, together with its investing sac, so that the contents still remain strangulated, and as imperatively demand liberation as if the tumour was unremoved from its original situation. It is generally caused by the employment of too much force in the use of the taxis. In the instances, however, which occurred in Mr. Luke's practice, this had not been the case, no violent efforts having, apparently, been had recourse to. We have not space to enter upon any examination of these cases, but must confine ourselves to a brief sketch of the method of proceeding which should be adopted when the occurrence of such a serious accident is suspected. Of course, before determining upon any decided step, the most rigid manual examination must invariably be instituted; and while conducting this the surgeon will bear in mind the usual condition of parts when a hernia has been regularly reduced. He will remember that the presence of the sac, even without any contents, causes an abnormal fulness, obscures the spermatic cord, and renders the edges of the abdominal aperture somewhat indistinct. If, therefore, upon examination, he finds the contrary of all this—no fulness of parts, a well-defined cord, and a large aperture free and unobstructed, and with unobscured margins, his suspicions will be greatly strengthened. In some cases there may, in addition, be circumscribed pain in the situation of the returned herniæ; and it is possible that a tumour may be indistinctly felt deep in the abdomen. But whether this be so or not, it is right, if the former circumstances exist, to endeavour to accomplish re-protrusion, by causing the patient to be placed in the erect posture, and then to cough forcibly, strain, and make exertion. If these means are successful—and they may be when the hernia is either in or very near the internal aperture—the ulterior course to be produced is at once self-evident. If they fail, Mr. Luke advises an exploratory operation to be performed; and the following is the one he recommends: A free opening should be made through the integuments, so as to expose the tendon of the external oblique, the external abdominal ring, and the cord; the condition of these parts being carefully noted, either as confirming or setting aside the former opinions. The next step is to lay open the inguinal canal; the state of the cord will then be ascertained, and the surgeon should also seek for the condensed cellular capsule which usually invests the hernial sac, and remains in its place even when the sac is returned. If no tumour is now discovered, the finger should be carried through the internal ring, enlarging it if necessary, and passed from side to side. If a hernial tumour be present it will be at once recognized and found lying externally to the general peritoneal membrane, although within the parietes, and presenting a rounded surface and tense feel. Should the tumour not be present, the circumstance may be ascertained by observing the smooth surface of the peritoneum, and the continued adhesions which it maintains with the parietes immediately surrounding the ring. When a tumour is discovered, it should be brought into the canal by incising the ring to the requisite extent, and should then be opened, and the neck of the sac freely divided. Great care should also be taken that the contents of the sac be perfectly liberated, and, for this purpose, the finger should be passed through the neck after they are reduced.

XV. *Observations on the medicinal properties of the cannabis sativa of India*, by J. Clendinning, M.D. The author gives a very favorable account of this drug. Its exhibition was followed, with remarkably few exceptions, "by manifest effects as a soporific or hypnotic in conciliating sleep, as an anodyne in lulling irritation, as an antispasmodic in checking cough and cramp, and as a nervine stimulant in removing languor and anxiety." These effects were observed in both acute and chronic affections, and in patients of all ages and both sexes. It had also the advantage of not producing the injurious effects of opium. We think the remedy well deserving further trial.

XVI. *On the sugar of diabetic blood*, by Dr. H. Bence Jones. This is an account of the application of H. Trommer's new method of distinguishing between cane and grape sugar, to the detection of sugar in diabetic blood. The test consists in the perfect solution of the precipitate which first forms when caustic potash is added in excess to a solution of sulphate of copper and grape sugar, and the after-formation of a peculiar coloured precipitate by heating the mixture. In performing this experiment with serum, it is essential that the albumen be entirely removed by evaporation to perfect dryness; the residue must then be reduced to a very fine powder, and treated with water for a considerable time: then filtered fluid will sometimes, but not always, give the desired results.

XVII. *Observations on encysted hydrocele*, by R. Liston, Esq. The object of this paper is to show that some of these collections of fluid are more intimately connected with the testicle or its seminiferous tubes than has been generally supposed. He was first led to form this opinion from finding the fluid drawn from a sac of this nature "quite full of spermatozoa," and containing also some of the primitive cells in which the spermatozoa are developed, and a certain number of mucous globules. If further observation should confirm these views, the difficulty of effecting a cure will be at once explained.

XVIII. *Account of an epidemic at Teheran*, by Dr. C. W. Bell. In this very singular epidemic, which occurred in January and February, 1842, the patients were affected, often suddenly, with numbness of one side of the body, spasms, convulsions, and insensibility; these were accompanied with extreme excitement of the circulation, violent headach, and frequently pain in the region of the heart. The disease was evidently periodical. Venesection and all lowering remedies were found to be injurious; but a cure was generally effected by the use of assafœtida, in the form of enema, and full doses of carbonate of iron by the mouth. Quinine did not prove trustworthy.

XIX. *On the nature of the ossification of encysted tumours*, by J. Dalrymple, Esq. On examining, by the microscope, a small tumour which had been removed from the tarsal cartilage, he found that its concentric layers were entirely composed of epithelium scales closely agglutinated together; but instead of the usual transparent thin lamina with its central nucleus, they were thickened and hard, and contained granular earthy molecules, which could be removed by immersion in weak muriatic acid. No amorphous earthy deposit existed around or among the scales. The central nuclei were clear and large.

XX. *On the anatomical characters of some adventitious structures*, by Dr. Hodgkin. The design of this very valuable communication is to demonstrate the following points: That the type of the whole family of cancerous diseases is that of compound serous cysts; that microscopic examination does not furnish perfectly conclusive tests of any particular form of adventitious structure to which a specimen may belong, but that it demonstrates the application of the nucleated-cell theory, whilst it is fatal to that of cancerous matter being formed in the blood, and eliminated at the spots at which the tumours became manifest; that to have a complete view of the mode of production of these structures, we must combine the cell-theory of Schwann and Müller, the coagulation-principle which the author had previously suggested, and the process of organization investigated by Kiernan, and that none of these phenomena, taken singly, is an adequate test of malignancy; that chemical analysis affords an imperfect and inadequate criterion; that in removing a tumour of this class, it is extremely important to leave behind none of those minute cysts which often form granules in the surrounding cellular membrane, though it may appear perfectly healthy in other respects; and, lastly, that the infiltrated form of these diseases occurs in the neighbourhood of the purely adventitious growth, when these structures have been the seat of inflammation, and that the chances of success from operation are consequently infinitely diminished when such inflammation has taken place. The presence of the peculiar matter of the disease in the interior of vessels appears to be one of the modes in which infiltration, the result of inflammation, exhibits itself, and is therefore not a valid argument in favour of the preexistence of such matter in the circulating blood. We feel that it is quite unnecessary to say one word regarding the importance of these subjects, and shall simply but earnestly counsel our readers to give themselves the pleasure and profit of a careful perusal of the original, which would be only marred and mutilated by any attempt at condensation.

XXI. *Case of a foreign body in the right bronchus,* by Sir B. C. Brodie. The details of this case, in which the celebrated Mr. Brunel was the sufferer, are sufficiently familiar; we shall merely mention that the foreign body, a half sovereign, was successfully removed (after an opening had been made in the trachea, and the forceps employed without avail), by placing the patient on an apparatus by which his body was inverted, and striking the back over the bronchus with the hand; two or three efforts to cough dislodged the coin and it fell into the mouth. The same plan had been tried before the tracheal opening was made, but it produced such distressing feelings of suffocation that the attempt could not be persisted in.

XXII. *On the pathology of the ear,* by Mr. J. Toynbee. This is the sequel of a paper which we formerly noticed with praise, (Br. and For. Med. Rev., vol. XIV, p. 62,) and we are well pleased to find the author still persisting in a course which must eventually lead to valuable results. He has now performed 120 dissections, and the more extended experience thus acquired fully bears out the opinions formerly stated as to the great frequency of disease of the tympanic cavity. The present communication is occupied with the subject of inflammation of the mucous lining of the tympanum, in which he recognizes three stages. In the *first* stage, the membrane retains its natural delicacy of structure, but the blood-vessels are considerably enlarged and contorted, and blood is effused into its substance, or more frequently at its attached surface. The *second* stage is characterized by a variety of pathological phenomena, of which the principal are: 1. A considerable thickening of the membrane, which is often pulpy and flocculent. 2. Concretions of various kinds on the surface of the thickened membrane. 3. The membranous bands of which notice was formerly taken. In the *third* stage the membrane is ulcerated, the membrana tympani destroyed, and the tensor tympani muscle atrophied. The ossicula are diseased and discharged from the ear, and not unfrequently the walls of the tympanum become affected and the brain involved. Now out of the whole number of dissections, the first stage of inflammation existed in 20, the second in 65, the third in 6, and the organ was found healthy in 29 only.

XXIII. *On the effects of rickets on the growth of the skull*, by Alex. Shaw, Esq. An interesting paper. It is well known that in patients affected with this disease, the body generally retains the characteristics of childhood, the upper half bearing an undue proportion in the lower. Mr. Shaw has shown that the same obtains with the cranium as compared with the face, in the natural skull, the ratio of the former to the latter being as 6 : 1, while in the rickety it is as $7\frac{1}{13}$: 1. He explains this upon the principle, that as years advance the growth of the face proceeds with far greater rapidity than that of the cranium, and consequently, when from disease the progress of development is arrested, that part will most suffer which in ordinary cases would have gained the most decided increase. In confirmation of this, he has observed in individuals whose general growth is above that of the standard size, the dimensions of the skull show a precisely opposite arrangement, the cranium bearing to the face the proportion of only 5 : 1. The diminished or enlarged size of the face appears to depend upon the greater or less development of the frontal and maxillary sinuses; and it is a remarkable fact connected with this, that the diameters of the orbits do not vary, being the same in the normal, the rickety, and the gigantic skull. In reference to the maxillary bones, he has also been led to the opinion that the part which suffers most in rickets, is that portion which is appropriated to the permanent molares.

XXIV. Mr. Lloyd has narrated three cases in which spermatozoa was found in the fluid of *ordinary hydrocele*. Is there no fallacy in these cases?

XXV. *Statistics of Bethlem Hospital, with remarks on insanity*, by Dr. Webster. The following table is very interesting in many respects, exhibiting as it does the number of patients who were admitted, discharged cured, or died, during five periods of twenty years each, ending the 31st December respectively:

In 20 years ending	Number admitted.	Number cured.	Number died.
1762	3286	1069 or $32\frac{1}{4}$ per cent.	714 or $21\frac{2}{3}$ per cent.
1782	3945	1366 $34\frac{1}{2}$ „	560 $13\frac{1}{6}$ „
1802	3906	1379 $35\frac{1}{3}$ „	203 $5\frac{1}{6}$ „
1822	2149	892 $41\frac{1}{2}$ „	111 $5\frac{1}{6}$ „
1842	4404	2269 $51\frac{1}{2}$ „	224 $5\frac{1}{12}$ „
Totals . .	17,690	6975 $39\frac{1}{3}$ „	1812 $10\frac{1}{4}$ „

We would especially direct attention to the very gratifying diminution of mortality and increased number of cures during the later periods, in which milder and more humane methods of treatment have been in operation. Suicides have likewise become more rare; from January 1750 to January 1770 they occurred in the proportion of 1 in about 202; during the last twenty years the ratio has been 1 in 925. During the twenty years ending January 1770, 1 patient out of every 66 made his escape; during the last twenty years the proportion has been 1 in 292, though restraint is now the exception instead of the rule.

There have been 72 dissections made since the 1st January, 1837, of patients who died in the hospital. In 59 of these there was infiltration of the pia mater; in 59, turgidity of the blood-vessels of the brain and membranes. In 41, dropsy of the ventricles. In 27, water at the base of the brain. In 19, bloody points on the cut surfaces of the medullary substance. In 16, thickening and opacity of the arachnoid. In 14, the colour of the medullary or cortical substance was brown, pink, gray, violet, ochre, or white. In 13, there was effusion of blood in the brain. There were also various other lesions in some patients, as effusion of pus, altered consistence, dryness of the membranes, &c. In 55 of the 72 cases there was also disease in the thoracic organs.

The volume is altogether one of unusual interest.

ART. IX.

1. *Om Formen af Nordboernes Cranier.* Af A. RETZIUS.—*Stockholm* 1843. 8vo. *Mit eenige bekorting vertaald door* J. van der HOEVEN, M.D. Tijdschrift voor Natuurlijke Geschiedeniss en Physiologie, Tiende Deel, pp. 127-80.—*Leiden*, 1843.

On the Form of the Skull in the Natives of the North [*of Europe.*] By A. RETZIUS.—*Stockholm*, 1843. *Translated* [*from Swedish into Dutch*] *with some Abbreviation by* J. van der HOEVEN, M.D.; in the Journal for Natural History and Physiology, vol. X.—*Leyden*, 1843.

2. *Ueber die Ureinwohner von Peru.* Von Dr. J. J. von TSCHUDI; in Müller's Archiv für Physiologie; Jahrgang 1844. Heft II, pp. 98-109. —*Berlin*, 1844.

On the Original Inhabitants of Peru. By Dr. J. J. von TSCHUDI; in Müller's Archives of Physiology, for the year 1844. Part II.—*Berlin.*

3. *Bijdragen tot de Natuurlijke Geschiedenis van den Negerstam.* Door J. van der HOEVEN, Med. Dr. Gewoon Hoogleeraar aan de Leidsche Hoogeschool enz. Met vier Platen.—*Leiden*, 1842. 4to, pp. 68.

Contributions to the Natural History of the Negro-race. By J. van der HOEVEN, M.D., Ordinary Professor in the University of Leyden, &c. With Four Plates.—*Leyden*, 1842.

WE propose, from the contents of these very interesting essays, to supply for our readers, some contributions to their knowledge of anthropology, and, especially, of that department of the science which relates to the form of the skull in different nations. We take this as the chief subject of our review, not only because of the number of facts relating to it which these essays furnish,* but, also, because the evidence which can be obtained in England, both in books and in museums, is very defective. The general characters of skulls can be discerned, and comparison between them can be fairly made, only by the examination of large numbers of specimens of each form; but our best collections have, at the most, only one or two of each. And this, while it excuses our imperfect knowledge, will enhance, we hope, the value of this contribution to that of our readers.

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* Complete accounts will be found of no less than eleven forms of skulls, most of which have hitherto been imperfectly described, namely, those of the Swede, Slavonian, Laplander, Finnlander, three races of ancient Peruvians, and the actual Peruvian, the Negro, and the Kafir.

Before entering on the main subject of his essay, Professor Retzius gives an arrangement of the principal tribes of the human race, according to the form of the skull and face; taking for its basis the comparative length of the skull and prominence of the jaws. This character, indicating as it does the degree of development of the posterior cerebral lobes, and the extent to which they overlap the cerebellum, must be important; for it is a main distinction between man and those of the mammalia, whose cerebral organization is nearest to his, that their posterior cerebral lobes are so little developed, that the cerebellum is nearly or quite uncovered. His first division of the nations of men is into *Dolichocephalæ*, and *Brachycephalæ;* the *long-heads* being, of course, those whose cerebral lobes completely cover the cerebellum, the *short-heads*, those in whom they do not. Each of these main divisions is next divided again, according to the prominence or uprightness of the jaws into the *orthognathæ* and the *prognathæ*. On these grounds, the nations whose skulls he has been able sufficiently to examine, may be thus classed:

Class I. DOLICHOCEPHALÆ.

Ord. 1. *Orthognathæ*, Gauls, Celts, Britons, Scots, Germans, Scandinavians.

Ord. 2. *Prognathæ*. Greenlanders, various North and South American Indian races, such as the Caribs, Botocudi, &c., Negroes, New Hollanders.

Class II. BRACHYCEPHALÆ.

Ord. 1. *Orthognathæ*. Slavonians, Finns, and other Tschudisch races, Affghans, Persians, Turks, Lappes, &c.

Ord. 2. *Prognathæ*. Tartars, Kalmuks, Mongols, various North and South American races, such as the Incas, Carruas, &c., Papoes.

If any deduction can be made from this arrangement in its present imperfect state, it is that the most intellectual nations are the Dolichocephalæ Orthognathæ; those, therefore, who, in the high development of the posterior lobes of the cerebrum are most removed from the cerebral character of the brute.

Leaving this, however, let us come to the main subject of the paper. It relates especially to the form of the skull of the Swedes, as compared with those of other European nations, particularly the Slavonians, Finns, and Lappes; and its general result is to prove that the form of the head in Swedes is distinctly different from that in the three neighbouring nations; and that among these three, there are, also, sufficient differences to indicate that they are all of distinct origins.

1. *Of the skulls of Swedes.* Between two and three hundred skulls of Swedes were examined; they were all marked by the same general characteristic form, and the examination of several skulls believed or known to be those of Swedes who lived in the middle ages, proved that this form had been general for at least eight centuries. The general result was that the skulls indicated (by comparison) a remarkable elongation of the posterior lobes of the cerebrum, so that they must not only have covered the cerebellum, but have extended back beyond it.

To describe them more particularly;

"The form of the skull, as seen from above, is ovato-rotund. The greatest length is about one fourth more than the greatest breadth, ... very nearly as 9:7. On the average the length* from the glabella to the most prominent part of the occipital protuberance is 7·48 inches; the breadth, between the temporal fossæ 4·21 inches; the greatest breadth, which is behind the temples, just in front of the parietal protuberances, is 5·78 inches; the greatest circumference, above the glabella and the occipital spine, 21·26 inches; the height, from the anterior margin of the foramen magnum to the vertex, 5·31 inches. . . . . The supra-orbital protuberances are strongly marked; and the skull, behind its broadest part, becomes smaller to the occiput, and is lengthened out to the occipital protuberance, which forms a strongly developed roundish process. . . . . The parietal protuberances are absent, or are rounded and but little prominent. The lambdoidal suture forms the posterior boundary of the skull; its edges extend above the surface of the occiput to the lateral surfaces of the skull. The great transverse occipital ridges meet at almost a right angle below and in front of the very prominent occipital protuberance. . . . . When, also, the skull is looked at from the side, the occipital protuberance appears peculiarly large, like a process, which is bounded above by an impression above the angle of the lambdoidal suture, or in the situation where the fontanelle was—a peculiarity which is an essential characteristic of skulls of this form.

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* All these measurements may be compared with a table hereafter given of the average dimensions of European and Negro skulls.

"In consequence of this remarkable elongation of the occiput, the external auditory apertures are more forward than in the other skulls; a vertical plane carried through them would always very nearly, and often exactly, bisect the line marking the greatest length of the skull. Another consequence of it is that the temporal ridges do not extend so far backwards as in skulls with a short occiput, but, like the mastoid angles of the parietal bones, lie wholly on the lateral parts of the skull, without reaching the occipital surface." (Retzius, pp. 134-5.)

The same remarkable length of the occiput of the Swedish skull is evident in its base. It has, here, an oval form, and the distance between the apices of the mastoid processes, is only equal to the distance between the anterior margin of the foramen magnum, and the hindmost point of the skull. A line drawn from one to the other external auditory opening would touch that anterior margin. The surface on which the muscles of the neck are attached, and over which the cerebellum rests is nearly level; it lies altogether at the base of the skull. The occipital protuberance which covers in the ends of the posterior lobes of the cerebrum, lies remarkably behind the margin of the surface which receives the cerebellum. The foramen magnum has an average length of 1·41 inch, and breadth of 1·14; and in some skulls is pointed at one or both sides or ends. The mastoid processes are generally large and strong; the pterygoid processes nearly perpendicular.

"The bones of the face, looked at from above, hardly project beyond the cranium; the external orbitar processes are small, and the lower margin of the orbit is almost exactly below the upper one. The prominences of the malar bones lie directly below the external orbitar processes, in consequence of the moderate length of the jaws. The zygoma goes in some skulls straight backwards; in others it forms a nearly regular arch; the greatest distance between the two zygomata is from 5·11 to 5·31 inches. The cheek-bones themselves are flattened externally. The orbits vary in the size as well as in the shape of their external apertures from the oval to the circular form; they are, for the most part, arched obliquely outwards, and the space between their inner margins is usually, as in the other northern nations, wide. The palate is, most commonly, high-arched; and the alveolar process of the upper jaw is deep; it measures from the nasal spine to its lower margin from ·787 to ·983, and its lower margin is a little below the level of the points of the mastoid processes, and level with the middle of the ascending ramus of the lower jaw. The face, therefore, is long; on an average, in male skulls, it measures from the naso-frontal suture to the alveolar margin of the foreteeth 3·03 inches. The lower jaw, also, is deep and strongly formed; it measures on an average, 2·95 inches from the condyle to the angle, and 1·38 from its lower border to the border of the alveolar process; and thus, as well as by the teeth being, usually, set almost upright, the comparative length of the face is still further increased." (Retzius, pp. 137-40.)

II. *Of the skulls of Slavonians, or Slaves.* The Slavonian skulls that were examined, were those of a Tschech, a Pole, and two Russians; and these were compared with the heads of Professors Presl of Bohemia, and Purkinje, who is a Tschech, and many other living Slavonians. The differences between them and the Swedish skulls were almost constant and uniform. Instead of the long oval form, they presented a remarkable shortness, and an approximation to the quadrangular or spherical form; all indicated a shortening of the posterior lobes of the cerebrum, so that they could scarcely cover the cerebellum.

> "Seen from above, the skull exhibits a short or transversely rounded oval form, (*forma breviter ovata*); its greatest length being to its greatest breadth, about as 8:7. In three of the skulls the outline resembled a quadrangle with rounded corners, and with the anterior angle smaller than the posterior; in the fourth, (a Russian skull,) it more approached the round form. The bones of the face, like those of the Swedish skulls, when the head was seen from above, appeared but little prominent in front of the cranium.
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> "The greatest length of the skull is about 6·79 inches; the breadth between the temporal fossæ 4·01, and between the most prominent parts of the parietal protuberances 5·94 inches; the circumference round the glabella, and the most prominent part of the occiput 20·47 inches; the height varies from 5·07 to 6·02.
>
> "The upper surface of the parietal bones is broad and little arched; the occiput is not elongated backwards into a protuberance, growing smaller as it goes further back, but it descends more vertically downwards to the occipital ridges. The parietal protuberances are placed by the beginning of the occiput, (i.e. not the *bone*, but the *back of the head*,) which forms a large low-arched or flat surface, occupying the greater part of the height of the skull, and including the posterior part of the parietal bones, with the end of the sagittal suture; and the whole of the lambdoidal suture. The greater ridges of the occipital bone thus form nearly the lower angle of the posterior boundary of the occiput, or of the base of the skull. The arch of the occiput, just above these ridges, forms an arc, whose height is equal to about half the length of its cord, supposing that cord to be drawn, as in the Swedish skulls, between the external apertures of the ears across the anterior margin of the foramen magnum. The two transverse ridges meet at a very obtuse angle, or pass into each other with a slight bend. Hence the occipital protuberance has the form of a low blunt elevation. The surfaces on which the hemispheres of the cerebellum rest are strongly arched, and the posterior parts ascend and pass into the posterior surface of the occiput. The foramen magnum is like that of the Swedish skulls. The distance between the mastoid processes is from 4·48 to 5·5 inches." (Retzius, pp. 145-6.)

The forehead in profile, in consequence of the frontal protuberances, is nearly vertical, (but in one Russian skull it inclined backwards.) The external auditory passages are behind the middle of the head. The mastoid processes are large, and the temporal ridges reach the surface of the occiput. The form of the face is like that of the Swedish skulls.

III. *Of the skulls of Finns.* The skulls of six Finns, the genuineness of all of which was well proved, were found short, *cuneato ovata* in circumference, with high parietal protuberances placed far back. They differed from those of the Slavonians, in having a smaller and rounder occiput, with straight and flat temples, and an elevation of the parietal bones along the sagittal suture. From those of the Swedes, their differences were as great as those of the Slavonian skulls; and from the skulls of the Lappes, they were distinguished by having a stronger bony fabric, stronger mastoid processes and superciliary prominences, and a longer facial profile; as well as by the round occiput, backward parietal protuberances, and the elevation at the sagittal suture. Their forms were such as indicated a somewhat greater length of the posterior lobes of the cerebrum than the Slavonians possess; though those lobes probably extend only just far enough to cover the cerebellum. Their development in breadth, though greater than in the Swedes, is less than in the Slavonians.

"Seen from above, the skull presents a *forma cuneato-ovata*, with its greatest length about 1-5th more than its greatest breadth. . . . . The average length is 7 inches; the average breadth 5·66; the breadth between the temporal fossæ, 3·93; the average greatest circumference 20·63 inches. From the front it appears flat, in consequence of the position of the margins of the orbits, and the superciliary ridges, but the forehead is vaulted. The outline of the temples is nearly straight, and the temples are flat. The parietal protuberances, which stand out prominently, make an angle at their passage to the occiput, whose arch is greater than in the Slavonians, and forms almost a segment of a circle, . . . and never projects far backwards. . . . .

"Seen from behind, these skulls present a nearly quadrangular occipital surface, which is apparently somewhat higher than broad, (in the Slavonian, the height is only equal to or less than the breadth.) The upper side of the quadrangle lies between the parietal protuberances, the lower between the mastoid processes; its vertical sides are between these points; (in the Swedish skulls the mastoid processes are not within the limits of the occiput, but in front of it.)

"In five (of the six) specimens there is an elevation in the course of the sagittal suture, which is also mentioned in the only account of the skull of the Finns proper hitherto published.* The posterior part of this suture, as well as of the parietal bones, bends downwards at the arch of the occiput already mentioned as forming almost a segment of a circle, and as characteristic of the Finnish skull. The apex of the lambdoidal suture is higher than in the Swede—about as high as in the Slave. . . . . . The great transverse ridges of the occiput are rather lower than in the Slave, but higher than in the Swede. . . . they meet rather above the posterior and lower border of the occiput. The greatest convexity of the occiput is at its middle. . . . . . The part of the occipital bone which covers the posterior cerebral lobes forms about 1-3d of the great round arch of the occiput . . . . The height of the arch, drawn from the margins of the auditory openings round the greatest convexity of the occiput, is equal to about 3-4th of its cord; (the line uniting these openings.) . . . . . . The *conceptaculum cerebelli* is peculiarly developed; and, in five cases (of six) its posterior part ascends. . . . . . The foramen magnum is like that of the skulls already described. The distance between the mastoid processes is, in different skulls, from 4·88 to 5·31 inches: the mastoid grooves for the attachment of the depressors of the lower jaw are deep and narrow.

"In profile the forehead is round-arched. . . . . . The frontal protuberances stand out and unite in a prominent glabella. The auditory apertures are behind the middle of the antero-posterior diameter. The arch of the occiput is most obvious. The line of the profile of the face is nearly vertical: its height from the root of the nose to the margin of the upper alveolar process is, in five specimens, 2·75 inches; its width between the greatest convexities of the zygomata varying between 5·03 and 5·7 inches. . . . . The zygomata are directed, for the most part backwards; and their lower margins are nearly straight. . . . . The apertures of the orbits are quadrangular, nearly rectangular, and not so high as they are wide. . . . . The palate is but little arched. . . . . The margin of the upper jaw is on a level with the point of the mastoid process. . . . . The lower jaw is not particularly different from that of the Swede." (Retzius, pp. 154-9.)

IV. *Of the skulls of Laplanders.* The characters of these very interesting skulls were examined in no less than sixteen genuine specimens; a number so much larger than any other anatomist has had the opportunity of comparing, (for even Blumenbach had only two in his collection,) that Professor Retzius' account of them must be received with deference, though it differs in many respects from those hitherto published. The general result of his examination is to prove that the Lappes, entirely different from the Swedes, belong to the races with short occiputs. Herein they agree with the Slaves and Finns; but they are distinguished from these, chiefly by their skulls being smaller and thinner, with smaller mastoid processes, and, in general, having less marked muscular impressions; moreover, by having the occiput more inclined backwards, and somewhat compressed laterally. They differ also from the Slavonians' skulls, especially in having higher parietal bones, with large protuberances; and from the Finns' in their round temples. The Lappes, therefore, would appear to have the middle lobes of the cerebrum more developed than they are in those other nations, while the posterior lobes hardly cover the cerebellum, and exhibit even a less development in breadth than they have in the Finns'.

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* By Hueck, in the Bull. Scientifique, publ. par l'Acad. de St. Petersbourg, t. v, p. 316.

"Seen from above, the Laplander's skull presents an outline approaching to the same short oval form as that of the Finns'; while the parietal protuberances are large and far apart, but the lowest part of the occiput is in some degree prominent, and lengthens out the form, and the temporal bones are more arched, and rounder laterally......... The face, as in other Europeans, projects but little before the front of the skull.

"Among the sixteen skulls, the greatest circumference varies from 18·5 to 21·25 inches; in four of them it was 20·66. The greatest length is from 6·1 to 7·08; the average length of the sixteen being rather less than the length of the Finns' skulls...... The greatest breadth is not, as in the Finns' skulls, between the parietal protuberances, but below and somewhat in front of them, in part on the temporal bones, in part on the mastoid angles of the parietal bones. It varies from 5·23 to 6·16 inches, the average being 5·78. The least breadth is from 3·58 to 4·03, the average 3·93 inches. Therefore the longest diameter is nearly 1-8th more than the greatest breadth, and 2-5th more than the least. The greatest average height is 5·07 inches.

"In thirteen of the skulls the lower part of the occiput comes out in a *tuber occipitale*, compressed at the sides. ......The posterior aspect of the skull presents, as in the Slaves and Finns, the form of a quadrangle, with rounded corners, but somewhat elevated at the sagittal suture. The two upper angles are formed by the parietal protuberances, the two lower by the mastoid processes...... The posterior part of the sagittal suture and of the parietal bones is certainly much inclined downwards, but not so much arched as in the Finns, nor so abrupt in its descent as in the Slaves. The apex of the lambdoid suture lies higher than in the Slaves and Finns, and thus much higher than in the Swedes........ The great transverse ridges lie somewhat higher than in the Finns, are only slightly impressed, and meet at a very obtuse angle, without an occipital spine. The surface for the cerebellum ascends in part, and thus passes to the surface of the occiput, as in the Slaves.

"Twelve of the skulls have an elevation along the sagittal suture. But it does not go backwards, as in the Finns; but begins on the middle of the head, extends forwards and, in some skulls, is continued over the upper part of the frontal bone. The temporal ridges reach to the occipital region........ In eleven subjects the grooves for the attachment of the digastric muscles are but little hollowed, but wide and open. The lesser transverse ridges of the occipital bone form small ridges in the neighbourhood of the foramen magnum. In only one subject have the mastoid processes the average size which they possess in the Swedes', Slaves', and Finns' skulls; in all the others they are small: the extreme distance between them is in most 5·11 inches...... The height of the arch, including the occiput, and raised on the line between the auditory openings, is half, or less than half, the length of its cord.

"The horizontal part of the great ala of the sphenoid bone, which receives the middle cerebral lobes, is very broad and flat. The pterygoid processes are directed somewhat forwards...... Seen from the side, the frontal bone in most instances appears arched a little backwards, but in three is nearly vertical. The parietal bones are high-arched, and reach into the occipital region...... The squamous portions of the temporal bones are small and curved. At their union with the great alæ of the sphenoid they especially stand out. The external auditory openings, which in most cases are round, are for the most part behind, but in some at the middle of the length of the skull.

"The orbital prominences are commonly absent, or but little developed. Nearly all the Lapland skulls have their walls with but slight marks of the attachments of the muscles, and are of little strength.

"The profile of the face is but little different from that of the other inhabitants of northern Europe......... As in them, the distance between the orbits is considerable. The apertures of the orbits are nearly quadrangular, and nearly equal in breadth and height; the average breadth being 1·53, the height 1·29 inch. ....The malar bones are small, and their prominences are turned a little outwards. The malar process of the upper jaw is, on the contrary, large, and forms part of the malar prominence........In consequence of the small height of the malar bone, the zygoma covers, in only a few cases, the point of the coronoid process of the lower jaw; in most cases it ends below the zygoma. The greatest arch of the zygoma is formed by the process from the temporal bone; the greatest distance between the two varies from 4·92 to 5·44 inches, and is therefore much less than in the other inhabitants of northern Europe. The alveolar process is low... ....The arch of the palate also is low, and remarkably flat from the front. A line continued backwards from the alveolar margin and in the same direction, passes, in fifteen specimens, to the external auditory openings. ....The lower jaw is in most small and low, its angle very obtuse, its lower margin in many convex. ......The alveolar margin also is low........The teeth-fangs, in both jaws, are short." (Retzius, pp. 164-71.)

To these, although not entering into the chief part or purpose of the essay, we shall add the description which Retzius gives, from two well-authenticated specimens in his museum:

v. *Of the skulls of Greenlanders.*

"These skulls have a strong bony fabric, strongly-marked muscular impressions, and an ovato-round form, their length being 7·48 inches, their greatest breadth 5·51 inches. In these dimensions they nearly agree with the Swedish; but the breadth in the forehead—which in the Swedes is 4·21—is in them only 3·71 inches. Both the Greenland skulls are knobbed (*knobbelig, knöliga*), and the upper jaw, the malar bone, and the zygoma stand out remarkably from the circumference of the skull.

"The foramen magnum is large: 1·65 inches long, and 1·25 broad. The surface on which the cerebellum rests is large, arched, and remarkably upright; the great transverse ridges of the occiput meet one another at an obtuse angle; the occipital protuberance is round and laterally compressed. The apex of the lambdoid suture lies low, and is very obtuse; the parietal bones arch backwards towards the occipital protuberances; the parietal protuberances are small. The distance between the two external auditory apertures is nearly as great as that between the anterior edge of the foramen magnum and the greatest convexity of the occiput.

"In the West-Greenland cranium there is an elevation along the sagittal suture, which, however, sinks in a little at the middle of the head: in the other cranium the elevation is less, and ceases at the anterior part of the suture. The frontal bone is low, with a slight elevation at the middle, and without protuberances. The temporal ridges reach high up on the parietal bones, and backwards close to the lambdoid suture. The auditory openings are just anterior to the mid-length of the skull, and are small. The mastoid processes are very large: the distance between them 4·92 inches; and the greatest breadth of the skull—5·32 inches—is just above and in front of them. The temporal fossæ are very deep; the temporal portions of the alæ of the sphenoid bone are small, and, as it were, dressed in in front of the part at which the middle cerebral lobes raise up the temporal surfaces. The squamous portions of the temporal bones are large and flat, but prominent at their union with the sphenoid alæ.

"Seen from the front, the frontal bone is small, the external orbitar processes stand very much outwards, the orbital prominences are small, the glabella is elevated: the nasal bones are extremely small, though the breadth between the orbits is the same as in the other northern Europeans. The orbits are large, obliquely placed, with rounded corners, and the external and inferior angle carried downwards: the orbital fissures are large. The height of their anterior aperture is 1·49, the breadth 1·6 inches.

"The upper jaw is high........the malar prominences are large, standing outwards horizontally, cut in an arch below, and descending low over the alveolar portion of the jaw, which is very broad. The distance from the root of the nose to the alveolar margin is 3·14 inches; from the nasal spine of the jaw to the same, ·98. The alveolar margin is broadly rounded, like that which Blumenbach (Decas, v, p. 11,) has described in a Chinese. The palate is low: the pterygoid processes are directed forwards, and small. What is most striking, next to the round prominent upper jaw, is the position of the cheek-bones. Their outer surfaces extend so much from above downwards and *outwards*, that they give these heads, when seen from the front, a pyramidal appearance, such as induced Dr. Prichard to give the name of *pyramidal* to his third class of skulls. The zygomata are very large, and most arched at the middle: the greatest distance between them is 5·68 inches. The ascending rami of the lower jaw are low—2·28 inches; the distance between the angles is 4·52 inches; the chin is round, its depth 1·22." (Retzius, pp. 177-80.)

From these particulars, which we have inserted in unusually long extracts, because the original is within the reach of so few of our readers, we will now point out those general conclusions which are made most probable. In the first place, it is evident, as we have already stated, and as a work by Professor Nilsson, on the History of the Original Inhabitants of the North of Europe, had before shown to be probable, that there is a great difference between the physical characters of the present Swedes and those of the oldest nations of the North, who probably all resembled more closely the Lappes of the present time.

2. The common supposition that the Slavonians, Scandinavians, and Germans are all derived from a common stock is most probably incorrect. The form of the face of the Slavonians differs, indeed, little from that which is general among Europeans; but their skulls, by their approach to the quadrangular or spherical form, differ widely from those of the long oval form which exists in the Swedes and other Europeans, and which Dr. Prichard regards as a general character of the Indo-Atlantic races. And this opinion of the national difference of the Slavonians is confirmed by history, as well as by the manner in which they keep themselves distinct, even when living in the midst of other nations and under foreign governments; a striking example of which is offered by the Tschechi, who have possessed Bohemia for 1000 years; and, notwithstanding their constant intercourse with the Germans, retain still their own language and their national peculiarities.

3. It is further evident that the Lappes and Finns do not belong, as they are usually supposed, to the same race. Their national characters indicate their difference, as well as their respective peculiarities in the forms of their skulls; and it is probable, from the evidence of history and language, that the Finns, Slavonians, and Scandinavians were all nations originally of milder climes, while the Lappes have always inha-

bited the North. There is evidence[*] of their having formerly occupied Finnland, a great part of Russia, the south of Sweden, and the whole of Denmark; from which, it would seem, that having been always unwarlike, unsettled, and little civilized, they were gradually driven by more powerful tribes.

4. The anatomical differences of the Finns are also confirmed, as those of the Lappes are, by historical evidence, (adduced from Professor Keyser, of Christiana,) and which renders it very probable that they and their allied tribes, the Esthonians, are direct descendants of the ancient powerful Scythians, who were, to the end of the fifth century, the dominant tribes on the northern side of the Black Sea, but were afterwards dispersed and, for the most part, driven northwards.

5. A comparison of Retzius' description of the Finns' skull with that by Hueck[†] of the Esthonians' shows great differences between them, but they are of a kind which may be in a great measure due to the differences in the regions they have inhabited, and in their modes of life and social condition. Esthland is a flat country; Finnland, for the most part, mountainous. The Esthlanders were, for many centuries, slaves; while the Finnlanders were, in great part, proprietors of their own land...... And they probably lived under different circumstances long before the beginning of our historical records; though their language still agrees so nearly, that the Esthland can only be regarded as a dialect of the Finnish.

6. Blumenbach and most ethnographers have considered the Laplanders and Monguls to be related. A comparison of the skulls of the Lappes with that of a Kalmuk shows that the chief difference between them is, that the upper jaw in the Kalmuk is large and broad, with a large malar process, and deep maxillary grooves; that the malar bone stands far outwards; and that the whole bony fabric is strong.

7. Again, the relation commonly assumed to exist between the Greenlanders and Laplanders cannot be maintained. The description of skulls of the former by Retzius, which agrees with those which Blumenbach and others have given of the skulls of Greenlanders and Esquimaux, proves that they possess a form which is different from any European skull, and constitutes a division in the long series of American races. Retzius' Greenland skulls agree in many respects with the skulls of two Titicacan mummies in his museum.

The mention of this similarity to the skulls of Titicaca will conveniently lead us from Prof. Retzius' essay (on the value of which we need not dwell), to that which stands next on our list, the paper of Dr. von Tschudi, on the Aborigines of Peru. It is but a preliminary essay to a larger work, which he proposes, on the strength of abundant evidence, to publish on the same subject. Its general purpose is to show that among the ancient skulls to be found in Peru there are three distinctly different forms in different localities; and that in the present Peruvians the general form of the skull presents a union of the features of all the three.

VI. *First form of Peruvian skull belonging to the Chincha race.*

"This skull, seen from before, presents the form of a truncated pyramid, whose basis is turned upwards. The facial part is small, the orbits transversely oval. The upper jaw descends perpendicularly. The external angular processes of the frontal bone are directed nearly vertically downwards, and are short. The superciliary arches are slightly developed. The arching of the frontal bone from the glabella is slight, nearly vertical to the superciliary arches, and thence gradually extending to the coronal suture. The frontal protuberances are well marked. The parietal elevations are very prominent, so that they form the parts of the skull which project most laterally. At the sides and behind, the parietal bones pass almost perpendicularly to their connexion with the temporal and occipital bones. The posterior wall of the occiput descends perpendicularly to the superior transverse ridge, and then gradually bends obliquely inwards and downwards to the foramen magnum.......... The length from the glabella to the point of the occiput opposite to it, which is somewhat above the great transverse ridge, is equal (1 : 1) to the transverse diameter. The inclination of the forehead forms an angle of 68° with the line drawn through the length as above indicated. Camper's angle is 77°. The line from the foramen magnum to the external prominence of the occiput forms an angle of 45°. A plane carried vertically through the meeting of the sagittal and coronal sutures, passes in front of the meatus auditorius and foramen magnum." (p. 99.)

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[*] The greater part of which is to be found in Prof. Nilsson's work, entitled Skandinaviska Nordens Ur-invanare.

[†] De Craniis Esthonum, Dorpat, 1838.

The race to whom the skulls of this form belonged occupied the whole of the coast, which is bounded on the north by Despoblado de Tumbez, on the south by the desert of Atacama, on the west by the Pacific ocean, and on the east by the great coast range of the Andes. Their name is given to them from that of the nation which occupied the coast, between 10° and 14° s. latitude. Their skulls are those which are most commonly seen in Europe, because they are found near the sea-shore of a district many miles in length, covered only by a thin layer of drift-sand. They present, indeed, several varieties of form, such as having one or both sides of the occiput pressed in; but there is no doubt that these forms have been produced by artificial means: the form already described may be easily discerned to be that which is typical of the race by examining a great number of their skulls; and that its peculiar quadrangular form is natural, and not the result of pressure, is proved by its being found in the skulls of children that died unborn; and of whom the skeletons are not unfrequently found in the great burial places of the old Indians of both this and the other races.

VII. *Second form of Peruvian skull, belonging to the Aymara race.*

"Seen from before, this skull is oval: laterally it presents a tolerably regular and somewhat extended arch. The facial portion is large. The orbits are quadrangular; their vertical is equal to their transverse diameter. The upper jaw descends obliquely. The external angular processes of the frontal bone are very strong, short, and directed outwards. The nasal process of the frontal bone is very broad and convex. The frontal bone arches backwards from the glabella, with a tolerably regular inclination, but a greater one than in the preceding form of skull. The superciliary arches are lost; the frontal protuberances are inconsiderable. The parietal bones incline backwards and downwards from their union with the frontal; their protuberances lie deep and are but little developed, so that they are not in the situation of the greatest transverse diameter of the skull, which lies between the upper roots of the zygomatic processes of the temporal bones. The upper part of the occipital bone descends vertically for about an inch from the lambdoid suture, and then bends suddenly and very considerably forwards, and so continues to the foramen magnum with a very slight inclination to the horizon. .... The antero-posterior diameter of these skulls is to their transverse diameter 1 : 1·3.* The inclination of the frontal bone to the line of the former diameter, (a line carried from the glabella to the point of union of the posterior and middle thirds of the occipital bone), = 45°. The inclination of the lower part of the occipital bone, from the foramen magnum to the superior transverse ridge is only 17°; of this to the upper fifth of the occipital bone is 55°; and that of the upper fifth itself is 85°. The vertical and transverse plane through the junction of the sagittal and coronal sutures passes behind the mastoid processes and through the middle of the foramen magnum. Camper's angle is 68°." (V. Tschudi, p. 99.)

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* So it is in the original; but, no doubt, it should be as 1·3 : 1.

This second race of Peruvians originally inhabited the Peruvio-Bolivian table-land south of the mountain chain of Asangara, elevated 12,000 feet above the sea. From them were derived the dynasty of the Incas, who, in the course of a few centuries, subdued all the other races, and at the same time that they imparted to them their own customs and language, gradually almost obliterated the anatomical differences by which they had been distinguished.

VIII. *Third form of Peruvian skull, belonging to the Huanca race.*

"Seen from the front, the skull presents the form of a quadrangle elongated from below upwards and from before backwards, whose anterior side, extending from one cheek-bone to the other, forms the greatest transverse diameter of the head. The facial portion is large, but shorter than in the second form. The orbits are more elongated, their vertical diameter exceeding by some lines the transverse. The nasal process of the frontal bone is intermediate in breadth between the first and second forms. The external angular process of the frontal bone is thin but rounded, and less arched outwards than it is in the other forms. The frontal bone is small and long; its inclination from the glabella is very great. In many skulls it is concave in the middle, and rises before its union with the parietal bones to a very prominent middle *tùber frontale.* Behind the coronal suture the vault of the skull is somewhat concave. The parietal bones first arch from this part a little upwards, and then descend straight to their union with the occipital bone. The latter bone, between the lambdoid suture and the superior transverse ridge, inclines obliquely inwards; and thence arches very rapidly forwards and inwards to the foramen magnum.

"The antero-posterior diameter, from the glabella to the junction of the sagittal and lambdoidal sutures, is to the transverse diameter as 1 : 1·5.* The inclination of the frontal bone to this line forms an angle of only 23°. The vertical transverse plane through the junction of the sagittal and coronal sutures passes through the part at which the parietal, temporal, and occipital bones are united, and between the foramen magnum and the inferior transverse ridge." (V. Tschudi, p. 100.)

The habitation of this third race was limited to the elevated plains and valleys between the mountain group of Asangara and that of Pasco, which are bounded on the west by the coast-range, and on the east by the inner range of the Andes. They are named after one of the most powerful nations belonging to the race; and the very peculiar form of their skull can be still easily discerned in variously mingled generations.

IX. *Of the skulls of the Peruvian Indians of the present time.* One of the results of the wide-extended conquests of the Incas, descendants of a branch of the ancient Aymaras, was, as already said, such a mingling of the original races, that the succeeding generations nearly lost the typical forms of their skulls. Still, however, all the three forms may be found unaltered in the inhabitants of certain very limited localities. Dr. v. Tschudi states from his own observation, that

"The race of the Chinchas occurs unchanged in some villages on the coast, as well in North Peru as in the valleys of the province of Yanyos; the race of the Aymaras is often found unchanged in the high valleys of Southern Peru; and I have found the most peculiar of all, the race of the Huancas in its unaffected purity in some families in the department of Junin." (p. 107.)

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* This also should be reversed, and stand as 1·5 to 1.

But in the majority of the Peruvian Indians, who have kept from any mingling with Whites and Negroes, the form of the skull, though so different from that of other South American races, that it might easily be taken for a primary form, yet may be traced as developed out of the three forms already described. In the several varieties, also, which this general form presents, it approximates more or less to one or other of the three original forms, and those in whom such approximations are found dwell in parts which were formerly occupied by the original race whom they most resemble.

"The skull (which presents the most general characters of the present Peruvians) approximates in its outline most nearly to the quadrangular form of the Chincha skull. The facial portion is strongly developed; the upper jaw stands rather obliquely; the orbits are quadrangular; the external angular process of the frontal bone is directed considerably backwards. The nasal process of the frontal bone is very convex, and descends perpendicularly; the upper margin of the orbit is swollen and prominent. The arch of the forehead has rather a strong inclination backwards, as in the Aymaras. The frontal protuberances are half lost. The posterior part of the frontal bone and the two parietal bones are formed just as in the Huancas; but at the union of the parietal bones with the upper part of the occipital, the Aymara form appears again very evidently, in that the occiput first inclines slightly from the lambdoidal suture and then arches rapidly and with a greater inclination towards the base of the skull. The antero-posterior diameter of the skull extends, as in the Huancas, from the glabella to the union of the sagittal and lambdoidal sutures; its greatest transverse diameter is, as in the Aymaras, between the upper roots of the zygomatic processes of the temporal bones; they are in the proportion of 1·1 to 1, and therefore, in this respect, the skull most resembles that of the Chinchas."

The evidence of the coexistence of some of the characteristic features of all the original races in the skulls of the modern Peruvians, as well as the existence of the three forms unchanged in some of the natives among whom there is no evidence of any artifices being employed to alter the form of the new-born child, are strong facts in favour of all these forms being natural. Dr. von Tschudi says he has found them all well marked in the fœtal skulls; neither has he, in the numerous mummies of Peruvian children, which he has examined with all their clothes, ever found any trace of an apparatus for compressing the head. He remarks, too, as others have done, that the form of the head, artificial as it looks, is not such as could be given by pressure. The horizontal surface of the occiput of the Aymara race ought to have a corresponding inclination of the sinciput; and the remarkable slope of the Huanca forehead should have had some corresponding trace of pressure on the occiput, if it had served as the surface for counter-pressure. The great length of the Huanca skulls also should indicate that the pressure had been made on their sides; but the inclination of the forhead and occiput are opposed to such a view. On the whole, therefore, the evidence is strongly against any artifice having been employed among the ancient Peruvians to acquire the strangest of the forms which their skulls present; although it is certain that such artifices have been and still are practised in other nations,* and, since the Spanish conquest, have been employed by the Chincha race in Peru.

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* We gave some account of the process as employed among the tribes on the Columbia river in the last Number, p. 224.

At the conclusion of this paper, Dr. von Tschudi gives an account of the remarkable occurrence of an *interparietal bone* in the skulls of the children of all three of the Peruvian races. It lies in the situation of the upper angle of the occipital bone, where the parietal bones separate from each other, and is the analogue of the interparietal bone of ruminants and carnivora; it is nearly heart-shaped, its apex lying in the angle of the lambdoidal suture, its base being connected with the occipital bone by a suture which extends between the angles of union of the temporal and occipital bones, a little above the superior transverse ridge. It usually begins to unite with the occipital bone four or five months after birth; the union proceeds from the middle towards the sides, and is not completed at the end of the first year. The existence of this bone was observed in all the children's skulls, (more than a hundred in number,) which were examined, and traces of its suture with the occipital bone are seen in a furrow across this part of all the skulls of these races. The bone might, therefore, if it is found so generally in no other American race, be called the *Os Incæ*, to indicate its remarkable occurrence as a normal formation in one portion of the human race, though it is found only very rarely in any other.

We come, in the last place, to the work of Professor van der Hoeven. He has been long occupied in the study of the natural history of our race, and has published several admirable, both original and translated, papers on it in the 'Tijdschrift,' of which he is joint-editor with Professor de Vriese; and of which we may take this opportunity to say that among the periodicals dedicated to the pure and wide science of physiology, it stands second to none. The present work is the first of a series which he proposes, if it be possible, to publish, collecting in them his scattered anthropological essays, and adding what his continued inquiries have taught him. It has an introduction, containing some good general remarks on the study of the natural history of man, and on the divisions of the human race proposed by former writers on the subject. These we shall pass by; for we wish to keep our first intention of treating only of the forms of skulls; when the learned Professor finishes his essays, and sums them up, we hope to have the pleasure of entering with him into the matter in a more general way; for certainly few are more able than he to give instruction.

x. *Of the skulls of Negroes.* The chief value of the examinations here published lies in the comparative measurements of the skulls of Negroes, Europeans, and Chinese. In the following table the admeasurements given are the averages in the male skulls of ten Negroes and twenty Europeans (namely, five Russians, five Dutchmen, five Spaniards, an Italian, a Scotchman, an Englishman, an Irishman, and a Pole.) The extremes between which the averages lie were in no case wide apart; all the particular measurements are, in the original, given in detail. For the convenience of comparing these dimensions with the corresponding ones of the skulls of the northern Europeans already given from Retzius' paper, we have reduced all the measurements from the decimals of the French metre, in which they are stated, into English inches, reckoning the metre at 39·3708 inches. The *height* of the skull means the distance from the foramen magnum to the vertex; the *length* that from the glabella to the furthest part of the occiput; the *length of the vault* is measured from the naso-frontal suture over the vertex to the margin of the foramen magnum.

	Europeans.	Negroes.	Chinese.
"Height of the skull	5·66	5·47	5·70
Length of the skull	7·04	6·96	7·00
Greatest breadth of the skull	5·47	5·11	5·43
Breadth of the frontal bone behind the external angular processes	3·74	3·74	3·66
Length of the vault over the vertex	14·67	13·81	14·71
Circumference of the skull	20·51	19·75	20·35
Length of the foramen magnum	1·41	1·37	1·37
Breadth of ditto	1·18	1·1	1·15
Greatest distance between the zygomata	5·15	5·03	5·23
Height of the lower jaw	1·25	1·22	1·29
Distance from its condyle to its angle	2·58	2·40	2·67
Length of the lower jaw, from the angle to the symphysis	3·22	3·38	3·26"

(V. d. Hoeven, p. 33.)

From this table it appears that there is no important difference in the average dimensions of the European and Chinese skulls; the greatest differences being in the breadth between the zygomata and in the circumference. But the Negro skull is in nearly every dimension smaller than either; especially, it has less height, less breadth, and less circumference. Its average breadth is less than the minimum of all the European skulls that were examined; and the maximum length of its vault was less than the average of theirs. This conclusion is opposed to that commonly deduced from the examinations of Tiedemann, who, determining the capacities of skulls by weighing the quantity of millet-seed they would contain, believed he could show that the capacity of the Negro skull was not less than that of the European. Measurement is probably a more accurate mode of examination than weighing in cases of this kind; but Van der Hoeven shows from Tiedemann's own numbers, that *on the average* the Negro skulls, which he weighed or had weighed for him, contained 1-20th less than the European skulls; a difference between them which nearly accords with the differences found in the measurement of their several dimensions; and which is amply sufficient to prove that the popular opinion of the inferiority of the negro brain is true.

It appears also that the Negro skull is in the same inferiority of dimensions when measured according to the craniological plan of Carus, in which skulls are divided into three parts, corresponding with the three cranial vertebræ of which they are composed, and distinguished as the forehead, middlehead, and hindhead. Each of these compartments may be estimated in its three dimensions of length, breadth, and height. The breadth of the first is taken at the coronal suture, of the second at the parietal protuberances, of the third at the lowest part of the lambdoid suture. Their heights, in the same order, are measured with callipers from the upper margin of the external auditory opening to the highest part, 1, of the frontal bone; 2, of the vertex; and, 3, to the most prominent part of the occiput. Lastly, their lengths are measured, 1, from the root of the nose to the beginning of the sagittal suture; 2, from the beginning to the end of that suture; and, 3, from its end to the margin of the foramen magnum.

The following table of the comparative average measurements of these dimensions will be useful for more than its immediate purpose:

	Europeans.	Negroes.
" Height of the forehead . . .	4·92	4·80
Breadth of ditto	4·72	4·38
Length of ditto . . .	4·92	4·72
Height of the midhead . . .	4·95	4·92
Breadth of ditto . . .	5·47	5·11
Length of ditto	4·95	4·75
Height of the hindhead . . .	4·36	4·44
Breadth of ditto	4·33	4·09
Length of ditto . . .	4·52	4·48
Distance between the outer margin of the orbits	3·89	3·89
Distance between the temporal bones .	5·47	4·8."

(V. d. Hoeven, p. 40.)

The rest of Professor van der Hoeven's observations on the Negro skulls are of a general character; chiefly confirmatory of those of previous observers, or complementary to the descriptions already given and well known. Of these, therefore, we need render no account; neither shall we enter upon the subject of his sixth chapter, " the geographical distribution of the Æthiopian race;" for this has been treated at much greater length by Dr. Prichard, upon the evidence of the same authorities as those which are here adduced, and many more besides. We shall conclude with Van der Hoeven's account of the skull of the Kafir (Kaffer, Caffre) tribe, which he describes from five skulls examined by himself, and well authenticated. That which he takes as the type of them, and of which plates are annexed to the work, was the skull of a young Kafir, the son of a chief, twenty-five years old.

XI. *Of the skulls of Kafirs.*

"The height of this skull is about 5·55 inches; its length 7·046; its greatest breadth, which lies between the parietal protuberances, about an inch and a half above the squamous suture, is 4·96 inches. The breadth of the frontal bone behind the external angular processes is 3·46; the length of the vault of the skull 14·76 inches, of which the frontal bone occupies 5·15, the parietal 5·07, and the occipital 4·54 inches. The circumference of the skull is 19·88 inches. The foramen magnum is very oblong, and measures 1·52 by 1·18 inches. The distance between the zygomata is 4·8 inches. The cheek-bones are small. The nasal bones are small, and run to a point; while the ascending processes of the superior maxillary bones are here very broad, and the distance between the orbits is filled up chiefly by them. The nasal bones are not flat, but form an acute angle with each other in the middle line. The superior maxillary bones, below the orbits and beneath the infra-orbital foramen, are deeply hollowed out, and then are continued, small but round, beneath the nose so far forwards that Camper's facial angle is not much more than 70. The teeth. . . .are white, strong, and regular. The lower jaw is long, strong, high in front, (1·19 inches,) not inclining backwards at the chin, but hollowed out below the incisor teeth, and then again projecting at its lower margin. The distance from its condyle to the angle is only 2·16 inches, and the posterior margin of its ascending ramus runs obliquely forwards." (Van der Hoeven, p. 46.)

In its general characters the Kafir skull may be further described as oblong, large, and tolerably high. The nose cannot be flat, as in other Negro races; for in the other skulls examined, although the nasal bones were not so narrow above as in that described above, yet in none were they flat:

"The face is very prominent. The forehead is smooth and round. The occipital bone is in its upper and lateral parts arched and prominent; in the upper middle part flat or even concave, like the parietal bones which at the posterior part by the sides of the sagittal suture are often, and sometimes very remarkably, hollowed out." (Ibid. p. 48.)

It is especially by its greater circumference and greater height that the Kafir skull differs from the common form of Negro skull; and the prominence of the chin is also characteristic; but their general features are the same, and the Negro type is so strongly marked in all the skulls that he examined, that Van der Hoeven does not hesitate to retract the opinion, that the Kafir race must be separated from that of the Negroes, which he had formed on the testimony of travellers.

The acute facial angle, the small size and oblong form, the little distance between the parietal protuberances, and especially between the squamous portions of the temporal bone; and, lastly, the great foramen magnum, all together, make a wide distinction between the Kafir skull and those of the Caucasian races.

In concluding an article on anthropology, in which none but anatomical evidence is adduced in support of the conclusions which are maintained, we are bound to state, on the part of the authors of the essays which we have reviewed, that they have not been wholly negligent of evidence from history, language, and other sources. Both Retzius and von Tschudi's papers are indeed too nearly anatomical; we should hesitate to accept their conclusions, if there be no other evidence for them than that which is contained in these papers; but the former refers to works by Professor Nilsson and others in which he implies that historical evidence in favour of his opinion is contained; and the latter intends his essay only as a preface to a more comprehensive work on the same subject. It would be absurd to neglect or deny the importance of anatomical evidence in questions of this kind; yet, when it stands in opposition to the history of nations, and still more when it is contradicted by the evidence of national language, we think it should give way. We should put a lower value upon it than Professor Retzius does, who, in at least two instances, relies upon it against better evidence from other sources. The Slavonians, whom he regards as a separate race, are proved by language to be most intimately connected with the Goths and Germans on one side, and with the Arian race (including Medes, Persians, and Brahmin-Hindoos), on the other. All the radical words of their speech are found in the languages of these other nations, and their grammatical formation is only a modification of the same principles. Again, as to the diversity of the Lappes and Finns maintained by Retzius, the German writers who have most profoundly studied their history and languages, have come to a nearly unanimous decision, that they are branches of one race. All the evidence for this decision may be found in the third volume of Dr. Prichard's great work. We will also just refer to the notion which Professor Keyser maintains, on the supposed evidence of their history, that the Finns were Scythians. No two races were ever more unlike. The Scythæ were always nomadic, equestrian, warlike, divided into clans, under kings, &c.—the Finns always agricultural, without horses, unwarlike, having no civil government. They had not even a word for *king*, ruler, or potentate of any kind in their speech; and were easily conquered by the Swedes, because they had no political relations between families, and not a notion of clanship.

ART. X.

Die Krankheiten der Bauchspeicheldrüse, etc. Von D. H. CLAESSEN. —*Köln*, 1842.

The Diseases of the Pancreas. By Dr. H. CLAESSEN.—*Cologne*, 1842. 8vo, pp. 368.

ALTHOUGH several valuable contributions to the pathology of the pancreas have been made from time to time in this country by Abercrombie, Bright, Sandwith, Annesley, and others, nothing like a general treatise on the diseases of this organ has been attempted in the English language, with the exception of a short paper by Dr. Bigsby, in the Edinburgh Medical and Surgical Journal for 1835. On the continent several systematic treatises on diseases of the pancreas have been published, the first in Germany in 1812, by Harless,* and more recently by Bécourt and Mondière in France; to these we have now to add the treatise of Dr. Claessen, the most complete which has yet appeared on this confessedly obscure part of pathology.

As the object of our present notice is rather to give a condensed view of the contents of Dr. Claessen's work than to criticise it, we shall content ourselves with observing that it exhibits both the merits and defects due to its German parentage; while, on the one hand, it presents us with a laborious collection and examination of materials from all sources; on the other, it is certainly obnoxious to the charge of prolixity, which we are not unfrequently obliged to bring against the productions of our learned brethren beyond the Rhine.

The introductory part of Dr. Claessen's work is occupied by a short historical sketch of the opinions and observations of the earlier anatomists respecting the pancreas, and with an account of its anatomy and physiology, at least as far as the latter is at present understood. On these points we discover nothing possessing sufficient novelty to detain us. In regard to the opinions so prevalent amongst foreign authors, viz. that the pancreas is strictly analogous to the true salivary glands—that like these it is liable to be morbidly excited by the action of mercury, and when so excited to give rise to frequent watery colourless or greenish stools—and, lastly, that morbid actions are frequently transferred from the one to the other of these glands; our author maintains that many of the facts on which these opinions are founded have been misunderstood, and that the rest are by no means sufficient to justify the very decided manner in which they have been received as true. We are inclined with our author to think that the data on which these doctrines are founded, especially the two latter, are not sufficient to justify their reception without further question, a point on which it may be the more necessary to insist, as they have been lately introduced, with approbation, to pathologists in this country by Dr. Battersby.†

The work before us is arranged in two divisions: The first contains considerations on diseases of the pancreas in general, under the respective heads of occurrence, duration, symptomatology, complications, diagnosis, prognosis, and cure. The second division contains a special exposition of diseases of the pancreas, whether functional, inflammatory, attended with morbid change of structure, or arising from morbid growths.

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* Abhandlungen der phys. mediz. Soc. zu Erlangen.

† See a paper on Scirrhus of the Pancreas, in a recent number of the Dublin Journal.

1. *Statistics and etiology.* Dr. Claessen doubts the correctness of the general opinion that affections of the pancreas are very rare, and thinks it has arisen rather from the difficulty of their diagnosis than their non-existence. In corroboration of this opinion, he states that though he had without difficulty collected reports of 300 cases in which disease of the pancreas was discovered after death, he had found only twenty instances in which the physician had ventured from symptoms alone to announce the existence of diseases of this organ, and that in very few of these the diagnosis appeared to have been correct. The size of the gland, and the important function with which it is connected, would also lead us to expect it to be not unfrequently diseased. The evidence which can be offered in favour of hereditary influence is not great, but rather in its favour. Of 322 cases collected by Dr. Claessen, 193 were in men, 129 in women.

In 262 cases in which the age is stated, the disease occurred,

In newborn infants . . .	5 times.
In the first year	2 „
From 1 to 10 years . .	20 „
10 to 25 „ . . .	41 „
25 to 60 „ . .	156 „
After 60	38 „

In many of the narratives, excess in food and drink, especially the latter, is stated to have been the exciting cause of the disease. The action of mercury, the abuse of tobacco, and the long-continued use of cinchona bark, have been supposed to give rise to disease of the pancreas.

In several cases, sudden stoppage of the menstrual discharge was speedily followed by affection of this gland.

Scrofula seems to be a frequent cause of these diseases in children. Metastasis from the salivary glands and from the testicles is thought by some authors to give rise to pancreatic diseases.

Gout and rheumatism have each been accused as occasionally producing similar effects.

A frequent cause of these diseases is supposed to exist in extension of morbid action from neighbouring organs, as the duodenum, the under surface of the liver, &c. to the pancreas. This cause Dr. Claessen thinks much overrated, and considers that the evidence goes rather to show that there is an indisposition on the part of the pancreas to take on the diseases of neighbouring parts, in evidence of which he cites several instances in which the pancreas was found healthy, though surrounded and pressed on by malignant and other diseases of neighbouring parts.

2. *Symptomatology and diagnosis.*

"In a certain number of cases (51) the pancreas was found more or less altered in structure, without any symptom having occurred during life to lead one to expect this. In some instances the patient had died from disease of a distant part, as of the viscera of the head or chest; in some, from well-marked acute abdominal disease; whilst in others, the organs adjoining the pancreas had been chiefly affected, and had given rise to a set of symptoms none of which seemed particularly attributable to the pancreas. From this latter class of cases the conclusion has been attempted to be drawn, that in complications of pancreatic diseases, with diseases of the neighbouring organs, the former are prevented from manifesting themselves, and are supplanted and concealed by the symptoms of the latter affection."

Our author combats this as a general proposition, and states that in many of these cases the change of structure in the pancreas consisted only in some trifling alteration of size or consistence, which scarcely merited to be considered as morbid. He, however, admits that a suspension of the function of this gland may take place without any apparent disorder of the operation of digestion, and this even when the suspension is caused by the invasion of the gland itself by abnormal formation. This is, however, by no means peculiar to the pancreas.

In the majority of cases, however, organic disease of the pancreas does not exist without giving rise to marked disturbance, first of the digestive functions, and by reflected action in more distant parts.

In order to arrive at satisfactory conclusions respecting the symptoms to which diseases of the pancreas give rise, it was not only necessary to select cases in which the diagnosis had been confirmed by a necropsy, but also to limit the choice to those in which either the pancreas was the only part diseased, or the affections of other organs were too trifling to give rise to doubt whether these might not have been the cause of the symptoms. Seventy such cases were selected by our author.

Local abnormal sensations. These were present in 63 cases, but their character varied as well as their situation. In 28 cases the pain was referred to the region of the stomach, and consisted of either a mere feeling of discomfort or of pressure, stretching or distension in that part; in some cases the feeling was more acute, like burning or cutting, sometimes existing spontaneously, at others excited by pressure. In 12 cases the pain was above the navel; in 15 cases behind the stomach, near the spine, and extending to each side, so as to resemble pain in the kidneys; in 9 cases the pain was more changeable in situation.

These differences did not arise from the presence or absence of disease of the stomach or other organs; and the presence of pain in so large a proportion of these cases shows that even where disease exists at the same time in other parts, the pain may yet fairly be attributed to the pancreatic affection and not to the accompanying disease. "The nature of the pain is very variable, and may be either a dull pressure at the epigastrium; heat and anguish in the precordia, causing incessant restlessness; tension, as of a weight suspended from the stomach; the sensation of a heavy body sinking in as the patient turns on the side," &c.; sometimes the pain is more acute, as of a dog gnawing the vitals, and may even be so agonizing, as almost to cause madness and a desire to tear the flesh.

The pain in duration and severity was altogether independent of external influences, though in several cases it was increased by pressure with the hand or by a loaded state of the stomach or arch of the colon. Occasionally, the position of the body influenced the pain. In general it was constant, in some cases fearfully so, enduring day and night for years. It was generally one of the earliest symptoms. Pain was attended with vomiting in 35 cases; with constipation in 24; with watery evacuations from the mouth in 16; with thirst in 12; with diarrhea in 7; and existed alone in 5.

The great depth at which the pancreas is placed makes it often impossible to distinguish the presence of enlargement of the organ, even when this is considerable; especially if the patient remains tolerably fat, or if dropsy is present. A position on the hands and knees—no meal having been taken for some time—and the bowels having been previously emptied by injections, as Annesley advises, will facilitate the examination. The position of the swelling varies much in different cases: generally it will be found situated above the navel, but, where the tumour is large, may extend into the right or left hypochondrium, or right or left flank. In several instances pulsation, synchronous with the heart's action, was felt.*

Thirty cases presented, in a greater or less degree, a *watery discharge from the mouth*, in seven cases with the characters of well-marked pyrosis. In one of these no other symptom of affection of the stomach was present throughout the disease; but in the others the character of the affection gradually changed into vomiting of watery fluid and food, or continued along with these. Generally the pain terminates like a cramp, with a more or less abundant discharge of thin and watery fluid, or of tough, ropy, spittle-like or slimy fluid—tasteless or sourish, sourish and bitter, or decidedly acid—colourless or brown, tinged with blood, yellowish, green, or of a dark greenish hue. Authors have differed in opinion respecting the source of this secretion. Some, as Wichman and Rahn, attribute it to the salivary glands which they suppose to take on a vicarious action on the cessation of the pancreatic secretion. Others conceive it to have its source in the pancreas, and have accordingly called it *sialorrhœa* of the pancreas. Our author objects to both these views. Against the former he adduces the fact, that though at first, in some few cases, the fluid filled the mouth without the patient having been aware of its ascent through the œsophagus, in the further progress of these, fluid of the same kind was distinctly thrown from the stomach by vomiting. In half of the whole number the evacuation was from the first accompanied by vomiting, whilst in five others the patient was sensible of its ascent through the œsophagus. In none of the cases, moreover, was any other symptom of excitement of the salivary glands present.

"In only seven cases do the writers speak of the symptom decidedly as a flow of saliva. In three of these it is simply called so, without any mention of a careful examination of its nature having been made; in two others *vomiting* accompanied the secretion, which may lead us to suspect a mistake on the part of the observer. In one case an involuntary flow of saliva was thought to have set in shortly before death; lastly, in one case, which was attended with mental disorder, the patient spat a large quantity of saliva, night and day. In three other cases in which this symptom is mentioned, we are furnished with no means of judging as to the correctness of the designation."

These cases, our author thinks, give no sufficient support to the notion of vicarious action of the salivary glands in pancreatic disease, founded on the supposed sympathy between these organs. In the great majority of instances it was evident that the fluid ascended the œsophagus; and we have therefore to choose between the stomach and the pancreas as the source of the secretion. Harless, Heischmann, Schmackpfeffer, and others have supposed the pancreas to be the source of the secretion, having been led to adopt this view from happening to meet only with cases of partial hardening of the pancreas. But numerous cases are to be found detailed in which this discharge of fluid was present in a marked degree, in which the structure of the pancreas was so entirely destroyed as to render secretion out of the question. To this we may add, that vomiting of a watery or slimy fluid is a not unfrequent symptom of disease of the stomach itself, of which some remarkable examples are detailed by Andral in his Clinique Médicale. For these and other reasons our author comes to the conclusion, that the stomach is the source of the rejected fluid. This fluid is often thrown off in immense quantities. Frank saw a case in which six pounds of it were brought up in a day; and Trumpes relates one in which from two to three pounds were evacuated every two hours.

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* In a case related by Dr. Battersby in the Dublin Med. Journal, vol. xx, p. 219, the pulsation was so marked, attended with bruit de soufflet, as to lead to the opinion that the case was one of abdominal aneurism. After death, a cyst was found in the pancreatic tumour.

In the 30 cases in which this symptom existed, we find it attended with vomiting in 17; pain in 16; costiveness in 11; diarrhea, attended with costiveness, in 4; diarrhea in 3; loss of appetite in 3; variable appetite in 2.

It will be observed how frequently these watery evacuations are accompanied by pain and costiveness. "The latter," observes our author, "will be shown to be especially connected with inflammation of the pancreas, and this also holds true as regards pain; from which it will be apparent that the increased secretion of the stomach is in most cases excited by inflammatory action in the pancreas."

Thirst and hunger. There are other symptoms which, from their nature, imply disorder of the functions of the stomach, but which are so constantly present in affections of the pancreas as to be necessarily reckoned amongst the essential symptoms of these diseases; and it is evident that, in studying them, only those observations should be selected in which the stomach was found, after death, to exhibit either no sensible change, or only changes of such an unimportant character as not to require to be taken into account. The first to be considered is the disturbances of those sensibilities of the stomach which exhibit themselves as thirst and hunger. Of 28 cases in which the state of the appetite is noticed, we find 19 in which it was either lost or remarkably diminished. In 6 cases the appetite was natural or increased. In 3 cases want of appetite, alternated with natural desire for food. This is never the only symptom present. Those cases in which the appetite was natural were attended with fewest symptoms, whilst of the 19 cases in which a marked diminution or loss of appetite occurred, the following was the proportion in which this was joined with other symptoms: pain in 17 cases, vomiting in 11, costiveness in 7, thirst in 5, watery evacuations from the mouth in 4, diarrhea in 3. Thirst was present only in 12 cases; and in these it appeared rather to accompany a feverish state of the system than as a special symptom of the pancreatic affection.

The state of the tongue was noticed in very few of the above cases, and no particular connexion could be traced between loss of appetite or thirst and a loaded or clean and moist state of the tongue.

> "The tongue is very rarely coated in diseases of the pancreas. As regards the diagnosis this fact possesses interest. The secreting power of the stomach, as will be seen, undergoes a profound change in a certain direction. Enormous quantities are often secreted, in these cases, of a watery, tasteless, or sourish, thin, and ropy fluid; yet the tongue is very seldom coated. In diseases of the stomach the secretion of this organ is also changed; but the tongue is very seldom found in a perfectly natural condition." (p. 81.)

Vomiting. On the important symptom of vomiting we shall give the author's own observations, at length:

> "With a view to a minute investigation of the vomiting which occurs in diseases of the pancreas, we have selected 39 cases, in which the morbid action had either exclusively confined itself to the pancreas, or in which there were sufficient grounds, in the slight affection of other organs at the first appearance of this symptom, for not attributing to them any important share in its production. The slightest form of this symptom was shown in those cases in which no peculiar vomiting of the food occurred, but in which a morbid excitability of the stomach and an inclination to vomit was exhibited, either by continued urgings and retchings, or by the fact that medicines having naturally no action of that kind excited vomiting. In one case, which occurred in a girl of thirteen, who died from disease of the lungs, and in whom the pancreas was found to have undergone what appeared to be tubercular degeneration, there had existed, in addition to the only symptom of disordered digestion—entire loss of appetite, this peculiarity, that the food, as soon as swallowed, was returned again into the mouth, without being rejected, by a sort of ruminating process. In all the other 35 cases there occurred more or less complete vomiting, by which food and drinks, after being taken into the stomach, were sooner or later again rejected. This symptom did not always make its appearance at the same stage of the disorder. We have before had occasion to remark that other consensual disturbances of the functions of the stomach exhibit themselves long before vomiting takes its place as one of the features of the disease. There are, however, cases in which the sympathetic excitement of the stomach betrays itself by no other symptom; indeed, in which vomiting is the only observable sign of disease during their whole progress. The *acute* forms of pancreatic disease exhibit, moreover, from their commencement violent and suddenly-occurring vomiting. To this class may be referred those cases in which, though the progress is not rapid, the development of the morbid process is of a sudden and forcible character; as, for instance, in a case narrated by Storck, under the title *hemorrhagia interna pancreatis*. In the great majority of cases this symptom comes on at a later stage of the disorder.
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> "The form in which this phenomenon continues to exhibit itself is not always the same. In almost all acute cases the vomiting was extraordinarily pertinacious and incessant, excited afresh by every kind of food or drink of even the mildest kind, and continuing, with the same degree of violence, up to the period of death. In one of these cases this symptom exhibited itself in a very peculiar manner. Whilst the accompanying fever retained a continued character, the vomiting was excited or moderated at irregular periods; but when, on the ninth day of the disease, the fever took a tertian intermittent form, the vomiting occurred with greater violence during the fits, and was somewhat moderated during the intermissions. After the gradual disappearance, however, of this intermitting fever, the vomiting continued in an aggravated form for two months unceasingly, and was only quieted eight days before the death of the exhausted patient, who had been kept alive during this long period by means of nutritious clysters......
>
> "Except in one case—apparently a scirrhous degeneration of the pancreas, of an indolent character, in which the vomiting, after having lasted a long time,

again disappeared—all the observations showed that this symptom, having once appeared, continued in the same or an aggravated form, seldom with a lessened degree of violence, and never entirely disappeared......

"Both as regards its frequency, and the periods at which the vomiting takes place, there is considerable variation. It seldom occurs at irregular intervals, frequently as often as twenty times a day, in cases, as it would seem, in which a copious secretion takes place into the stomach. It is more frequent in the morning on a fasting stomach, at which time, on first waking, large quantities of fluid are ejected, that had collected during the night. It occurs most frequently, however, at a fixed period after meals, from two to three hours.

"The vomiting has an effect on the other symptoms and on the general state; local pain is relieved by it, and exacerbated when it is checked. It comes on late, when digestion is much damaged, and food irritating the stomach is rejected: in this state it has the effect of diarrhea, which sometimes replaces it. Where the vomiting is violent the fluid is tinged with bile; sometimes blood is rejected, —in one case so abundantly as to cause speedy death." (pp. 81-5.)

The author remarks, that "an examination of the comparative frequency with which the other symptoms accompany vomiting, may give us an insight into the manner in which it is brought on in diseases of the pancreas." In the 39 cases selected, vomiting was accompanied by pain in 35, watery evacuations from the mouth in 17, costiveness in 13, thirst in 11, loss of appetite in 11, diarrhea in 6, it was the only symptom in 3. Notwithstanding the frequency of this symptom, the opinion maintained by many authors, that vomiting is an unfailing symptom of diseased pancreas, is erroneous. In the cases examined by the author it was found to be much more often absent than present. Indeed, as he observes, it is a symptom of an advanced stage of the disease, and when it has assumed an active state.

Constipation. This was present in 32 cases, and is considered by our author to be also a symptom of the inflammatory stage of pancreatic disease.

Diarrhea was found in 12 cases, and most generally in diseases of a malignant kind.

Other secretions, as of the kidneys and skin, exhibited nothing peculiar.

Vascular excitement was often absent even when other symptoms were very aggravated.

Reflected affection of the nervous system.

"It is probably to the deep situation and to the parenchymatous structure of the organ, that we should attribute the restlessness and distress as well as the other nervous affections which are met with in almost all the acute cases. It was without doubt anxiety and restlessness which drove one patient from his bed, notwithstanding his weakness, or an internal feeling of disease proceeding from the affected part which continually agitated and forced groans from another, in others caused affection of the head with slight delirium, or great faintness, or continued sleepleasness, and absence of mind."

In chronic cases sleeplessness is a very frequent symptom.

Effect on the nutrition. The opinion maintained by Pemberton, that pancreatic disease gives rise to a peculiar degree of wasting is confirmed, Dr. Claessen thinks, by a comparison of numerous cases. At the same time it must be admitted that several cases are recorded by Abercrombie and others, in which the body was found after death to contain rather an unusual amount of fat.

The third section of the work is devoted to the *complications* of the diseases of the pancreas.

1. *Complications with diseases of the stomach.* We have seen that the symptoms of pancreatic diseases are chiefly such as arise from sympathetic affection of the stomach. We also find after death in a certain number of cases, that the stomach has undergone alterations of structure varying in character, but generally such as are the results of inflammatory action. The question then arises, whether during life these latter cases have exhibited gastric symptoms in a more marked degree than those in which no such structural alterations are found in the stomach. To this Dr. Claessen replies as follows:

> "Such an agreement between the symptoms and the results of the necropsy does not necessarily exist; a certain number of cases will be found, and amongst them some of the most exquisitely marked specimens of acute inflammation of the pancreas, which exhibit that symptom which marks the highest degree of sympathetic affection of the stomach, namely vomiting, in a degree paralleled only in acute inflammation of the stomach produced by corrosive poisons, and in which, nevertheless, the pancreas alone is found to have been diseased; in other similar cases an alteration of structure of the stomach is found indeed, but can be shown to have come on at a late period of the disease. On the other hand we find that it very rarely happens that any change of character or increased severity of the symptoms accompanies the invasion of disease of the stomach; and lastly, that very considerable changes of structure of this organ occur without the slightest disorder in its functions."

The cardiac orifice of the stomach is very rarely affected by disease of the pancreas; Rahn relates one such case. The pylorus, on the other hand, was found narrowed and indurated in 15 cases; vomiting was altogether absent in 5 of these; in the other 10 it was present; in 7 of these not in such a degree as to distinguish them from others in which no narrowing of the pylorus existed. In 2 however, related by Serval, where the pylorus was quite closed, nothing but milk and cold drinks would remain down, and in a third the stomach continued to receive food for three, four, or even five days, and then the whole undigested mass was rejected together.

2. *Complication with diseases of the liver.* This complication is nearly as frequent as the preceding. It rarely occurs that the disease begins in the liver, and extends subsequently to the pancreas, or that both organs are simultaneously affected; on the other hand, communication of disease from the pancreas to the liver is very frequent, and this may take place either by actual extension of the morbid process, or as in the case of the stomach in consequence of the physiological connexion of the liver and pancreas. Obstruction of the gall-ducts is not an unfrequent cause of jaundice, which is sometimes the first symptom to give rise to suspicion of pancreatic disease; jaundice may, however, be present without any actual obstruction of the ducts. Dr. Claessen here refers to the cases related by Dr. Bright in the 'Medico-Chirurgical Transactions,' of fatty matter voided in cases of complication of disease of the pancreas and liver, and agrees with Mr. Lloyd in considering the liver as the source of this secretion.

3. The complications of affections of the pancreas with diseases of the *spleen*, *kidneys*, *mesenteric glands*, and the other parts of the alimentary canal, are much more rare; and neither the connexion between these nor

the symptoms by which their presence is indicated is sufficiently clear to merit particular exposition.

4. Much importance has been attached by some authors to the pressure made on the *aorta* by the enlarged pancreas. A variety of affections of the head and chest have been attributed to this cause, but, our author thinks, without sufficient grounds. The cases where such results were obviously due to this cause are very rare.

5. *Complication with fever.* Sylvius and his followers attributed to the pancreas a chief part in the production of intermittent fevers, and Fred. Hoffman takes a similar view. In examining the grounds of such an opinion, Dr. Claessen finds sufficient evidence of a connexion between intermittent fever and certain affections of the pancreas, and quotes some cases to show that these affections may be accompanied by an intermittent as well as a continued form of fever, but concludes by expressing the opinion that this connexion is not more intimate than between these fevers and the affections of the liver or others of the abdominal viscera.

6. *Complication with nervous diseases.* Many of the older physicians, as Fernelius, Trew, and Fr. Hoffman, ascribed to pancreatic diseases great importance in the production of *hysteria*, *hypochondriasis*, *epilepsy*, and other nervous and mental affections. By far the greater number of the cases brought in support of this view, are either so imperfectly related, as to be valueless, or the affections of other more important organs were so considerable as to prevent our coming to any conclusion as to how much might be due to the pancreas.

Diagnosis. Diagnosis of diseases of the pancreas, Dr. Claessen truly observes, has always been considered most difficult. "They have not only been confounded with diseases of the neighbouring organs of digestion, as the stomach and liver, an error into which Annesley appears to have fallen in some of the cases seen and described by him with extreme anatomical accuracy, but have even been sometimes mistaken for those of a very different nature, as disease of the kidneys or pulmonary consumption." This difficulty in recognizing disease of the pancreas, arises from various causes, amongst which are the following:

"1st. The difficulty not to say impossibility of arriving at any certainty respecting functional disturbance of this organ either by direct or indirect methods. The observation of inquirers has not yet been turned to the examination of the pancreatic juice in diseases. Independently of the admixture with other organic fluids which it must undergo before being evacuated, no such examination could be thought of, except in cases where it was voided in large quantities either by the mouth or anus. We have before shown it to be highly probable that the so-called *sialorhæa* of the pancreas, in truth, has its origin in the secretion from the mucous coat of the stomach, and the rarer cases in which diseases of the pancreas are accompanied with purging would probably only furnish a negative result.. The absence of any peculiar colour, smell, &c., in the pancreatic fluid prevent our recognizing its resorption, when the secretion is obstructed, as we often can in the case of bile or urine. .... The fæces probably contain no traces of it, since its constituent parts appear to be altogether of a vital and not of an excrementitial character, and would therefore be absorbed along with the chyle in its passage through the intestines. Again, the part which the pancreatic juice performs in assimilation of the food, and consequently on the nourishment of the body, is not of such striking importance as that its absence from the process of assimilation would be immediately perceived, although on this point depends the importance which has been attributed to wasting as a diagnostic mark of diseases of this organ. It would appear as we have before remarked, that its secretion when checked, is replaced by that of some other part, probably the stomach, since in complete degeneration of this gland the digestion is often not much disturbed, and after its entire abstraction in animals, nutrition is but little affected.

"2d. The depth at which this organ is placed, which bids defiance to external examination.

"3d. The slight sensibility of the organ. Not only is the pancreas itself scantily supplied with nerves, but its connexion with the more sensitive peritoneum is but slight, being clothed by it in front only, and that loosely. Hence the pain caused by inflammation of this organ is but slight, and becomes severe only under particular circumstances; the situation of it is seldom sharply defined and referred to the spot occupied by the diseased viscus, but generally vague and deceptive. The peculiar organic sensibility of this viscus also appears to be but feeble, at least, we can only in this way account for the fact that scirrhus and cancer, which in other structures, as the mammæ and testicles, cause so much pain, remains generally indolent in the pancreas. We can adduce examples of the complete destruction (verjauchung,) of this organ by cancer, in which scarcely any morbid sensitiveness existed.

"4th. Its being surrounded with organs of greater sensibility, and of more importance in the organism. The symptoms which occur in diseases of the pancreas depend partly on the sympathetic affection of other organs connected with this part, especially of the stomach; partly on this, that the morbid processes which slumbered unperceived, whilst confined to the pancreas, have now extended to neighbouring parts, and begin to excite disturbances in the system. Thus, jaundice has often afforded the first evidence of the presence of scirrhus in the pancreas. As this organ does not hold an important place in the general system, its diseases comparatively seldom end fatally without other more important parts being affected. And hence, whilst on the one hand, the group of symptoms occurring in pancreatic disease exhibits an admixture of some unconnected with this organ on the other, the number of cases of pure pancreatic disease observed is limited.

"5th. The slight influence of the diseases on the general systems of the organism, the circulation, nervous system, and secretions.

"6th. The small number of well observed and fully reported cases which we as yet possess."

The author shows that no pathognomonic symptoms exist. Vomiting was supposed by Casper to be a constant symptom of scirrhus of the pancreas, but erroneously; other authors have attached equal importance to the salivary discharge; this, however, is frequently observed. There is, in fact, no symptom which can be accounted pathognomonic; certainly none which can lay claim to be considered essential. Failing this, it has been attempted to form a diagnosis by the grouping together of a certain number of symptoms. Rahn enumerates as "diagnostic, essential, little varying, and very generally present," the following symptoms:

"A pain seated between the pit of the stomach and navel; a perceptible, hard, moveable swelling, giving rise, in the upright posture, to the sensation of a heavy body in the same spot; a constant sour, burning heartburn, extending over the whole gullet, with a copious flow of a watery, tasteless, or sour saliva; a hard dry fecal evacuation; diminished appetite and loathing for food; vomiting coming on in the course of the disease, occurring at intervals, and attended with the rejection of all the food taken, together with a slimy fluid; lastly, wasting and hectic fever, with its accompaniments.

"Pemberton observes that all the patients had complained of a greater or less deep-seated pain in the precordial region, and had felt more or less ill and had wasted; he thinks, therefore, that these appearances, where other symptoms of disease of the stomach, of the back part of the liver, of the gall-bladder, or ducts, or of the small intestines, are wanting, would denote pancreatic disease.

Kreysig says: induration of the pancreas produces the greatest disturbance of digestion; on making pressure over the situation of the stomach, a resistance will be perceived, on careful examination, such as might be caused by a narrow board; the patients have also, though not always, an increased flow of saliva in the mouth, they complain of acidity, &c. Hohnbaum considers, as the most certain signs, eructation of watery fluid; vomiting of water, especially when accompanied with vomiting of the food several hours after a meal; great thirst with a moist tongue; remarkable wasting in the absence of colliquative symptoms."

The author passes in review various other symptoms on which a diagnosis has been attempted to be founded; we shall, however, content ourselves with extracting the following summary of the differences between diseases of the stomach and those of the pancreas:

" *Diseases of the Stomach.*	*Diseases of the Pancreas.*
The pain is readily increased by pressure; and this in proportion to the severity of the pain.	The pain is less frequently and readily increased by pressure; even when very intense, pressure generally fails to aggravate it, or even serves to lessen it. A particular position of the body on one side or the other on the back or belly, or bending forwards, &c. has an effect on the pain.
During this, a notable discharge of saliva occasionally takes place, with swelling and pain of the salivary glands.	The flow of water from the mouth is sometimes so copious as to have the appearance of salivation; but this is never attended by swelling of the salivary glands.
The ravenous appetite found in some cases is often accompanied by faintings, pains of the head, oppressed breathing, which continue until it is appeased.	Increased appetite is sometimes seen, but never attended with such sympathetic affections.
The tongue, besides being coated in various ways, is not unfrequently dry and red; the mucous membrane of the mouth is also often beset by aphthæ.	The tongue is generally clean, or at most, and that rarely, has a thin white or yellowish coating.
Bloody, chocolate-coloured, or coffee-ground fluids are often vomited with little effort, and in diseases of very chronic character.	Vomiting of such fluids is extremely rare, and occurs only in acute cases or with violent retching.
In affections of the cardia, the food is rejected immediately after it has been swallowed. When the pylorus is narrowed, the vomiting has generally a mechanical character, without being attended by other stomach symptoms. Vomiting is often the only symptom.	The vomiting is almost always attended by other disturbances of the functions of the stomach, and has never a simply mechanical character.
Evacuations, caused by administering purgatives, generally aggravate the disease.	They relieve the disorder.
The secretion of the skin is frequently altered, generally lessened, or suppressed, or eruptions take place on it. The urinary secretion is very variously modified.	The secretions of the skin and kidneys are very rarely altered, and then only in consequence of disturbance of the circulation.
Digestion is sometimes attended with feverish excitement, cough, and dyspnœa, or sympathetic palpitations of the heart, are present, and these are aggravated or lessened with the stomach affection.	Both kinds of symptoms are entirely wanting; on the other hand, in a few cases, pulsation is seen or felt in the epigastrium, or a murmur is heard there, isochronous with the heart's stroke.
Digestion is frequently attended with a sense of weariness, oppression, and pain in the limbs, &c., or other nervous affections are found, as pain in the head, hypochondriasis, &c.	Such symptoms are never found *during digestion;* sometimes during the course of the disease aberration of mind has occurred.
The wasting is not great, it exists in proportion to the duration of the disease.	The highest amount of wasting is not unfrequently seen; sometimes it comes on suddenly.

During their progress, they show great diminutions of intensity, sometimes without evident cause, and sometimes under the influence of internal or external causes. Their progress is more rapid. Crises are more frequently seen.	When they have reached a certain stage, they assume a more stationary condition, with a less amount of susceptibility to external and internal influences. Their duration is very prolonged, and they show no signs of crisis.
They are more within the reach of medical treatment, and are affected by such agents.	To judge from facts hitherto known, it is more difficult to affect their progress by therapeutic agents."

Prognosis. This must be unfavorable, if we are to judge from what we as yet know of diseases of the pancreas; at the same time, although the termination may be unfavorable, the progress is generally slow, sometimes extraordinarily so. The prognosis will also depend upon the stage of the disease, the severity of the symptoms, and the nature of the disease whether malignant or not; and also on the diseases of other parts with which it may be complicated.

Treatment. This, at present, must be founded on general principles, as experiment has not as yet discovered any means specially fitted for overcoming disease in the pancreas. The subduing inflammation by leeches, bleeding, &c.; the use of mercury to overcome chronic inflammation, &c.; the removal of congestion, &c. by the use of saline mineral waters, &c.; probably iodine and its compounds—such are the weapons with which Dr. Claessen proposes to cope with these diseases.

In the Second Part of his treatise, Dr. Claessen proceeds, in the same systematic and minute manner, to treat of the special diseases of the pancreas. Our space will not allow us to notice this portion of the work so fully as we have done the former.

1. *Simple hypertrophy* of the pancreas is so rare a disease that our industrious author has not found on record a single unequivocal case of it.

2. *Atrophy.* Several cases are narrated in which atrophy, or rather the conversion of the gland into fat, had followed the abstraction of the ordinary stimuli by closure of the pylorus, or from other diseases of the stomach or liver in which the food had been rejected. Conversion of the *left* end into fat is sometimes due to obstruction of its secretion, from the duct being pressed on by scirrhus of the *head* or *right* extremity of the gland.

In cachexia and dyscrasia, the wasting of the pancreas is very common.

In people of weak intellect (blödsinnig), in whom the vital functions, and especially the digestive, are often in a very depressed state, the pancreas has been found so wasted as to have been supposed to be altogether absent. Certain cases of wasting of the pancreas are related, accompanied by hardening of the structure, and ossification of the vessels; but Dr. Claessen considers these changes as the result of chronic inflammation and not of wasting or atrophy properly so called.

3. *Calculus.* Sometimes this change appears to take place without any other morbid alteration in the gland or elsewhere, as in cases related by Lieutaud and Baillie. Galeati mentions an instance of a man who had suffered for thirty years from pain in the precordia, vomiting, costiveness, and repeated attacks of jaundice, in whom, after death, the head of the pancreas was found changed into a sac as large as a hen's egg, containing twenty small calculi. Portal relates a case: The pancreas contained a dozen stony concretions, of which some were as large as a

nut. The duct was so much enlarged as to lead to the supposition that some of the stones must have been evacuated. These concretions appear to be composed of carbonate of lime and phosphate of lime in variable proportions.

4. *Hyperemia.* Under this head an interesting case is quoted by our author from Storck, of a woman twenty-eight years of age, in whom, vomiting coming on suddenly and violently during the menstrual period, the discharge ceased. Difficult breathing, cold extremities, and palpitation followed—then faintings. After a month, pulsation at the epigastrium, with weight; then, costiveness and want of sleep. To these succeeded vomitings and bilious diarrhea—great wasting—death. The pancreas weighed thirteen pounds, and contained a sac filled with coagulated blood. Probably disease of the vessel had before existed, and rupture took place on the cessation of the menses, giving rise to a sort of aneurism in the gland.

5. *Inflammations.* Authors agree in stating that acute inflammation of the pancreas is an extremely rare disease. Andral says its existence has never yet been proved by necropsy. This, however, if strictly true, does not disprove the existence of such a disease, since, as M. Mondieù and our author observe, the pancreas is not an organ of such great importance in the economy as that inflammation of its structure would be likely to prove fatal during the acute stage.

Hitherto the history of acute pancreatitis, as given by systematic writers, has been founded on two or three cases, in only one of which, related by Schmackpfeffer, did death take place before the affection had decidedly passed into a chronic state. Even in Schmackpfeffer's case, a month had elapsed from the first appearance of symptoms of acute inflammation of the pancreas, before the patient was carried off by a pleuritic affection, and the original symptoms had, in the mean time, considerably subsided; nevertheless, the state of the organ after death was such as to leave no doubt of the nature of the attack. In this instance, the pancreatic disease supervened on an inflammatory state of the mucous membrane of the stomach and bowels, the consequence of salivation by mercury for the cure of syphilis. Another case, bearing considerable resemblance to Schmackpfeffer's, as regards the symptoms, is related by Juppin in the 'Journal de Médecine,' but here a still longer period elapsed before the disease proved fatal. Both the above cases possess much interest from the well-marked character of the symptoms, and are given at length by Dr. Claessen; they are, however, rather too long for quotation, and we shall therefore select his third case, extracted from Casper's 'Wochenshcrift,' as offering a well-marked instance of acute inflammation supervening on chronic pancreatitis.

"Case. A Frenchman, of athletic build, forty-five years of age, of temperate habits and happy temperament, having always enjoyed good health, was suddenly seized with severe pain in the belly which became much distended, and with violent vomiting of a green bilious fluid, renewed as often as he swallowed any liquid, even water. His ordinary medical attendant bled him: the blood appeared healthy. During the night Dr. Casper was also called to see him. The patient was at this time suffering from obstinate constipation, notwithstanding the administration of various purgatives. His belly was greatly distended, and he complained of constant severe pain and tenderness a hand's breadth above the navel. Elsewhere the belly was scarcely tender, and no hardness could be perceived, the patient being very fat. Everything swallowed was immediately rejected with a full stream of greenish fluid. With a moist though foul tongue he had constant thirst, which he, however, dreaded to gratify. He tossed restlessly from one side to the other, crying out constantly, " j'étouffe, j'étouffe!" an expression denoting rather great discomfort and distress than a want of breath. His pulse was eighty-five, neither full and hard nor, on the other hand, small and oppressed.

"The *diagnosis* was obscure. His first attendant leaned to the opinion, that inflammation existed in the abdominal viscera; but the state of the pulse, and the entire absence of inflammatory appearances in any of the blood drawn at three several times, as well as the absence of any beneficial effect from this and other antiphlogistic means, led Dr. Casper to doubt the correctness of this view. The disease continued its course, unaffected by treatment, for four days, at the end of which time the patient died delirious.

"*Dissection.* Neither the peritoneum nor intestines were inflamed. The liver was free from disease, nor were any gall-stones found. All other organs were healthy, except the pancreas, which was swollen to the size of a man's fist, of cartilaginous hardness, strongly adherent to the stomach and duodenum, and of a brownish red hue. The internal structure was no longer perceptible."

The suddenness and violence of the attack—the deep-seated pain—the vomiting—the urgent thirst—and the extreme restlessness justified the opinion that inflammation was present in the abdomen; whilst the seat of the pain, "a hand's breadth above the navel,"—the moist tongue—the absence of reaction in the circulation, as shown by the state of the pulse and the blood, peculiarities also present in Juppin's case—might serve to distinguish the affection from acute gastritis.

Dr. Claessen contrasts the symptoms of acute pancreatitis and gastritis as follows:

"*Acute Pancreatitis.*	*Acute Gastritis.*
The pain is situated deep behind the stomach in the flank (weichen), it may be violent, but is dull, heavy; it gives rise to a peculiar distress and restlessness, or is attended by great faintness. It holds no proportion to the other symptoms, such as vomiting. Pressure does not increase it in any remarkable degree.	The situation of the pain is more superficial; it is tearing, cutting; it is the most urgent symptom. Pressure increases it.
A swelling is not perceptible even when the pancreas has undergone considerable enlargement, on account, as it would appear, of the accompanying distension of the belly, even when this is inconsiderable.	The distension and upward pressure of the belly are more considerable.
The vomiting is especially violent; in proportion to its severity, greater or less quantities of green fluid are evacuated.	Probably only in cases of inflammation from corrosive poisons does the vomiting attain such a degree of violence; but little bile is evacuated.
Thirst and constipation are common to both disorders, and in similar degree.	
The tongue always moist, either clean or has a light white coating.	The tongue is dry and red, or coated with thick dry crusts, or aphthæ are seen all over the inside of the mouth.
The appetite is little affected, it may be natural or even greater than usual.	The appetite fails altogether, and disgust is exhibited at all kinds of food.
The vascular system takes little part in the disease; the attendant fever is very slight.	In the vascular system a reaction shows itself, answering to the violence of the inflammation; and other feverish symptoms are present.
Strong purgatives and even emetics do not aggravate the symptoms; the latter even seem to have a temporarily beneficial effect.	Emetics increase the pain; only such purgatives are borne as do not irritate the inflamed surface."

No instance is on record of *abscess* in the pancreas as a result of acute inflammation nor of gangrene.

In treating of *chronic inflammation* of the pancreas, Dr. Claessen insists on the necessity for distinguishing those cases in which the organ has simply acquired increased hardness without undergoing any other change from those in which hardness is the result of chronic inflammation, and is attended with the other alterations usually produced by inflammation.

He considers that the pancreas, like the salivary glands, may exhibit various degrees of size and hardness, within the limits of its normal state, and consequently without giving rise to any symptoms during life. The induration produced by chronic inflammation requires also to be distinguished from that produced by scirrhus, and our author has at some length pointed out what he considers to be the distinguishing marks between these two forms of disease as exhibited in the pancreas. We believe, however, that it will be found more easy to draw up such formulæ on paper, than to decide, with the disease before us and the scalpel in hand, exactly under which head an individual case is to be placed.

Nine cases of chronic pancreatitis, more or less fully reported, and possessing various interest, are given by Dr. Claessen from various sources, and serve well to illustrate this form of disease. We give the summary of the symptoms observed in these cases, in the author's own words:

> "All the specimens of this class agree entirely in their symptoms, not only with each other, but with the symptoms before described as marking a case of acute inflammation terminating in a chronic state. They differ from it only in the first stage of the development of the disease as well as slightly from each other. Whilst in all, the progress was at first slow and insidious; in one local pain was for years the only symptom; in another there was weak digestion, requiring a careful attention to diet, followed soon by heartburn; in a third periodical pains in the stomach; and, lastly, in another complete vomiting, by which even from the first large quantities of fluid were evacuated, was the earliest symptom. When the form of the disease had become more fully developed by the appearances of other symptoms, we find that in none of the cases related was any one of its peculiar characters absent; on the contrary, it exhibits throughout a remarkable uniformity in the number and nature of the symptoms. In the first place there was *pain*, either in the epigastrium, or in some situation pointing more especially to the diseased organ, increased by external impressions, as pressure, a full state of the stomach or colon, the position of the body; and, secondly, there was *copious evacuations from the mouth of watery and slimy fluids*, which in its passage through the œsophagus, was sometimes preceded by burning pain at the pit of the stomach, and at a later period subsequently attended by actual *vomiting*. In others, no pain existed, but the passage of the fluid from the stomach was evident to the patient; or lastly in others, though from time to time the evacuation of the fluid was attended with retching, it frequently gathered so insensibly in the mouth, as to mislead the patient and the medical observer as to its source. Thirdly, in most of the cases, *there was diminution or entire failure of the appetite.* Fourthly, there was in all, without exception, the most *obstinate costiveness*, sometimes giving way for a few days, or towards the end of the disease, being supplanted by a diarrhea. Fifthly, there was generally *considerable thirst* with a moist and clean tongue, or with a tongue having a yellow coat. Sixthly, in all there was *wasting*, generally to a great degree, together *with but slight affection of the vascular system, the secretions, or the nervous system.*"

When the pancreatic disease is complicated as it frequently is, with disease of the stomach or of the liver, the difficulty of arriving at an accurate diagnosis will necessarily be much increased.

Suppuration in the pancreas has been described by many authors, and there seems no doubt that chronic inflammation of the pancreas may terminate in this manner. The symptoms to which it gives rise are, however, unknown; if there be any beyond those attendant on chronic inflammation generally.

A few cases are on record of *gangrene* of the pancreas, the result of chronic inflammation. One of these is related by Portal in his 'Anat. Médicale,' which appears to have been a case of chronic disease attacked by acute inflammation terminating in gangrene after twenty days. The symptoms of this also are too obscure to furnish grounds for diagnosis.

Anormal hardness of the pancreas we have before stated is to be distinguished from chronic inflammation. Our author has found forty-eight cases on record, in which the pancreas had acquired this indurated state. In most of these no symptom had led to a suspicion of the existence of disease of the pancreas. Scholler met with an instance of this kind in an infant only nineteen days old, so that there can be no doubt this state of the pancreas existed during fetal life.

Morbid softness of the pancreas has been occasionally met with but rather as dependent on some general disease of the body, as scurvy, scrofula, &c., than as an original affection of this organ.

Pseudo-morphous diseases. The following diseases of this class have been found in the pancreas: Scirrhus, medullary tumour, steatoma, tubercle, melanosis, hydatids. The symptoms caused by these several diseases are very similar, and may be arranged under two stages. In the first, the symptoms are few, and are rather of a mechanical kind arising from the weight of the tumour, or its pressure on other organs, as the stomach or liver, and giving rise to vomiting, to jaundice, &c. In the second stage when an inflammatory action has come on in the part, symptoms arise such as have been described under the head of chronic inflammation.

Scirrhus. After making all allowances for the erroneous descriptions of some of the older authors by which various diseases were classed under the term scirrhus, this still appears to be the form of disease most frequently met with in the pancreas. The symptoms exhibited by scirrhus of the pancreas vary greatly in number and intensity in different cases. This difference Dr. Claessen considers to be owing to the stage at which the disease has arrived, and to the presence or absence of inflammatory action. "As long as inflammation is not present in the morbid structure, nor is excited in the surrounding parenchyma of the organ by the presence of this parasitic formation, we have merely an indolent swelling, which has no other effect than to put a stop to the function of the organ. Hence it has not unfrequently occurred that a scirrhus of the pancreas has been unexpectedly found in the bodies of persons dying of the most varied diseases." (pp. 278-9.) The symptoms, when any are present, are rather of a mechanical kind, arising from the weight or pressure of the tumour on neighbouring parts. "As soon as the inflammatory process establishes itself in the tumour or in the neighbouring parts, the various anormal conditions of sensation and sympathetic affections of the stomach come on, by which the disease obtains, more or less, the character of chronic inflammation of the pancreas."

Dr. Claessen has selected 16 cases of scirrhous pancreas from various sources, to illustrate the disease in its two stages, or as complicated with inflammation to a greater or less degree; and has endeavoured from these to elicit the means of diagnosis between scirrhous and chronic inflammation. We fear, notwithstanding these endeavours, we must be contented, in many cases, still to remain in doubt respecting the nature of the disease, since the difference is, after all, rather in the degree in which the same symptoms are present in the two diseases than in the difference of the symptoms. We shall, however, give the parallel with which our author concludes his examination :

“ *Chronic Pancreatitis.*	*Scirrhus of the Pancreas.*
The form of the disease (krankheitsbild) comprises very constantly all the symptoms* united.	The group is never complete. When the other symptoms are found, vomiting is generally wanting, and constipation.
A swelling is less frequently to be distinguished; it comes on in a late stage of the disease, is of a smaller size, and is not in proportion to the severity of the symptoms.	A swelling is more frequently found, and is of a larger size ; often it is the first sign of the disease noticeable ; it holds more proportion with the other symptoms.
A single symptom is never found unaccompanied by others.	Often only one symptom is present, and that is most frequently *pain*.
The pain generally precedes swelling, but is preceded by other sympathetic stomach symptoms.	The pain is often periodical, and appears in paroxysms. Often it is relieved by pressure, or by taking food.
These sympathetic symptoms bear a pretty close relation to the severity of the pain.	The pain exceeds all the other symptoms, often in a remarkable degree.
They either make their invasion at the same time as the pain, or precede it.	When sympathetic stomach symptoms occur, it is generally long after the pain.
Vomiting of the food is very constantly present.	Vomiting of the food alone is very seldom found.
The watery evacuations from the mouth are moderate and tasteless.	The fluid rejected from the mouth is sometimes profuse or remarkably sour.
The appetite is always lessened, and thirst generally increased.	The appetite is often normal, sometimes decidedly increased ; which gives a contradictory character to the symptoms. Thirst is rarely present.
The constipation is urgent and enduring. Diarrhea is found only at the commencement or termination of the disease.	The constipation is transitory : it generally alternates with diarrhea ; sometimes the latter alone is found, and has a colliquative character.
Wasting is always present, in a greater or less degree ; but follows the other symptoms slowly, gradually, and towards the close of the disease, and is in proportion to their severity.	Wasting is sometimes entirely wanting; even an undue amount of fat may be found. But often the wasting bears no proportion to the small amount of suffering, or comes on very early.”

We must observe, however, that some of these statements appear to be hardly borne out by the very cases which Dr. Claessen has quoted; and we may add, that though occasionally the aspect of the patient or the prominence of some peculiar symptom—as the vomiting of a peculiarly acrid fluid, colliquative diarrhea, &c.—may make us pretty certain of the existence of malignant disease, we feel assured that our means of diagnosis are by no means sufficiently advanced to enable us, in the great majority of cases, to determine whether the case is one of chronic inflammation, or scirrhus, even should we have overcome the previous difficulty of determining what organ is really affected.

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* Those namely which have been found to be so often present as to deserve to be considered as essential, namely, pain, vomiting, watery evacuations from the mouth, constipation, thirst, loss of appetite, and wasting.

ART. XI.

A Practical Treatise on Diseases of the Eye. By WILLIAM JEAFFRESON, late Surgeon to the Bombay Eye Infirmary, in the Hon. East India Company's Service, &c.—*London*, 1844. 8vo, pp. 308.

MR. JEAFFRESON tells us, in his preface, that although "he has selected an old subject, he trusts that having treated it somewhat differently, it may not prove altogether uninteresting. If he has painted it," he adds, "in new colours, he hopes these colours may be found true to nature." After this announcement, we felt, we must confess, somewhat disappointed to meet with so very few new views in the course of the work. We are ready to grant, however, that Mr. Jeaffreson does not, in general, write unadvisedly; he has evidently studied, with some attention, the opinions of his predecessors on most of the topics which come before him, and has not hesitated to examine and to judge for himself.

In his introductory chapter, and in several other parts of his work, Mr. Jeaffreson refers, with great complacency, to the extensive opportunities he has had of studying practically the diseases of the eye, especially at Bombay, where, in little more than ten years, he treated, he says, 53,359 cases. Of these, he says, 7334 were cases of cataract; and that out of this number, 5517 were "restored to perfect sight by operation or other treatment." (p. 12.) This is a statement which we do not feel disposed implicitly to receive; especially when he reiterates it at page 16 in the following startling terms. "Out of this number," he says, "nearly seven thousand utterly blind persons were restored to perfect sight; namely, one thousand four hundred and thirty-two suffering from amaurosis, and five thousand five hundred and seventeen affected with cataract." Now, laying out of view the amaurotic cases, it is well known to all medical men, that "an eye affected with cataract," to use the words of Mr. Wardrop, "never becomes perfectly blind;" and it is as certain that an eye, cured of cataract by operation, is never restored to perfect sight. Mr. Jeaffreson may perhaps say, however, that his phrases, "utterly blind" and "restored to perfect sight" are not to be taken quite literally, but are small oriental flourishes, which he had picked up during his Indian residence.

Mr. Jeaffreson belongs to the school of ophthalmologists who are terribly afraid of doing too much, in the treatment of diseases of the eye. He cautions us, therefore, at the outset, against "an eagerness to arrest all morbid processes of this organ, with an activity and impetuosity of treatment," which may sometimes lead the practitioner to sacrifice the patient's constitution to the securing of his sight. "The excessive practice," he says, "to which I allude, is especially that of bloodletting, mercurial treatment, and all those means which tend greatly to lower and impair the general powers of the system." (p. 5.)

Upon this question of active treatment, and especially of active depletion from the system by the lancet, we think the rule a very plain one. In those acute inflammations of the eye, which are attended by a quick full pulse, throbbing pain in and around the eye, such as prevents sleep, and an imminent danger of disorganization, or at least of serious morbid change, of some important part, as in purulent ophthalmia, rheumatic ophthalmia, syphilitic iritis, &c., the lancet should always be employed; while in those inflammations which are not attended by such violent symptoms, as catarrhal ophthalmia, scrofulous ophthalmia, &c., general bloodletting may be abstained from. As for mercury, the rule is, also, in our judgment, a very simple one: When the disease is of that sort which is apt to be attended by fibrinous or purulent effusion into the interior of the eye, make the mouth sore. It is a defective diagnosis, which leads those who are imperfectly skilled in eye-diseases, sometimes to destroy their patients' constitutions, by bleeding and mercury, when they should perhaps prescribe tonics, as in scrofulous ophthalmia; and at other times to leave them with the pupil contracted, adherent, and opaque, from the neglect of venesection and mercury. This last event is very apt to happen in the hands of those who take fright at what they call the patient's "want of power."

Chapter II. treats of the *acute non-specific inflammations of the eye*; and, on the whole, we regard this and the following chapter as the best part of Mr. Jeaffreson's book.

After enumerating the local phenomena of inflammation, "redness, heat, pain, and swelling," Mr. Jeaffreson tells us that "to these signs might have been added increased and painful susceptibility of the nerves of the part affected, or in its neighbourhood, to their customary impressions," (p. 19,) referring, we suppose, to the intolerance of light, which attends some of the ophthalmiæ.

With respect to the pain which attends scrofulous ophthalmia, our author emits an opinion which is new to us, namely, that it is owing to the motion made by the iris in excluding the light. "It is this movement of the iris," says he, "which I believe is the cause of pain under these circumstances, just as to move the finger or toe, in the case of common whitlow or gout, occasions great pain." (p. 21.) Whether this notion has any truth in it, we shall not venture to decide. Certain it is, that dilatation of the pupil by belladonna is generally attended by striking relief to all the symptoms of the disease in question, a fact, however, which does not seem to have come under Mr. Jeaffreson's notice.

The following remarks on the general line of treatment to be followed in the inflammatory diseases of the eye are judicious, and may be quoted as a fair specimen of Mr. Jeaffreson's views:

> "In all cases of ophthalmia the medical attendant should carefully inquire into the cause of the affection, and the previous condition of his patient's system. The more intimately he finds the local affection to be connected with conditions of general health, the more carefully must he endeavour to set the general health to rights, which will constitute the most important part of his treatment as directed to the eye; for however much it may be in his power, by local applications, to restore the eye to an apparent condition of health, such restoration will indeed be but apparent and temporary." (p. 26.)

Mr. Jeaffreson's treatment of catarrhal ophthalmia we should regard as over-active, and as leaning too much to the use of tartar-emetic:

> "The treatment required for acute conjunctivitis, includes the use of the emetic substances, especially tartar emetic, and active purging in the first instance; a dose of calomel, combined or not, as the case may require, with some antimonial preparation, or aperient, followed by the common black draught of senna and salts. If the inflammation be very severe an emetic may be found desirable. After the action of such remedies, our object will be by rest, abstinence, and determination

to the skin, bowels, and kidneys, to keep down the force and frequency of the circulation, and with it the local inflammation." (p. 37.)

Mr. Jeaffreson's remarks on collyria coincide pretty much with those of Mr. Travers:

> "As [for] collyria, the simple element water constitutes the great base or menstruum of other collyria, and is often sufficient in itself. I am in the habit of recommending it to be used cold, even in the earlier stages of conjunctivitis, and generally with the best effects; it sometimes, however, happens in these as in some other cases, that cold causes a disagreeable, whilst warm applications induce an agreeable sensation to the patient, and it is always well to have respect to the sensations induced, which usually indicate with great certainty the degree of temperature not only most comfortable but most beneficial to the patient." (pp. 38-9.)

One particular of Mr. Jeaffreson's practice surprises us exceedingly; and that is his employment of the preparations of lead, even in cases where they are almost sure to prove highly injurious.

"More important," says he, "than the collyria, are the various substances used to drop into the eye." (p. 39.) "Some of the substances thus used have a direct tendency to induce contraction of the capillary vessels to which they are applied, as the saturnine lotion." (p. 40.) In purulent ophthalmia, he says, "a little tepid water or goulard lotion should be carefully injected under the edges of the lid four or five times a day." (p. 87.) And in ulcer of the cornea he says, "in these cases the liq. plumbi diacetatis dropped into the eye has the best effect." (p. 115.)

The fact being completely established, that when a solution of a salt of lead is applied to the surface of the eye, it immediately undergoes decomposition, so that an insoluble precipitate of chloride of lead is thrown down, which attaches itself to any excoriated or ulcerated spot of the conjunctiva or cornea, adhering thereto tenaciously, and in the healing of the spot becoming permanently and indelibly imbedded in the cicatrice, we had thought the application of lead to the eye was altogether abandoned. Apply Goulard water to an ulcer of the cornea, as Mr. Jeaffreson advises, and the result is almost sure to be a chalk-white opaque cicatrice. Dr. Jacob's remarks on this subject in the fifth volume of the 'Dublin Hospital Reports,' are worthy of the serious attention of all who meddle with the treatment of eye-diseases.

Mr. Jeaffreson refers to certain applications, which he says "owe their efficacy to their secondary effects, after their stimulating powers have passed off, and to their increasing the secretions of the part, by which the blood-vessels are unloaded and relieved. The vinum opii," he adds, "is the agent most frequently employed for this purpose." (p. 40.) We had always understood opium to possess the opposite effect of that which is here attributed to it, namely, to diminish the secretions of all those organs which come under its influence. It would be strange were the eye an exception to the general rule.

Our author informs us, that "the natives of India are in the habit of using a liquid which they drop into the ear in cases of ophthalmia, and which appears to owe its beneficial agency to a diametrically opposite mode of action to that of counter-irritation." (p. 47.) He believes the "practice to be, in many cases, highly efficacious." It is somewhat remarkable, that after a ten years' residence in India he is able to tell us no more of the composition of the liquid thus employed, than that he suspects "it to consist of narcotic and sedative ingredients."

Chapter III. treats of *chronic and specific ophthalmiæ.*

With respect to acute and chronic inflammations of the eye, Mr. Jeaffreson tells us, that "the state of the tears will sometimes mark a distinction, and it is always of importance," he adds, "to inquire whether they feel hot or cold when falling down the cheek." (p. 50.) We have never happened before to hear of cold tears.

The ophthalmia commonly called *pustular*, Mr. Jeaffreson designates as "vesicular or vesiculo-pustular." The most correct name we believe to be that used by Mr. Morgan, viz. *aphthous inflammation* of the conjunctiva.

At p. 59 Mr. Jeaffreson relates, at great length, a common case of scrofulous ophthalmia, which yielded readily to leeches and a solution of nitrate of silver. We notice this case to point out the inconsistency of the treatment, with the clamorous attack on the practice of leeching, in the very disease in question, only a few pages before.

The notice Mr. Jeaffreson takes of intermittent ophthalmia is very short. It is well known that this disease is extremely rare. We should have been glad, therefore, as Mr. Jeaffreson hints that he has had some experience of it, had he favoured us with some account of its symptoms and course.

"Syphilitic iritis," says our author, "as regards the local phenomena to which it gives rise, does not materially differ from the ordinary inflammation of this delicate structure." (p. 67.) This is an assertion to which we cannot subscribe. The tubercles or small abscesses which rise on the surface of the iris, when this membrane is affected with syphilis, differ most materially from any of the symptoms, which attend the idiopathic or any other variety of iritis. In the treatment of syphilitic iritis, Mr. Jeaffreson trusts chiefly to mercury, and expresses a great fear of taking away blood. "The practitioner should be careful," he says, "how he lowers the powers of life in a person labouring under syphilitic disease." (p. 68.) That cases of syphilitic iritis do occur in debilitated subjects, unable to bear either bleeding or mercury, without danger of sinking, we readily admit; but, on the whole, we believe the error commonly fallen into to be quite on the other side. Depletion is too much neglected, and the cure impeded by too stimulating a diet. Most assuredly, the irregular, adherent, semi-opaque pupil, so often left as a sequela of syphilitic iritis, is the result of trusting too much to mercury and too little to the lancet. "I have seen the disease," says a very competent authority, "running on with rapid strides to dangerous hypopion, notwithstanding the full action of mercury, and its further progress at once arrested by a full bleeding from the arm and a blister on the hindhead."*

Under the head of "gonorrheal ophthalmia," Mr. Jeaffreson makes a remark, which requires some modification to be perfectly correct. "In favour," he says, "of the occasional constitutional origin of the disease, it must be borne in mind, that it [gonorrheal ophthalmia] not unfrequently forms part of a chain of secondary symptoms, chiefly of a rheumatic character, which are apt to supervene upon gonorrhea." (p. 71.) Now, the disease of the eye, which originates through the medium of the constitution, is not at all an affection of the conjunctiva, like the ophthalmia which is produced by inoculating the eye with the discharge from the urethra. It is, on the contrary, an iritis of very severe character, attended with a more profuse effusion of lymph than any other variety of iritis; but, under the influence of depletion and mercurialization, susceptible of a cure much more complete than what generally follows syphilitic iritis. We have no evidence of a purulent ophthalmia or a conjunctivitis forming any part of the chain of secondary symptoms, which undoubtedly supervene, in certain cases, upon gonorrhea.

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* Monteath's Essay on Iritis, Glasgow Med. Journal, vol. ii, p. 59; Glasgow, 1829.

In the active stage of the purulent ophthalmia arising from inoculation of the conjunctiva with gonorrheal matter, Mr. Jeaffreson tells us that "a mercurial course is worse than useless, as tending to break down those constitutional powers of resistance to the disease of which we stand most in need," (p. 73;) and a little farther on, when speaking of the purulent ophthalmia of adults, he says, "In purulent ophthalmia, the mercurial treatment carried to salivation is decidedly objectionable, as increasing the tendency to sloughing and ulceration." (p. 92.) We believe this to be a mere prejudice, and that the treatment so successfully employed by Mr. Muir of Paisley, in the Egyptian ophthalmia, would prove equally efficacious in the gonorrheal or in any other purulent inflammation of the conjunctiva. "I have employed salivation," says Mr. Muir, "in many other cases of ophthalmy with the same happy effects; the pain and inflammation always receding, as the salivary glands became affected, and the sight becoming as strong as formerly. The temporary inconvenience from the mercurial disorder is compensated, not only by the cure of this tedious and dangerous disease, but in many by a better state of health than they for some time enjoyed."* Were we to stop to inquire how it is that mercury proves so beneficial in purulent ophthalmia, we should find the explanation partly in a fact of which Mr. Jeaffreson is perfectly aware. "For it should be stated," he says, "that although the majority of inflammatory affections of the eye are happily slow to extend themselves to other structures than those primarily affected, it is the characteristic of such forms of ophthalmia as induce suppuration to extend themselves rapidly from the outer to the inner and deeper-seated structures of the organ." (p. 84.) Now, in inflammation of the internal textures of the eye, bleeding from the arm and salivation are well known to be our chief resources.

Mr. Jeaffreson is of opinion that in the treatment of eye-diseases, "in proportion to the amount of purulent secretion should we be most wary, most careful, in the adoption of powerful antiphlogistic measures." (p. 76.) Careful we grant, but against the timorous prudence recommended by Mr. Jeaffreson we must enter our *caveat*. His comparisons, drawn from internal abscesses, inflamed liver, purulent secretions from the bronchial mucous membrane, and sloughing compound fractures, are, in our opinion, very little to the purpose. In the acute purulent ophthalmiæ, such as the Egyptian, the gonorrheal, or even the ophthalmia neonatorum, were the oculist to stop, whenever he saw a copious purulent discharge from the conjunctiva, and say to himself, Now, I must use no more antiphlogistic means, what would be the result? The probability is, that in nine cases out of ten, the cornea would become infiltrated with pus, ulcerate, and burst, and the disease end in blindness.

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* Edinburgh Medical and Surgical Journal, vol. vii, p. 245; Edinburgh, 1811.

With respect to the purulent ophthalmia of infants, Mr. Jeaffreson observes, that "a disposition exists in the mind of many practitioners to attribute this disease invariably to the inoculation of the eye with gonorrheal poison. Certain it is," he adds, "that a vast majority of such cases occur in that class of society in which such a cause is most likely to be in operation; but at the same time it must be difficult to prove that no such cases ever occur without the operation of this cause." (p. 78.) This last remark is obscurely expressed; but so far as we can make out the meaning of it, we should say there was no difficulty in the matter. Cases of purulent ophthalmia in infants are not at all uncommon, where the mother is perfectly free from gonorrhea.

In the treatment of the disease, our author uses a solution of nitrate of silver, beginning with half a grain to the ounce of water. This might do very well as a wash, with which to remove the purulent discharge, perhaps every two hours; but is far too weak as a drop, to be applied by means of a camel's hair brush, once or twice a day.

In regard to the effects of purulent ophthalmia, as displayed amongst the modern Egyptians, Mr. Jeaffreson, from personal observation, confirms the accounts of Mr. Lane and other travellers. The following account is interesting:

> "The number of persons one meets in Egypt who are totally and irrecoverably blind is indeed most frightful; but from my own observations I am disposed to think that more eyes are lost from those chronic forms of disease engrafted upon primary acute purulent ophthalmia, than during the active state of this complaint itself.
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> "Nothing is more common than to witness in the bazaars and other frequented places in Egypt, the most disgusting and revolting sight of persons going about with great streaks of puriform secretion running down their cheeks, and matted to their beards with dust and dirt, the accumulation of many days, unwashed. Naturally indolent, in common with the inhabitants of most warm countries, they neglect the earlier stages of disease; and amongst the male part of the population at least, the incentive to obtain relief is abated by their desire to escape military service, at the expense of serious mischief to, or even total loss of an eye; and indeed I understood that it was not uncommon for men voluntarily to inflict blindness of one eye, or other serious mischief upon themselves, with the view of escaping their liability to serve in the army. Another apology for the dirt and especial filthiness of the Egyptians may be found in the general scarcity and want of water.
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> In the hands of an intelligent practitioner, calomel, black draught, and a plentiful supply of water, would alone materially diminish the frequency and virulence of this painful disorder.
>
> The habit of the Egyptians is to crowd themselves much together; their mode of living is very poor; they spend a considerable portion of time in the bazaars and other public places, where the profusion of flies, at all times sufficiently great, is aggravated by the exposure of dates, raisins, figs, and other sweets, by which they are attracted; it is to these flies, carrying the purulent secretion from one eye to the other, that I believe the extension of the disease is frequently due; and I would strongly urge all persons in their travels through Egypt, to be provided with veils for the purpose of keeping off these insects; gauze, or dark-coloured spectacles, may protect the eyes against the glare of the sun, or partially even against dust, but they do not prevent flies from settling on the lids." (pp. 97-9.)

We must confess we are surprised at Mr. Jeaffreson's notions respecting variolous ophthalmia. We had believed that it was established beyond the shadow of a doubt, that the eruption of smallpox never appears on the conjunctiva; that the cornea is never the seat of a smallpox pustule; that the destruction of the eye, which is apt to follow smallpox, is never a primary but always a secondary affection; that it is after the swelling of the eyelids has fallen, the scabs have dropped off, and the eyes are open, that variolous ophthalmia arises; and that the affection of the cornea, so apt to lead to loss of sight, is an abscess in its substance, not a pustule on its surface. The reverse of all this is set down by Mr. Jeaffreson, (p. 101,) by whom everything that Gregory, Guersent, and Marson have said on the subject is either totally unknown or contemptuously neglected.

A very striking defect in Mr. Jeaffreson's work is, that no separate notice is taken of corneitis. Choroiditis is also omitted.

The fourth chapter is devoted to *structural changes the result of inflammation*, and to *certain affections of the humours.*

"Ulcer of the cornea," says our author, "may keep up inflammation of the surrounding parts, just in the same way as an ulcer of the leg, in which case the treatment best suited for the ulcer is best adapted for the inflammatory action of the surrounding parts. (p. 105.) The fact is true, but the illustration is poor and lamely expressed, serving only to recall to mind the felicitous comparison drawn by Andral,* between the affections of the eye, in which inflammation is by turns cause and effect, and similar phenomena, connected with tubercles in the lungs.

Mr. Jeaffreson boasts that it was his "good fortune to be educated under the justly celebrated and talented surgeon Mr. Abernethy." (p. 7.) It may have been from this source, that Mr. Jeaffreson picked up the notion held by John Hunter, that ulceration is a process instituted in disease to prevent the occurrence of a more serious evil, namely, mortification. This opinion Mr. Jeaffreson has outrageously caricatured, when he tells us that his "own conviction is that ulceration is properly, so to speak, a restorative process," (p. 112;) a remark which puts us in mind of the maxim of a talented old teacher of our own, that whenever a man qualified his statements with "so to speak," or "as it were," we might be pretty sure he was talking nonsense. "Why not, then, it may be said," exclaims Mr. Jeaffreson, "allow nature her free course in the processes of ulceration and sloughing? And so we do," adds he. Now we do no such a thing. When called to a case of ulcer of the cornea, knowing well, to use the words of perhaps the man of greatest talents that ever wrote on eye-diseases, that ulcers of this part, "abandoned to themselves or improperly treated, spread rapidly, become deep, and destroy the parts which they occupy," * we immediately set to work to limit the inflammation on which the ulcer depends, and promote the healing processes of granulation and cicatrization, by every local and general remedy in our power. If ulceration is "a restorative process," as Mr. Jeaffreson calls it, we should like to know what granulation and cicatrization will be in his vocabulary.

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* Clinique Médicale, tome ii, pp. 185-6; Paris, 1829.

† Scarpa's Treatise on the principal Diseases of the Eyes, translated by Briggs, p. 214; London, 1818.

Abandoning his principle, that ulcers are good things, Mr. Jeaffreson in a couple of pages necessarily comes to recommend practical views, perfectly consonant to the generally received opinion:

> "The most active steps should now be taken to restore the parts primarily affected to a more healthy condition, which may be affected by the application of the caustic, or its solution, to the ulcer—or when the ulceration is accompanied by a granular state of the lids, as is not unfrequently the case, by the use of blue-stone. Some local stimulants may be generally used with advantage, as dropping the vinum opii into the eye; but sometimes the immediate stimulating effect of the blue-stone or caustic will be sufficient to impart fresh vigour to the vessels of the ulcer, whilst they act as powerful sedatives to the other parts of the eye." (p. 115.)

The last notion would come nearer the truth, were it reversed, thus: Sometimes the effect of the blue-stone or caustic will act as a powerful sedative to the ulcer, while it imparts fresh vigour to the vessels of the other parts of the eye. The caustic, as Scarpa pointed out, destroys the naked extremities of the nerves in the ulcerated part, and quickly takes off the morbid excess of sensibility. This is something like sense, whereas to suppose that caustic will at once invigorate the vessels of the ulcer, and act as a sedative to the other parts of the eye, is as if one were to try blowing hot and cold at the same time.

Speaking of common staphyloma, Mr. Jeaffreson tells us, that "in this form of disease the iris frequently becomes adherent to the posterior surface of the cornea," (p. 123;) but the fact is that such an adhesion, partial or total, according as the staphyloma is partial or total, is a constant part of the disease.

Staphyloma pellucidum, a term applied by some authors to conical cornea, is used by Mr. Jeaffreson to signify hydrophthalmia, which must only give rise to still further confusion.

With respect to artificial pupil, our author informs us, that

> "The most suitable operation for all cases consists in the division of the cornea by the knife, as in the operation for extraction to a sufficient extent as to allow of the introduction of scissors adapted for the purpose, by which a portion of the iris is to be cut away, which will generally, as a matter of necessity, be of a somewhat triangular shape, the segment of a circle; a minute hook will also be necessary, either to hold the iris whilst dividing it, or to remove it after its section." (p. 135.)

Now, how are scissors to be safely introduced, we would ask, so as to cut through the iris, in cases where the crystalline lens is transparent? The thing seems impossible. Mr. Jeaffreson would probably tell us, that in cases where there is no adhesion or other disease of the iris, and where the lens is healthy, the iris will prolapse, so that it will not be necessary to introduce the scissors; but he mentions nothing of this in his book. To attempt to lay hold of the portion of iris, too, by means of a hook, after it is cut by scissors, we hold to be an absurdity. If a portion is to be cut out and removed, it must be laid hold of with the hook before the section is made, not after. Should Mr. Jeaffreson reply, that that is what he meant, then why is it not perspicuously and expressly stated? Referring to the elaborate discussions on artificial pupil to be found in systematic authors, he observes very innocently that there is "some risk of confusion in these very minute details." We apprehend there is much more risk in such meagre general directions, as he himself has favoured us with on the subject.

Chapter v. embraces the *affections of various appendages of the eye.*

For granular lids, Mr. Jeaffreson recommends blue-stone, but there seems some discrepancy between "the free application" of it, which he says "is generally sufficient," and the direction given in the very next sentence, that it should be "passed once or twice lightly over the parts." The following remarks are judicious, and are fully borne out by general experience:

> "This, indeed, may be repeated every other day, and an astringent collyrium be used; but I have repeatedly observed, that unless considerable attention be paid at the same time to the general health, such treatment is either very long in effecting its purpose, or sometimes even utterly useless or injurious. Purgatives, alteratives, tonics, proper attention to air, exercise, and diet, constitute the main objects of general attention." (p. 139.)

We cannot so fully concur in the general treatment recommended in cases of tinea and lippitudo. We agree with our author, that "purgatives will generally be required;" that "a light and nutritious diet" should be given; and that "regular habits and proper exercise in the open air should be enjoined," (p. 142;) but even "the moderate use of such stimulants as table-beer or wine," we consider more likely to aggravate than to abate the affections in question.

The sections on trichiasis and entropium are very imperfect. "I know of no remedy," says Mr. Jeaffreson, "for trichiasis, properly so called." (p. 145.) "No measures with which I am acquainted can, with certainty and confidence, be employed to prevent the further growth of these lashes in their inverted position." He recommends, therefore, that the inverted eyelashes should merely be plucked out from time to time. He just hints (p. 148) that an operation in which the bulbs of the cilia are extirpated, has been had recourse to by some practitioners, but seems to regard it as rarely necessary, whereas it is in trichiasis, that the operations of Vacca and Jäger prove the most satisfactory means of radically removing a very troublesome complaint. He tells us, that for entropium, the milder and simpler operation of clipping out a fold of integuments obviates the extirpation of the bulbs of the cilia. In the acute variety, no doubt, it does; but in the chronic, something more must be done, for the clipping out of a fold of integuments may be repeated over and over again, but still the inverted edge of the lid will brush upon the eyeball, as was long ago shown by Sir Philip Cramptom, in his Essay on the Entropeon. On this subject, Mr. Jeaffreson has evidently much to learn, and we would recommend to his perusal an excellent paper on the causes and cure of the diseases in question, by Mr. Wilde, in the 'Dublin Journal of Medical Science' for March, 1844.

The important subject of ectropium, Mr. Jeaffreson dismisses in two pages. His directions for the treatment of this disease form the most slovenly and useless part of his book.

> "Ptosis consists in the dropping of the upper lid, which the patient is unable by any voluntary effort to raise. It depends upon paralysis of the muscles whose office it is to raise the lid; and may therefore be due to important forms of cerebral disease. It is occasionally, however, congenital, and not unfrequently depends upon affections of the nervous or muscular fibre exterior to the brain itself, being one specimen of that form of facial palsy which Sir Charles Bell was the first to discover was not dependent upon any mischief within the cranium." (p. 151.)

We suspect Mr. Jeaffreson is here confounding palsy of the face, which Sir Charles Bell showed frequently to depend on disease of the portio dura, independent of any mischief within the cavity of the cranium, with ptosis, on which Sir Charles says little or nothing in his papers on the nerves. Ptosis, or palsy of the upper eyelid, except when it arises from an injury of the levator palpebræ superioris, or of the branch of the third nerve, which stimulates this muscle, always appears to depend on a cause lodged within the cavity of the cranium, never on one placed without. Exposure to cold, acting on the portio dura, as it is escaping from the Fallopian aqueduct, will produce palsy of the face, but no such cause affecting the third nerve after it leaves the cavity of the cranium, ever, as far as we know, produces ptosis.

On the subject of cutting for strabismus, Mr. Jeaffreson makes the following remarks:

> "This operation is an ingenious adaptation in the case of deformity of the eye, of the more generally applicable operation of division of the muscles or tendons. It must, however, be remembered that there is this great difference between the applicability of the operation in this instance and in the various other deformities to which the frame is liable, club-foot especially; that whilst in the latter case, mechanical means can be used to secure the parts in their natural position when thus freed from the restraint which has been removed; and whilst active bodily exercise is afterwards used, under proper directions, and at first with the application of mechanical apparatus for securing the natural position of the parts; in the case of the eye all must be nearly or entirely left to chance." (p. 160.)

Mr. Jeaffreson seems quite unacquainted with the fact, that strabismus always implicates both eyes, and to be cured by operation, requires the two adductors, or the two abductors, to be divided, according as the obliquity is convergent or divergent. We cannot admit the correctness of his statement, that "in the case of the eye all must be nearly or entirely left to chance." True it is, that no mechanical apparatus needs, in general, to be applied after the muscles are divided; but much may be done to secure the natural position and natural action of the parts, by what is termed orthophthalmic practice, a very curious and interesting subject, the true principles of which were first developed by Mr. Elliot of Carlisle. Mr. Jeaffreson remarks, that the "operation has been too frequently performed in compliance with the fashion of the day." Perhaps it may be so; but what is a greater evil, it has in general been so badly performed, that we recognize at the first glance, the unfortunate victims of the squint-clipper, by a leer which gives to their countenance an expression really hideous, and not unfrequently with one of their eyes half dislocated from its socket. This is the result of a threefold error: first, digging too deep, and cutting the muscle too far from its insertion; secondly, operating only on one eye, instead of dividing the parallelization between the two; and thirdly, neglecting or misdirecting the subsequent exercise of the eyes.

Chapter VI introduces us to the *affections of the nerves of vision.*

The following is an interesting case of amaurosis, owing, probably, as Mr. Jeaffreson says, to cerebral congestion, and cured by an attack of cholera:

> "Captain F., commanding a troop of cavalry at Hyderabad, in his Highness the Nizam's service, whilst on horseback, was suddenly struck by a coup de soleil. He was conveyed home in a palanquin, and remained some days in a state of almost

total insensibility. The most active antiphlogistic treatment was used by his medical attendant; but the sight, which was from the first entirely lost in both eyes, did not return with the gradual subsidence of the other symptoms. Soon after this he was advised to come to Bombya, a distance of three or four hundred miles, to consult me. He arrived in a most debilitated state, and so entirely blind as not to be able to distinguish light from darkness, not being able to discern the situation of a window, although opened and in the strongest light: although the vacant stare, so peculiarly characteristic of amaurosis, was well marked in this case, yet the iris was obedient to the stimulus of light, contracting and dilating, but neither to the same extent nor with the same activity as in health.

"He was put upon a steady mercurial treatment, strong stimulating applications were made to the eyes, and frequently repeated blisters to the temples. No benefit arising from this plan of treatment, it was discontinued for a course of tonics, which proved equally unsuccessful. He returned to Hyderabad, and in consequence of his total blindness was obliged to retire from the service.

"Having always given it as my opinion, that so long as the mobility of the iris continued, the case ought not to be abandoned as entirely hopeless; at the expiration of a year and a half, Captain F., being again anxious to consult me, came a second time to Bombay. He arrived quite an altered man in appearance, having ridden all the way on a rough-paced elephant. The eyes, however, were in the same state as when I first saw him, and no improvement in vision had occurred. I again repeated the former plan of counter-irritation by blisters, local stimulants, as of electricity, and put him upon a course of quinine, with generous diet; and, as he was a man of remarkably abstemious habits, I ordered him to take considerably more than his usual allowance of wine. He was also desired to take hard and regular exercise on horseback, before and after sunset, along the beautiful sands of the Back Bay, and to use cold sea-water shower-baths. During this state of things, I was one morning suddenly called upon to attend him immediately, and found him labouring under severe and well-marked cholera. Under proper remedies he rallied, when, to our mutual satisfaction and surprise, with the subsidence of this formidable malady came a return of sight, which after a few days became as perfect as ever. The cure was clearly due to the cholera itself, and not to any remedies prescribed by me for that disease." (pp. 181-3.)

The following remarks on the danger of carrying the depleting and reducing plan of treatment too far, even in those cases of amaurosis in which there is reason to suppose it to arise from pressure or from inflammation, appear to be very judicious:

"We now come to speak of an entirely different class of circumstances, as the cause of amaurosis, and that too of no unfrequent occurrence; I allude to debility, exhaustion, and draining of the blood, whether by its direct abstraction, by means of mechanical injury, the lancet, or other causes; or to the slower but equally sure process of drainage, arising from deficient supplies, or exhausting discharges or secretions. The exact state of the retina, optic nerves, and brain, under these circumstances, it is difficult to explain, or why the powers of vision should, of all others, in this particular form of disease be so singularly and prominently affected. That these nervous structures should suffer proportionately with all others in the deficient supply of their vital fluid, and should therefore be proportionately enfeebled in their functions, can be readily understood; but matters go further, vision is not simply impaired, but frequently entirely lost, whilst the susceptibility to their ordinary impressions of other parts of the nervous system may remain unimpaired. Bearing in mind this particular form of amaurosis, I have always been especially careful in adopting that ultra-antiphlogistic treatment for other forms of the affection, which has been by some so unhesitating recommended, and so unflinchingly practised. I have felt disposed to ask myself the question, how far (setting aside the particular condition to which the disease may be due) the continued perseverance in a powerful course of antiphlogistic treatment may render the retina, optic nerves, and brain, incapable of benefiting, by the removal of such condition, even supposing its removal to be within the power of such treatment? Let me be understood: I mean to say, that supposing gutta serena to be due to the pressure of a tumour, to inflammation, to cerebral congestion, to effusion of blood, serum or lymph; and supposing, again, that bleeding, purging, starving, calomel, antimony, &c., had the inevitable power of removing such causes, what benefit would acrue if, in removing them, the nerves of vision become themselves enfeebled and paralysed, incapable of receiving and transmitting their appropriate impressions? If such be even but occasionally the result of this plan of treatment, and considerable experience has convinced me that it is, some useful cautions may be learnt respecting the limits to which such treatment may, with propriety, be carried, even in cases which seem most adapted for it." (pp. 183-5.)

Chapter VII. is devoted to *cataract*, which subject is introduced to our notice by some remarks on the natural structure of the parts concerned.

"The crystalline lens has an outer investing membrane, known by the name of the capsule of the lens, a membrane perfectly transparent in the healthy condition of the part; a limpid and clear fluid, within this membrane, separates it from the lens itself." (pp. 204.)

Now, there is no such fluid in the healthy living eye. On the contrary, the capsule adheres pretty firmly to the surface of the lens. "I do not believe," says Dr. Jacob, "that any such fluid exists in a natural state." "In the eyes of sheep and oxen, when examined a few hours after death, not a trace of any such fluid can be detected."* Dr. Werneck of Saltzburg discovered on the inner surface of the capsule a distinct layer of nucleated corpuscles, which serve as the medium of union between the lens and the capsule.†

Speaking of the examinations necessary for forming a diagnosis of cataract from glaucoma and amaurosis, Mr. Jeaffreson says, "such examinations will be much facilitated, and rendered more decided, by the artificial dilatation of the pupil with belladonna, and the use of a powerful magnifying-glass." (p. 208.)

If the magnifying-glass is looked through, it will prove of little or no use; if employed to throw the light into the pupil, it will be serviceable.

Not a syllable is said by Mr. Jeaffreson of Purkinje's beautiful adaptation of the catoptrical properties of the crystalline, to the determination of its transparency or opacity, although it affords an easy and infallible test, by which to decide on the existence of cataract or of amaurosis.

We believe there never has occurred any well-authenticated instance of the diminution, much less the removal of a lenticular opacity, by natural nor by artificial means, while the substance of the lens was preserved. Mr. Jeaffreson is inclined to think, however, that the thing is possible.

"I believe that at one period the too prevalent habit of studying diseases in respect of their local character, rather than their constitutional relations, has led to the adoption of that very class of remedies in the treatment of cataract which were rather calculated to do harm than good; hence the general opinion that the disease is beyond the influence of all remedies is true in respect of those which have been most frequently tried. For the very agents employed have been those most likely to impair the functions of nutrition in the lens and capsule, by still further lowering the general health. Such are drastic aperients, local bloodletting, counter-irritation, mercurial preparations, &c. Whereas the means which are most likely to arrest the morbid processes on which cataract depends, are simply those which impart tone and vigour to the whole system. The treatment, therefore, which I would advise does not consist in any one particular medicine or local application, but in assiduous attention to the general health of the patient, the use of suitable aperients, alteratives, and tonics; aided by a careful management of the eyes themselves; and I have generally found benefit from copious ablution of the temples, eyes, and cheeks with cold water. This view of the subject, too, may not form an entirely useless hint in the cases of those who have an hereditary tendency to this affection, and who consequently are the more likely to apply for the earliest assistance on the part of the profession." (pp. 218-9.)

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* Cyclopædia of Anatomy and Physiology, vol. ii, p. 200; London, 1837.

† British and Foreign Medical Review, vol. vi, p. 206; London, 1838.

Mr. Jeaffreson gives, at page 221, a case as an instance of the spontaneous cure of cataract, in consequence of improvement of the general health; we do not think it conclusive.

Mr. Jeaffreson's account of the operations for cataract is too general. Speaking of extraction through the cornea, he tells us that "oculists differ in opinion as to which half should be divided, some recommending the upper, others the lower. The operator," adds he, "must of course have decided this point before proceeding to the operation," (p. 228;) a proposition from which few will dissent. We cannot admit, however, the following statement: "It must be remembered that the operation upwards on the left eye requires to be performed with the left hand." (p. 229.) On the contrary, whether the patient be in the sitting or in the lying position, the right hand serves perfectly well for opening either the upper or the lower half of the left cornea. In this case, the operator stands before the patient, while the assistant, placed behind the patient, raises the upper eyelid. If it is the right eye which is to be operated on, the assistant may stand before the patient, and depress the lower eyelid, while the surgeon, taking his place behind the patient, raises the upper eyelid with his left hand, and with the right opens the upper or the lower half of the cornea. Such, we think, is the mode generally followed by those who are not ambidexter.

The directions given by Mr. Jeaffreson for the performance of depression, are neither sufficiently definite, nor perfectly correct. He says, the needle is to be introduced through the sclerotic, "within a line or two of the junction of this membrane with the cornea, and slightly above the transverse line." (p. 231.) Now, it is not a matter of indifference, as Mr. Jeaffreson seems to think, at what precise distance the needle is introduced. Mr. Abernethy, in his lectures, used to direct the needle to be plunged through the sclerotica, as far as one could from the cornea, because, he said, the degree of inflammation was always in proportion to what was done to the front of the choroid, the ciliary processes, or the iris. But he did not advert to the danger of wounding the retina, if the puncture was at the distance of three tenths of an inch from the edge of the cornea, which is very much within the limit of "as far as you can from cornea."* If the puncture is at two tenths of an inch from the cornea, we should conceive the distance a very good one, as both the retina will be avoided, and the folded part of the ciliary body. But if the puncture is made at one line's distance from the cornea, both the gangliform ring of the choroid and the ciliary processes will be wounded. Wounding the ciliary processes we should not consider of much importance, for they seem mere accumulations of blood-vessels, but the gangliform ring is a part eminently supplied with nerves, and we have known an accidental puncture of it to be followed by tremulous iris and amaurosis. It should, therefore, be avoided.

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* Abernethy's Lectures on Anatomy, Surgery, and Pathology, p. 518; Lond. 1831.

Mr. Jeaffreson states no reason why the needle should be introduced "slightly above the central transverse line" of the eye. But there is a very good reason why it should not be introduced there, but exactly in the equator; and that is, that the long ciliary artery divides into two branches, at the distance of three tenths of an inch from the cornea, forming a fork, in the mid-space of which the needle should be passed, so as to avoid both the one branch and the other.

Some modification, Mr. Jeaffreson tells us, of the proceeding followed in depression, constitutes "what is technically called reclination. In this case," adds he, "the lens is not pressed directly downwards, but is turned, as it were, upon its axis." (p. 232.) There is surely some misapplication of terms here; for the axis of a lens is the line joining the middle points of its two opposite surfaces; and to turn the crystalline on its axis, while it remained in the eye, would be rather a difficult thing to do, however easy it might be if it were out of the eye, and stuck upon a pin. Mr. Jeaffreson complains of the *minutiæ* which are to be found in "the more lengthy works of many eminent authors," as "rather calculated to puzzle the practitioner than to assist him," (p. 285;) but we cannot say we have any complaint to make of *minutiæ*, if they be correct; whereas any practitioner trying to turn the lens, "as it were, on its axis," would certainly find himself at fault.

Our author's account of the operation of division through the sclerotica is altogether antiquated. No one, now-a-days, uses a chisel-shaped needle, such as that employed by Sir William Adams, or attempts to slice the lens into pieces, as Mr. Jeaffreson directs. (p. 232.) A round-necked needle is the instrument employed by the operators of the present day, with which the anterior capsule is lacerated, so as to allow the aqueous humour to act on the lens, but without displacing the lens from its situation, or interfering with it in any way whatever. This is the method of proceeding in at least the first operation, for the cure by solution generally requires two or more operations. The absorption of the lens, then, goes on for a certain length of time. In about two months the operation is repeated, the capsule is again lacerated—especially if its fragments seem to have coalesced—and the lens, now become friable by the action of the aqueous humour, is also broken up. The fragments of the lens now dissolve more readily; and probably, in two months more, the pupil is clear.

In his preface Mr. Jeaffreson had already prepared us for something new and important on the cure of cataract by operation:

> "In the chapter on cataract the author has advocated an operation of the successful application of which he has had ample experience, and to the easier performance and superior success of it, as compared with the operations in general use, he especially wishes to draw the attention of his professional brethren." (Preface, pp. viii.)

At p. 226 we meet with another note of preparation: "In addition to these ordinary modes of operating, there is yet another method to which it is my earnest desire particularly to call the reader's attention. It is a modification of that by couching and solution."

At last the "mixed operation advocated by the author" bursts upon us. Can the reader guess what this "fourth method of operating" consists in, for which "I am hardly, perhaps, entitled," says Mr. Jeaffreson, "to claim for myself the merit of originality, although, as far as I know, this plan has never been advocated by any practical oculist?" (p. 234.) But it is fair to let the author speak for himself:

> "The instrument I use is an ordinary couching-needle, with a double cutting edge, as in the operation for solution. The needle being introduced as before described, when speaking of couching and solution, I first endeavour to cut up as much of the lens in situ, without disturbing it from its natural position, as possible. If the cataract be of the softer kind, it of course yields to the needle, and the operation then becomes simply that by solution; if it be so hard that it cannot be cut up, the capsule at least is freely lacerated, and then the point of the needle being raised, is made very gently to press the lens downwards to the extent of a few lines only, and just sufficient to admit of the entrance of a few rays of light. It rarely happens, however, that the cataract is so uniformly hard but that some portion of it at least may be thus cut up. The lens being held in this position for a few seconds, the needle is then carefully withdrawn. Belladonna should now be kept constantly applied to the orbital ridges, for the twofold purpose of freely admitting the access of the aqueous humour to the lacerated lens, and for facilitating the vision of the patient during the process of absorption." (pp. 234-5.)

Mr. Jeaffreson's operation, then, is this: he tries to divide the lens; but if he finds it too hard to be divided, he depresses it a little, so as to allow the patient to see over the top of it, and leaves it in this position to be acted on by the aqueous humour.

We consider Mr. Jeaffreson's operation objectionable in the first step he takes. If the cure of a cataract is to be effected by solution, there is no use whatever in attempting to cut the lens up; all that is requisite is to lacerate the capsule, and allow access to the aqueous humour. The cutting up of the lens, of which Mr. Jeaffreson is so fond, cannot be effected, if the lens is even only of ordinary consistency, without displacing it from the cavity of the capsule; so that it either falls forward into contact with the iris, or is pushed through the posterior capsule, somewhat into the vitreous humour.

After working away at the lens, then, for some time with his double-edged needle, "if it be so hard that it cannot be cut up," he depresses it a little. Now, in this new situation it will either dissolve or it will not dissolve. If incapable of absorption, it will only prove a source of irritation, and will require to be removed at some future time. If it dissolves, it would have done so had it been left in its natural place, with the front of the capsule merely lacerated. So far as the ultimate cure is concerned, we see no possible advantage to be gained by the semi-depression which Mr. Jeaffreson advocates. The only thing that can be said in its favour is, that it allows the patient a little sight from the first; but we should consider this advantage greatly overbalanced by the danger of the dislocated lens exciting inflammation, by its pressing on the iris or on the ciliary processes. If the operator attempts the cure of cataract by solution, we are of opinion that he has good reason to congratulate himself when he finds the exposed lens to keep its seat within the circumference of the capsule; and that, at the first operation, at least, everything should be most carefully avoided which tends to displace it, in any direction or in the smallest degree.

Chapter VIII. is of a miscellaneous nature, including *malignant diseases*, *mechanical injuries*, *artificial aids to vision*, and several other topics.

Fungus hæmatodes of the eye is described by Mr. Jeaffreson as "commencing probably in the expanded retina," (p. 265); whereas it sprouts from the papilla conica of the optic nerve, and has been found filling the whole cavity of the eyeball, while the retina remained entire.

"My own opinion," says Mr. Jeaffreson, "is very confident and decided that, in the case of fungus hæmatodes, occurring in early life, the operation [of extirpation], if performed without delay, may be perfectly successful. Many cases have occurred to confirm me in this opinion, one of which will be presently related; nor is it unsupported by many high authorities." (p 264.)

It would have been more satisfactory had Mr. Jeaffreson stated whether the "many cases" which confirmed him in this "opinion" had occurred in his own practice, or in that of others. As for the case of a Hindoo child which he relates (p. 271), and whose eye he extirpated, it is to be observed that the tumour was of a "brown and dusky hue," and that "no blood-vessels were visible on its surface." Now, the bright yellowish surface, strewed with blood-vessels, though it does occasionally occur also in non-malignant tumours, is, we believe, an invariable attendant on fungus hæmatodes.

It is also to be regretted that Mr. Jeaffreson has not given us the names of any of the "many high authorities" by whom the opinion is supported, that the operation, performed without delay, may be perfectly successful. We must confess we are unacquainted with them, and feel inclined, after a careful examination of the subject, to coincide with the judgments of Mr. Travers, Mr. Lawrence, and Professor Syme on this very important practical question :

> "I have extirpated the eye affected with medullary cancer in several instances; but I am not acquainted with any case in which the patient, who has survived two years, has not been revisited by the disease." (Travers.*)
>
> "Our present experience is very discouraging, and leads to the inference, that the operation, even in an early stage, is unavailing." (Lawrence.†)
>
> "In adding another instance of unsuccessful extirpation of the eye for medullary degeneration to the many which are already upon record, I cannot refrain from expressing my conviction, that it would be better, both for the interests of humanity and the credit of surgery, if this operation were entirely abandoned. ....If it could be proved that excision had been successful in a single well-authenticated case of medullary tumour of the eye, it would be wrong to refuse the chance of benefit from the operation. But if the result of numerous trials, under every variety of circumstances, has been uniformly unfavorable, it must be cruel to repeat the painful experiment any longer." (Syme.‡)

We are puzzled by Mr. Jeaffreson's "common enlarged sebaceous follicle," "frequently observed on either lid," and "readily removed by merely drawing a lancet or cataract-knife across it, and squeezing out its contents." (p. 275.) Is this the fibrinous tumour, or what is called *chalazion*, and which tends to the inner surface of the lid; or is it the albuminous tumour, called *molluscum*, which studs the outer surface; or is it *bonâ fide* encysted tumour, which the investigations of Sir Astley Cooper proved to be an enlargement of a sebaceous follicle? After opening it in the manner described, Mr. Jeaffreson says, "as much of the walls of the cyst as possible should be dissected out, or, by gently rubbing or squeezing the sides together, sufficient inflammation is induced to procure their permanent cohesion." Now, chalazion has no cyst. Molluscum, if allowed to grow very large, becomes so firmly adherent by its external surface to the neighbouring parts, that a layer of it is often left when the interior is squeezed out, and requires to be dislodged separately, which can always be effected by firm continued pressure. We have never seen encysted tumour in the eyelids, although it occurs frequently at the temporal extremity of the eyebrow. Its cyst, which Sir Astley Cooper showed to be an involution of cuticle, must be entirely removed; for if any portion of it is left, the disease will certainly be reproduced. No rubbing or squeezing the sides of it together will make them adhere.

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* Medico-Chirurgical Transactions, vol. xv, p. 239; London, 1829.

† Treatise on the Diseases of the Eye, p. 701 ; London, 1841.

‡ Edinburgh Medical and Surgical Journal, vol. xliv, pp. 6-7 ; Edinburgh, 1835.

In cases of a particle of coke fixing itself on the cornea, which is an every day railway occurrence, Mr. Jeaffreson gives the following advice:

> "Great gentleness should be used in removing it; and if it is so imbedded in the cornea as not to be readily discovered or easily displaced, time must be allowed for the subsidence of the irritation, and for it to be loosened by the incipient process of ulceration." (p. 284.)

That gentleness should be used in whatever we do to the eye, we readily grant; but we have never seen any case of a particle of coke fixed on the cornea which could not be readily discovered on placing the patient in a good light, or which could not be easily displaced by using a proper instrument. A cutting instrument, such as a penknife or a lancet, should never be employed for the purpose, as it is apt to remove, with the foreign body, part of the investing membrane of the cornea, while the slightest touch with the edge of a thin spatula generally makes it start from its place. To allow any foreign body to remain on the cornea till loosened by ulceration, we should consider very bad practice; for the irritation will increase from day to day, and may end in violent inflammation.

After remarking that the removal of blood from the anterior chamber by absorption generally goes on rapidly, Mr. Jeaffreson relates the following curious example of the contrary:

> "In one instance, however, in which I operated for cataract by extraction, a drop of blood was effused, and remained in the anterior chamber, which continued bright as arterial blood, and undergoing no change for several years. Sight was not entirely prevented by this drop of blood, (for it appeared as a single drop,) although slightly impeded Year after year I observed this deposition remaining, perfectly bright, in the anterior chamber; until at last nature, as if having previously forgotten or overlooked it, seemed to take upon herself the office of removing it, and then in the course of a few weeks it rapidly disappeared." (p. 288.)

The following also appears a curious fact, whether we suppose, with our author, that the iris was actually ossified, or only that, being deprived, by some morbid process, of its colouring matter, its striated and flocculent tissue presented an appearance as if it had been converted into bone:

> "In India I once had a case of ossification of the iris, in a female, of about thirty years of age, throughout its entire extent. The case was very interesting and curious: the iris was entirely converted into bone, fixed and immoveable, and the radiated structure of the bone was beautifully seen through the transparent cornea. I showed the case to many medical friends, who were much inte-

rested in its inspection; and I do not remember that any of them had seen a similar affection. The sight was but slightly impaired, nor could I trace any sufficient cause for the production of this singular phenomenon." (p. 289.)

Mr. Jeaffreson tells us that he is not aware that entozoa have been seen in the human eye by any English oculist; a confession which only shows that his studies in this branch of the subject have not been extensive enough.*

Mr. Jeaffreson describes myopia correctly; but we have never met with those individuals who, from some defect in the humours, cornea, or lens, are "possessed of a more than ordinary power of discovering distant objects." (p. 296.) Those affected with presbyopia, whom our author has here in view, do not see near objects well; but their power of discerning distant objects is just ordinary.

Mr. Jeaffreson's style is but indifferent. His language is sometimes ungrammatical, and his meaning not unfrequently obscured by faultiness of construction. He is particularly fond of separating the preposition from the noun which it governs, in order to connect different prepositions with the same word—a construction which is always inelegant. He also uses the parenthesis too often, particularly in the early part of his book, stuffing within it incidental clauses which had much better been omitted. The following sentences may serve to illustrate these defects:

"The system sympathizes more or less with the local affection, according to circumstances; chiefly to its previous state, to the degree and seat of the local affection; much, often, when the deeper-seated structures are affected; but little, generally, when the conjunctiva only is the seat of disease." (pp. 23-4.)

"The history of the case may also afford some clue; and the difference in the degree of reactive fever, greater in the former instance. (p. 85.)

"It now remains to be stated that, in a few instances, inflammation sets in with, or simultaneously affects several, not to say all, the structures of the eye." (p. 99.)

"The conjunctiva and sclerotic each exhibit various degrees of vascular congestion." (Ibid.)

"Acting in a manner somewhat similar to the effects of tumours, apoplectic seizure, or mechanical injuries, attended by the rupture of blood-vessels within the cranium, and the effusion of blood, if in such a situation as to cause pressure either on the optic nerves or the part of the brain into which they are immediately inserted, may induce amaurosis." (p. 174.)

"Previous inflammatory affections of the outer tunics of the eye, which have left nebulous or leucomatous patches on the cornea, or of the deeper-seated parts, which have gone on to the establishment of adhesions of the iris to the cornea or capsule of the lens, &c. may all militate against the success of, or positively forbid the propriety of operating, according to their degree." (p. 223.)

"I shall therefore beg leave to conclude my observations upon the diseases of the eye with a very brief notice of a few affections which have not hitherto been mentioned by me, and which may, I think, be either useful to the practitioner, or interesting to those who are more curious in the study of disease." (p. 285.)

No doubt! There are *affections which are useful to the practitioner*, but we can scarcely regard it as decorous, thus nakedly to announce the fact. It is as barefaced as if we were to say that the blunders of authors were useful to the critical profession, or that we found it to our advantage,

"With hatchets, scalping-knives in shape of pens,
To bid, like Mohocks, hapless authors die."

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* Mr. Logan's case of cysticercus cellulosæ in the anterior chamber is well known. We learn that in the month of April last, a similar case occurred at Mr. Guthrie's Eye Infirmary.

Art. XII.

Traité complet de l'Anatomie, de la Physiologie et de la Pathologie du Système nerveux Cerebro-spinal. Par M. Foville. 1re partie—*Anatomie.—Paris,* 1844.

A Treatise on the Anatomy, Physiology, and Pathology of the Centre of the Cerebro-spinal Nervous System. By M. Foville. First Part—*Anatomy.—Paris,* 1844. 8vo, pp. 676.

No portion of the human body requires such strict anatomical investigation as the brain and spinal cord; and in none has previous examination given greater difference in the result. We must therefore express our satisfaction at finding, in the still very imperfect state of our knowledge of the cerebro-spinal nervous centre, a book like M. Foville's, which attempts at least to add something to our stock of information.

Since the time of Reil, but few important additions have been made to his discoveries in the texture of the brain. The extreme difficulty of the inquiry deters many from entering on it; and it is the lot of but few to devote a sufficient number of years to the dissection of the hardened brain, to make their statements worthy of consideration. The author of the book before us has been occupied the greater part of his life with the study of the nervous centre; and he presents us, in this volume, a portion of the knowledge obtained from his labours, viz. the detailed anatomy of the brain and spinal cord in the adult, and the connexions of the nerves with that mass. Much as we approve of M. Foville's mode of proceeding, and greatly as we value his experience, we cannot consider some of his statements as altogether free from suspicion. To arrive at correct conclusions as to the structure and use of portions of the brain, we not only think that the aid afforded by comparative anatomy should be freely accepted, but we deem its aid almost essential. But M. Foville takes the opposite course, and considers it "necessary, before commencing those studies (of the lower classes), to be well acquainted with the knowledge of the most perfect type—that is to say, the nervous system of the adult man, in order to be able to make exact comparisons, and to arrive at correct conclusions." The correctness of such reasoning we consider very doubtful, and think the plan pursued likely to have led the author and to lead others into error. For it is in the lower classes that the complex forms of the higher are observed to be simplified, and as it were analysed; and difficulties that, at first sight, appear inexplicable, to be removed, by means of a series of successive steps, only gradually increasing in complexity as we ascend the scale of life. In the examination of the delicate structure of the brain, whose fibres are so interlaced that great judgment and dexterity are required in their separation, every assistance that may be obtained from other sources should be diligently sought, so as to leave no room for fallacy to creep in and invalidate the result. It is, unquestionably, much easier to rest satisfied with an examination of the human brain, than to repeat the same process in a great variety of animals, but much less satisfactory. In the case of the present author we cannot doubt that a more extended inquiry would have diminished the chances of hasty conclusion occasionally evident in this work. The author, for example, sometimes assumes that, because some fibres take a direction towards a certain spot, they most probably reach it. Such a supposition may doubtless contribute readily to solve some hitherto inexplicable theory; but if the disposition of those fibres were traced through simpler forms of development the examination might lead to very different and certainly much more trustworthy conclusions. Until the plan referred to is pursued, our knowledge of the connexions of parts of the brain and spinal cord must be imperfect; and the want of attention to it makes M. Foville's treatise incomplete.

Few of our readers, we think, will consider our remarks misplaced, concerning the value of the aid to be derived from comparative anatomy, when they learn the views propounded by the author. So great is the novelty of M. Foville's views, that many results previously received as established, are rendered doubtful by his examinations. For instance, among other novelties, he makes the following announcements: That the three divisions of each half of the spinal cord—viz. the anterior, middle, and posterior fasciculi—contribute almost equal portions to the cerebrum; and that the posterior fasciculus or restiform body, united by an offset to the cerebellum, may be traced through the cerebrum as the axis to which the nucleus of the hemisphere is attached, and may be followed to the anterior perforated spot, or "fundamental part" of the hemisphere. That the corpus callosum is derived chiefly from the same prolongation of the restiform body. That the hemispheres of the cerebrum are united by a commissure at the base, extending from the infundibulum to the apex of the pyramids, &c.*

This volume is, what it professes to be, a complete treatise on the anatomy of the brain and spinal cord, the centre of the cerebro-spinal system of nerves: it is preliminary to the drawing of conclusions respecting the function of these parts. For the views of the author on their physiology and pathology, we must wait for the publication of a succeeding volume. An active spirit of inquiry has evidently directed the investigations of the author: everything appears to have been personally examined. M. Foville states his views boldly, as a man who entertains no doubt of their accuracy; and we are sure that he wishes them to be examined carefully, and criticised with candour.

The author treats, first, of the anatomy of form of the spinal cord, cerebrum, and cerebellum; and then considers their structural anatomy. A dissertation on the origin of the cranial and spinal nerves follows next in order; and this portion of the book closes with the membranous investments of the cord and brain. An interesting inquiry is then entered into concerning the correspondence of the spine and cranium with the form of the inclosed masses, reference being made to the locality of the convolutions of the brain; and artificial regions are proposed, to determine with more precision the position of those convolutions. Accompanying the volume we have an atlas of twenty-three plates in lithography; the designs by Messrs. Beau and Bion, and the lithography by Artus. The delineations of the convolutions on the surface of the bemispheres are excellent, but others of the medulla oblongata, &c. are very stiff, owing to the previous immersion in spirit of the parts from which they are taken. The execution of the lithography is good throughout. We must not, however, be misunderstood, as recommending the atlas to those who wish to learn the common opinions on the anatomy of the brain: the plates are designed to explain the views of the author, not to serve to instruct the beginner in his studies.

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* With all the candour that M. Foville has shown in his historical notice, we think he has scarcely given a due measure of praise to Reil, who certainly, as will be afterwards seen, preceded our author in many of his dissections of the cerebrum and cerebellum.

The spinal cord. The following remarks comprise the chief of M. Foville's views on this portion of the nervous centre. The three divisions of each half of the cord—viz. the anterior, middle, and posterior fasciculi, extending the whole length—increase in size superiorly, and constitute the medulla oblongata. In certain spots they differ in volume, the enlargement of the cord being occasioned by the greater size of one or other. Thus, in the cervical enlargement, from which the nerves of the cervical plexus are derived, the posterior and lateral fasciculi are the largest; in the brachial it is the posterior fasciculus that preponderates; whilst the greater size of that part of the cord, to which the lumbar nerves are attached, depends upon an increase in the lateral fasciculus. Superiorly, the lateral fasciculus increases in size. To it, on close inspection, may be seen attached along the line of the anterior roots of the nerves, "a great number of finer roots, proceeding from the contiguous part of the lateral fasciculus, and uniting with the former."* In the fetus it is said this lateral division of the cord is divided unequally into two by a gray semitransparent line on the surface; the smallest portion, called "accessory," is directed to the cerebellum, but the larger, the continuation, reaches the cerebrum. Fissures have been supposed to occupy the lines of attachment of the roots of the nerves, marking the limits of the fasciculi or divisions of each half of the cord; but M. Foville says there is not any fissure exteriorly in those spots, and that the roots of the nerves enter the cord by small apertures in a thin medullary fibrous layer on the surface. When this layer is torn through the fissures are exposed. For a detailed account of the minute structure of the cord we refer the reader to the work, p. 281. The attachments of the nerves will be noticed hereafter.

The commissural or connecting piece of the halves of the cord is seen by opening the anterior and posterior median fissures. It consists of a central gray part, covered before and behind by a medullary layer. This central part M. Foville considers the commissural part of the cord, that combines in action the opposite sides of the body. The layer of medullary substance in the posterior fissure is the posterior commissure, which serves as a bond of union between the posterior fasciculi. The stronger layer in the anterior fissure is the anterior commissure of the cord; it is "formed by a layer of white substance, hollowed in its whole length by very numerous small transverse fissures, very near one another, but which, it is very remarkable, exist only on the sides of that commissure, without encroaching upon the middle line, which presents all along a small crest or kind of very slightly-marked *raphé.*" A depression is also seen on each side, and, when the fissure is widely opened, the transverse surface of the commissure, which passes from the one to the other half of the spinal marrow. Where the cord ends above in the medulla oblongata other fibres are seen to cross the anterior fissure, and connect the opposite halves of the cord: this appearance, known by the term *decussation of the pyramids*, is considered by M. Foville as but a part of the great anterior commissure, with its fibres more strongly marked and more evidently digitating. The author supports his views of the importance of this connecting portion of the cord, by the following remarks, which we extract:

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* In a subsequent note at p. 530, the author throws some doubt on this statement. He says: "I have not been able to repeat my inquiries on this point frequently enough to convince myself of the existence of lateral nerves in the lumbar enlargement of the cord."

> "But what is the texture of this anterior commissure? Does it consist solely of fibrous laminæ, extended from right to left, to unite the two halves of the organ to which it belongs? Or does it, on the contrary, serve the purpose of a decussation of the one side with the other? This last opinion has been often suggested by the inspection of the parts, and not less often refuted by a physiological objection—the absence of all signs of decussation in injuries of the cord. Nevertheless, the question does not appear to me definitively decided. Firstly, the simple inspection of the anterior commissure is calculated to suggest the idea of a decussation between the two halves. Secondly, if this decussation exist, it should be between the anterior fasciculi, which unite so freely with that commissure; and an attentive inspection of those fasciculi seems to me to favour the opinion that they decussate, without interruption, from the top to the bottom of the cord." "Another reason which would lead us to admit the decussation of the anterior fasciculi of opposite sides of the cord, through the means of the anterior commissure, is the following: It is not uncommon to see the anterior fasciculi divided, at the top of the cord, into two or three portions. If the most internal of those portions is followed attentively, it is seen, after a course of greater or less extent, to end at the border of the anterior median fissure, where it terminates in a point, which penetrates obliquely into the corresponding wall of that fissure. Does not all this render probable the decussation between the anterior fasciculi of the spinal cord? But to appreciate the force of these reasons it is first necessary to consider the arrangement of the decussation of the pyramids, admitted by the majority of anatomists. We shall see further on that this decussation of the pyramids is but the continuance of a system of decussations, which commences between the cerebral peduncles, behind the corpora mamillaria, and is prolonged uninterruptedly to the pyramids." "Finally, all conjecture cannot supply the place of demonstration. The microscope seemed designed to render here a new service to science. Dr. Gruby has readily undertaken, at my request, the examination of those parts in which I suspected decussations. He has seen them (decussations), has made me see them; and he is moreover occupied in delineating them, to illustrate a description in which he is engaged." (p. 294-7.)

Following the order observed by the author, we take next the upper part of the cord or the medulla oblongata, the pons, and peduncles or crura of the cerebrum; all which intervene between the spinal cord and the hemispheres of the brain. To the anatomist and physiologist the investigation of these parts is most necessary and interesting, because of the present uncertainty of opinion respecting the disposition of the fibres, or the origin of the important nerves connected to them. We must confess we looked most anxiously through the pages containing the description of the above-named parts, and hoped to find (not that we in the least undervalue additional research,) announcements corroborative of our commonly received opinions, so that we might presume with something like confidence that we possessed a standard of knowledge. Our disappointment may be judged of when it is said, that the dissections of Foville unsettle our former supposed foundations, and render necessary farther examination. Much, certainly, do we wish that some one equal to the task, whose skill and accuracy are unquestionable, would enter on this difficult field of inquiry. In opposition to the received opinions, M. Foville says that all three fasciculi or divisions of each half of the spinal cord are continued to the cerebrum, the posterior or restiform body supplying only an offset to the cerebellum; that each, in passing the pons, contributes transverse fibres to its formation; and that the prolongations of the three, placed side by side, constitute the peduncle of the cerebrum. In this difficult and disputed region of anatomy we could wish more unexceptionable and illustrative plates than the author has given us, of the course in the brain of the prolongations from the three fasciculi of the spinal cord. The method we shall take is to follow uninterruptedly through the medulla oblongata, pons, and peduncle, the fibres continuous with either the posterior, anterior, or middle division of the cord, till they are about to spread out in the great or little brain: their distribution in the hemisphere will be subsequently mentioned.

The medulla oblongata, pons, and peduncle of the cerebrum. In these parts, placed in a line, in the order mentioned, are recognized continuations of the three fasciculi that constitute each half of the spinal cord. But the simplicity observable in the cord is much interfered with in the medulla oblongata.[*] Occupying the middle line in their whole extent is a commissure corresponding to the anterior of the cord.

The following is the best digest we could make of the author's opinions.

The posterior fasciculus of the spinal cord, known by the name of restiform body in the medulla oblongata, joined by the "accessory" portion of the lateral fasciculus, gives, at the lower border of the pons, some superficial transverse or commissural fibres to constitute the lower part of the pons, others appearing as a fibrous band on the surface between the fifth nerve and the portio dura of the seventh pair. Afterwards the restiform body divides into a posterior or cerebellar, and an anterior or cerebral portion. The posterior constitutes the inferior peduncle (Reil) of the cerebellum. The anterior or prolongation to the cerebrum, placed internal to the transverse fibres from the cerebellum to the pons, is then found to form successively the lateral boundary of the fourth ventricle and aqueduct of Sylvius, the inner part of the optic thalamus, and, finally, a portion of the lamina cribrosa, or *substantia perforata antica*, at the inner part of the fissure of Sylvius. Between the lower and upper borders of the pons, retrograde fibres are given from this fasciculus to the auditory and fifth nerves, also others deeper and transverse to the upper border of the pons. Beyond the pons this prolongation forms the part of the crus cerebri above the external depression, or, as Foville expresses it, "In other words, with the exception of some fibrous layers which descend into the lateral or the anterior fasciculus, all seen in profile behind the fasciculated region (lower half) of the cerebral peduncle, is continuous with the posterior fasciculus of the spinal cord."

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[*] M. Foville considers the peduncles of the cerebrum but one part, "*le tronçon pédonculaire*," which consists of a right and left portion, united across the posterior perforated spot by decussating fibres.

The lateral fasciculus consists of two parts, as before said; the small "accessory" being directed backwards beneath the superficial roots of the auditory nerve, and the other continued upwards to the cerebrum. It is in newly born infants that the accessory portion is seen. The course of the fibres of the cerebral portion is by the pons, where they are placed above the transverse fibres, and through the crus cerebri, as high as to the corpora albicantia. At the last spot the fibres end in an enlargement, resembling the half of the head of a club-stick slightly curved, from which fibres radiate to the cerebrum. In the crus they occupy a position between the prolongations from the posterior and anterior fasciculi. Opposite the lower half of the pons a large bundle is given to the deep transverse fibres of that body; and opposite to the tubercula quadrigemina, the *processus e cerebello ad testes* (superior peduncle—Reil,) joins the lateral fasciculus, and the offset from the tubercles to the olivary body traverses the fibres. Internally the fibres take part in a commissure to be presently noticed.

The anterior fasciculus, continued upwards as the anterior pyramid of the medulla oblongata, is described by anatomists as passing through the pons, and receiving in it additional fibres, to which are due its enlargement and conical form. But M. Foville states that this view rests on a theory of Gall, and is not supported by an examination of the structure of the parts. He attempts to show that the fibres are not added, but that the conical form results from fibres ceasing, from the base of the brain to the pyramid, along the middle line. In the crus, the prolongation from the anterior fasciculus of the cord occupies the lower or fasciculated region (the part below the depression on the outer side), whilst above it are the continuations of the posterior and lateral fasciculi, the last separated by the locus niger of Soemmerring. The following is the disposition of the fibres in their course from the hemisphere to the cord. In the crus they are inclined obliquely inwards from the hemisphere, the most internal ending in a commissure that occupies the middle line of the crura. The fibres that enter the pons, also decreasing in number from the upper to the lower border, are directed inwards, sometimes minutely divided, to the same median commissure, and assist in forming the transverse fibres of the pons. In the medulla oblongata the fibres join in the middle line, by means of the decussation of the pyramids, with the opposite half of the cord. So the number of fibres decreases downwards in the crus, pons, and pyramid, the strength of the intermediate commissure diminishing in the same degree.

Of much importance, in the opinion of the author, is a decussation at the base of the brain, or the median commissure that unites the "opposite halves of that portion of the axis of the nervous system comprised between the base of the cerebrum and the union of the medulla oblongata with the spinal marrow." It extends from the decussation of the pyramids to the infundibulum at the base of the brain, being, as it were, a continuation upwards of the great median commissure between the halves of the cord. Opposite the upper border of the pons it is most marked, and diminishes in strength from that point both upwards and downwards. To expose the commissure, take the base of a brain after the cerebrum and cerebellum have been divided so as to bring into view the third and fourth ventricles, and attempt the separation of opposite sides in the middle line of the fourth ventricle. Proceeding carefully, the separation is easy between the lateral fasciculi in the medulla oblongata, but impossible opposite the thickest parts of the pons and the fasciculated region of the crura cerebri. Between the lateral fasciculi the separated vertical surfaces are somewhat smooth, and present prominences with intervening depressions, the projections of one side fitting into the pits of the other. In the region of the crura cerebri is found a complete decussation, the fibres of one peduncle—say from the right—passing to the left, and *vice versâ.*[*] After decussating in the middle line, the fasciculated fibres of the crus cerebri are continued into the lateral fasciculus of the side opposite to that from which they proceed, resembling the decussation of the pyramids across the median fissure of the cord. What takes place in the pyramids is repeated in the whole length of the commissure, that is to say, from the summit of the pyramid to the infundibulum, except that the fibres opposite the pyramids are stronger than the remainder. From what has been said, the destination of the fibres in the crus cerebri may be understood: the oblique fibres on the under surface of the crus cerebri, directed to the middle line, are continued into the fibres of the lateral fasciculus of the opposite crus; whilst the more external fibres of the same surface, that reach inferiorly to the decussation of the pyramids, are connected with the anterior fasciculus of the cord of the same side, as well as with the lateral fasciculus of the opposite side.[†]

The author terminates the anatomy of the medulla oblongata, pons, and peduncle, with the following summary of the whole:

"In conclusion, three longitudinal fasciculi—viz. the posterior, lateral, and anterior of the cord—are found in the portion of the nervous centre that joins the spinal cord to the brain; the last having preserved its anterior position, the posterior having become external, and the lateral fasciculus posterior to the level of the floor of the ventricle of the cerebellum. A superficial part of the lateral fasciculus passes into the cerebellum.

"Some fibres disposed in transverse arches are also met with. The fibres with this direction are not alone those of the pons; to the same order belong the arciform fibres of Rolando, disposed in arcs of circles upon the side of the medulla, the concavity above, which join the pyramid and olivary to the restiform body, and contribute to the origin of many nerves. A considerable commissure decreasing from above. Lastly, two layers of fibres occupy the vertical median surface.

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[*] Here is an instance in support of the assertion, that M. Foville passes over Reil's merits. With this extract, and a future reference, it will be seen that Reil has completely noticed the appearances, though he does not state that the fibres on the under surface of the crus are derived from the prolongation of the lateral fasciculus of the opposite side. Reil, in treating of the fourth ventricle and the large bundles of the lateral fasciculi in the floor, says: "One other layer—the vertical fasciculi—composed, not of nervous matter alone, but of cellular membrane likewise, and vessels in unusual proportion, as may be conjectured from its toughness, extends from behind the pyramids, behind the upper transverse fibres of the annular protuberance, and below the ansa of the anterior peduncles of the cerebellum, to the gray substance which is found between the crura cerebri, extending beyond the corpora albicantia; thus building the floor of the third ventricle, and becoming continuous with the infundibulum. The fibres of this structure seem in some degree to decussate each other." (Mayo's translation of Reil's Commentaries. Part ii.)

[†] *Note of the author.* "This system of decussations, suspected by Rolando, who denied that of the summit of the pyramids, has been partly seen by Valentin, and recognized by M. Longet, to whom we had the pleasure of communicating it. No author hitherto has represented it altogether as it is described in this work, and has been made known some years by us to the Academy of Sciences and of Medicine."

"The posterior fasciculus, become for a time external, is prolonged under the tubercula quadrigemina, the optic thalamus, and the perforated quadrilateral space. A considerable part of each fasciculus enters the cerebellum, where it will soon be followed. Other offsets from this fasciculus belong to the transverse fibres of the pons.

"The anterior fasciculi unite the anterior parts of the cord with the centre of the base of the cerebrum, into which they penetrate by a sort of ring, formed by the posterior fasciculus united with the optic thalamus and the perforated quadrilateral spot.

"The lateral fasciculus, continued into the hollow of the crus cerebri above the locus niger, does not cease altogether in that spot; its prolongation into the cerebrum will be afterwards described.

"The transverse fibres (of the pons) are offsets from the posterior, lateral, and anterior fasciculi. They combine with all those fasciculi, also with the portion of the peduncle of the cerebellum annexed to the protuberance. We shall return to the transverse fibres of the cerebellum. It suffices here to point out that they contribute to form the ring of the protuberance.

"The commissure contains decussating fibres proceeding, in great part, from the fasciculated region of the crus. These fibres enter, after their decussation, the median vertical surfaces; those from the right crus appertain to the vertical surface of the left side, and reciprocally, those from the left crus proceed to the vertical fibres attached to the right lateral fasciculus." (p. 325.)

After an attentive consideration of the views of the author, let us pass in review his statements respecting the anatomy of the portion of the nervous centre between the hemispheres of the brain and the spinal cord. Regarding this part of the nervous system as the most important, and considering a correct knowledge of its anatomy necessary to the understanding the dependence of the various portions of the brain, it behoves us to receive with great caution all doctrines that introduce great changes. It is in this spot that the fibres seem to be rearranged before being developed into the hemispheres and convolutions, so that in all examinations of the brain it would appear necessary to start from it as from a centre or link between the cord and brain. As M. Foville's statements are supposed to be founded on observation, it must appear rash to give an opinion opposite to that of the author, unless the same is based upon facts obtained by dissection. Where it will be necessary to differ we shall not dissent without much hesitation, even though our opinion is supported by other anatomists and experience.

According to Foville, the posterior fasciculus of the cord (restiform body) enters as largely into the structure of the cerebrum as into that of the cerebellum. If the matter is examined anatomically, it appears that the restiform body is connected to the pons, giving probably a few superficial fibres to it, as described by Tiedemann, and ends in the inferior peduncle of the cerebellum, as so ably traced by Reil. Foville's assertion can, in our opinion, be explained only by presuming that some of the fibres of the lateral fasciculus, which spread out in the floor of the fourth ventricle, and are closely connected with the restiform body, have been traced to the cerebrum as a prolongation from the posterior fasciculus. That this is the explanation of the origin of those fibres followed to the hemisphere is rendered more probable by the statement of the author, that, with the exception of a few fibres, "all the part of the crus cerebri, seen in profile above the groove on the outer side," belongs to the prolongation from the posterior fasciculus. Now it must be so evident to all, that the portion of the crus referred to is formed by the superior peduncle of the cerebellum or prolongation from the cerebellum to the cerebrum, that we should infer some oversight, did not the author consider the superior peduncle of the cerebellum to perforate the prolongation of the posterior fasciculus, and join the lateral fasciculus. It is easy, in the dissection of hardened brain, to detach the superior peduncle of the cerebellum from the prolongation of the lateral fasciculus on which it lies, and to follow it backwards to the cerebellum or forwards to the optic thalamus in which it expands, as described by Reil; and the knowledge thus obtained is opposed to Foville's view of the peduncle perforating the posterior fasciculus, and joining the fibres of the prolongation of the lateral fasciculus opposite the tubercula quadrigemina.

The lateral fasciculus (made of comparatively little importance in the production of the cerebrum, since it is said to end at the upper part of the crus in a rounded head, from which few fibres again radiate, but without direct continuation,) appears to be limited in lateral extent by the author, and for the reason above mentioned, viz. the assigning a portion of its fibres to the posterior fasciculus. The fibres are described as uniting in a commissure in the middle line, which is strongest opposite the upper border of the pons. The arrangement of the commissural surfaces was described by Reil, without his naming the appearance, a commissure. We noticed before in a note that Reil observed the extent and disposition of the vertical fibres, and we give now an extract from the same author, to show that he was also acquainted with those fibres that cross in the middle line. He describes them as resulting from a union of the superior peduncles of the cerebellum. He says: "The peduncles now plunge downwards, forwards, and inwards into the cylinder, (the portion of the crus cerebri above the locus niger,) having above the tubercula quadrigemina; above and without, the fillets; within, the vertical fasciculi; below the latter they unite by means of an *ansa*, which is several lines in thickness, and forms the upper wall of the foramen cæcum; it is a question whether a complete continuity or [an ?] anastomosis occurs here." We have examined the commissure according to the method recommended by Foville, but are inclined to the opinion that Reil's description is correct, viz., that at the posterior part, where the surfaces are vertical, it is a question whether there is an interchange of the fibres of opposite sides; and that the fibres, crossing the middle line, are part of the superior peduncle of the cerebellum, forming the "ansa" of that author, and not fibres from the prolongation of the lateral fasciculus whilst contained in the crus cerebri.

Our experience is likewise opposed to the opinion of Foville, that the most internal fibres of the under surface of the crus, directed obliquely inwards and continuous with the anterior fasciculus of the cord, are derived by decussation from the crus of the opposite side. An examination demonstrates that those fibres do not cross the locus perforatus in the middle line, but wind backwards, round the inner part of the crus, to the projection in the floor of the fourth ventricle, remaining on the same side of the body.

In figure 1 of plate v, the pons is represented as having three distinct canals in the transverse fibres, to allow the passage of the prolongations of the three fasciculi of the cord. Those are said to be produced by the transverse fibres continued from each fasciculus to the pons; but the appearance is rather singular, when it is remembered that the prolongation of the lateral fasciculus is, and that of the posterior would be above or behind the transverse fibres from the cerebellum to the pons.

The cerebrum. Whilst following the author through his investigations of the anatomy of the brain, we shall select only those parts that are considered by him of greater consequence. With the general form and divisions, or the external anatomy, there is little to interest, except the classification of the convolutions. Much importance is given by the author to the fissure of Sylvius, and the perforated spot at its inner termination. These last two are considered "fundamental" parts in the human brain, and as the greater or less development of them distinguish the brain of man from that of beings lower in the scale, their examination cannot be void of interest.

Convolutions. Since the assertion of Gall, that the different faculties of the mind are located in the convolutions of the cerebrum, and that, according to the condition of these manifested by the form of the exterior of the skull, the mental endowments of an individual can be recognized, much attention has been very justly given to the study of the form of the brain. So weighty a subject has not escaped our author, and the opinions of one who has so extensively studied the brain deserve our most attentive consideration. What is advanced by the author has been obtained from observation of the brain, and is the more worthy of credit, because it is not brought forward by a partisan in support of a particular theory; for there seems a greater desire to record facts than to gain any triumph over an opponent. The author is not favorable to the opinion of the convolutions on the surface of the brain being either very numerous, or similar on the opposite hemispheres. Nor does he consider them always proportioned to the size of the brain; the extent of surface being sometimes inversely as the mass of the hemisphere. He asserts that the idea of the possibility of the convolutions being "limited, named, and traced on the surface of the cranium by a sort of chart," is altogether opposed to anatomical investigation. "There are long lines of convolutions uninterrupted on the surface of the brain, groups distinct from other groups; but these lines and groups merge in many points into the contiguous parts by direct union of the convolutions." Analogy, moreover, is opposed to the supposition that a single part serves to constitute a number of different organs, for it is found that a combination of different parts is necessary to produce one organ. The convolutions, too, are folds of one membrane, the cortical substance, which is plicated like the processes of a serous membrane, and is continuous like it over the surface. In many animals there are no convolutions; and in a hydrocephalic head, the convolutions may be unfolded by the fluid, the exterior presenting a smooth surface. Into the question of phrenology the author does not enter fully in this volume; but he considers its accuracy doubtful in the present state of our knowledge. An attempt is made to arrange the convolutions in four classes, and to ascertain not only their composition and dependence on the deeper structures, but their correspondence to certain known parts of the skull. To accomplish the latter purpose, the skull is marked into regions with great care and minuteness; but we must refer those who wish to ascertain the facts bearing on phrenology to an examination of the work. We cannot do more than give an abridgment of the writer's answer to the question: Does the form of the cranium point out the form of the brain? Only in a general way, viz. supposing the brain and skull natural, a long cranium cannot contain a short brain, nor a short skull a long brain. The same may be said of the general form of both; but in proportion as we inquire into the correspondence of individual convolutions with their containing case, so is precision and certainty destroyed. Thus a large forehead shows only a large frontal development of the cerebrum. Indeed the variable thickness of the skull, and the difference in the size of the frontal sinus, prevent the accurate determination of the size or outline of the underlying parts of the brain. In two individuals who have the same prominence and size of forehead, the same thickness of skin and bone, the same depth of frontal sinus, the dimensions of the frontal lobes may be very different. Foville says farther: "I do not doubt the possibility of ascertaining, with some degree of accuracy, by an examination of the skull during life, the relative development of the principal regions of the brain: I go farther, and think that, within certain limits, the physiological signification of the forms of the human head can be determined. But that science (phrenology) is not established, and it can be safely based only on exact anatomical knowledge." This opinion of Foville, who has practically investigated the subject, will, we trust, induce others to enter the same field of labour. We have long felt the views of phrenologists to be imperfect, because they have been supported chiefly by pathological facts and ingenious argument, instead of being founded on anatomical knowledge. Are not conclusions as to the functions of the brain, deduced from an examination chiefly of the skull, likely to be fallacious, when so little is certainly known of the contained organ, as regards either the position and extent of the convolutions, or the precise connexions of the component parts? Till those facts are ascertained, we cannot hope for any accurate acquaintance with the organ of the mind, and we would, therefore, urge upon inquirers to make the brain rather than the skull the source from which to obtain knowledge.

These are the four orders of convolutions in each hemisphere distinguishable one from another:

1. In the first order there is but a single convolution, the convolution of the band* (*circonvolution de l'ourlet*) or convolution of the corpus callosum of Reil. It surrounds the hemisphere like a riband, is fixed by each end to the perforated spot, and is divided into a large or vertical and a small or horizontal portion. The vertical portion commencing at the front of the perforated spot, and supporting the olfactory nerve, turns upwards round the front of the corpus callosum; placed on the upper surface of this body it bends round the posterior fold, and finally skirts along the crus cerebri, giving rise to part of the fissure of Bichat, to end

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* Beneath the convolution is a small band of white fibres, similar to a hem in the border of a cloth; hence the name of convolution of the hem or band. The same is the covered band of Reil.

in the same perforated spot. The horizontal portion, situate over a small fibrous arch (*cintre fibreux*), is attached in its whole length to the outer part of the perforated spot, and unites the extremities of the vertical portion. To the outer border of this portion are connected the convolutions of the island of Reil, and that of the olfactory nerve.

2. The convolutions of the second order are two: one, the largest, occupies the circumference of the hemisphere; the other surrounds the island in the fissure of Sylvius. The largest commences at the base of the brain, where it is fixed to the horizontal portion of the convolution of the first order, and is internal to the groove for the olfactory nerve; at the front of the brain it turns upwards to the highest part of the hemisphere, separating the outer from the inner surface, courses round the posterior lobe, and ends in the inferior, between the convolution of the band and that of the fissure of Sylvius. The remaining convolution, connected at its origin to the other, surrounds the fissure of Sylvius, giving rise to three lips or borders, occasioned by two considerable indentations; it is united to the horizontal part of the convolution of the band by the gray substance of the island of Reil.

3. The third order of convolutions unites the first and second orders; so that some pass from the great portion of the convolution of the band to the large convolution of the second order, and others, from the horizontal or attached portion of the same convolution to that of the fissure of Sylvius. The first set are seen on the inner surface of the hemisphere, and on the posterior and inferior lobes. The other set form the island of Reil, and occupy the fissure of Sylvius.

4. To the fourth order belong all the convolutions on the outer surface of the hemisphere and under part of the anterior lobe; they intervene between the two convolutions of the second order, that is to say, between the one of the fissure of Sylvius and that separating in the middle line the inner from the outer surface of the hemisphere. These convolutions will increase or diminish in length as the convolutions they connect approach at their commencement and termination; some are divided in their course. For a full description consult the work, p. 217, et seq.

From what has been said of the four orders of convolutions, it will be seen that only the convolution of the first order is united directly to the perforated spot; those of the second order being joined to the horizontal or attached part of the first, whilst those of the remaining two orders do not reach that spot. It is this spot that Foville considers the fundamental part of the brain,—the source of the convolutions and certain parts of the cerebrum, the point of termination of the prolongation to the cerebrum of the posterior fasciculus of the cord, and from which arise the olfactory and optic nerves. To support what we should call a theory, the author shows an anxiety which is not commendable. Accordingly, though the third and fourth orders of convolutions do not reach the perforated spot, as the third is connected to the first order, and the fourth "may be considered a sort of prolongation from the convolutions of the third order," they must, he thinks, be allowed indirectly to reach it. Such reasoning, if allowed to be substituted for that demonstrative evidence that the examination of the brain requires, will be most likely to lead into error, and to make of little value any of the conclusions. Much good will no doubt arise from a classification of the convolutions; for though we are not prepared to pronounce perfect the plan followed, the attempt will serve to point the direction of future inquiry, and assist in other arrangements.

Fissure of Sylvius. A minuteness of description is the chief novelty of this part. The fissure occupies, as is well known, a transverse position at the base of the brain, from which it is continued upwards and backwards, and it is remarkable in circumscribing the convolutions of the island. Narrow at its inner termination, where it presents the perforated spot, it enlarges, and finally ends in three borders or lips, a superior, anterior, and posterior, which are folds of the convolution of the second order before noticed. Corresponding to the space inclosed by the lips of the fissure, or to the island, is the nucleus of the hemisphere, viz., the corpus striatum and optic thalamus, situate on the crus cerebri. In the human brain the tip of the posterior border reaches in front of the anterior. In no other brain does the posterior lip pass the limits of the anterior; and in proportion as the animal is higher or lower in the scale does the convolution approach to a complete circle or recede from the same.

The *perforated quadrilateral spot** (*espace ou quadrilatère perforé*) is considered by Foville the fundamental part of the cerebrum; and to it, as will be seen, are connected many important structures of the hemisphere. According to the views and description of the author, this space has been hitherto much neglected by anatomists. Its situation is at the inner part of the fissure of Sylvius, anterior to the crus cerebri. To it the olfactory and optic nerves are connected, and from it seem to radiate, as from a centre, the convolutions of the surface of the brain. The limits of the space are, the optic tract behind; in front, the convolution with which the roots of the olfactory nerve unite; internally, the border of the hemisphere; and externally, the small or horizontal portion of the convolution of the band. Crossing from the outer to the inner part of the space is a whitish layer† which divides it into two, gray matter being found both before and behind it. The collection of gray matter behind is connected with the commissure of the optic nerves, closing there an interval, and is analogous to the gray root of the olfactory nerve; the gray matter in front of that band joins the roots of the olfactory nerve. The proportion between the size of the perforated spot and the surrounding convolutions marks the degree of importance of the brain. Thus the more the brain enlarges and swells laterally around the peduncle, the more the perforated spot is hidden, until in man it is the roof of a cavity at the base of the brain. As the cerebrum decreases laterally, the spot becomes superficial, and may equal in size the base of the hemisphere. The same inverse proportion is manifested between the perforated spot and the island of Reil; the one enlarging as the other diminishes. In this portion of the hemisphere terminates the prolongation from the posterior fasciculus of the spinal cord; but its consequence as an elementary part of the brain will be referred to when treating of the structure.

Structure of the cerebrum. The cerebrum consists, for the most part, of fibres that are directed from the centre to the convolutions on the surface; from the exterior to the middle line; or from the front to the back of the brain. Two chief sets of fibres have been considered to constitute the bulk of the brain: One, the diverging system, representing a cone (fibrous cone of Reil), whose apex is in the crus cerebri, and base at the circumference of the hemisphere. The other, the converging or commissural system, of which the corpus callosum and anterior commissure are the principal, united in the middle line, to combine in one whole the opposite halves of the brain. Foville undertakes to show that this view is incorrect. He asserts that only some of the convolutions receive diverging fibres from the peduncle, the others being supplied from the circular fibrous band* (*ruban fibreux de l'ourlet*) beneath the convolution of the band, which surrounds the root of the hemisphere. He also adopts the view, which considers the corpus callosum derived from the base of the brain and parts constituting the nucleus of the hemisphere, instead of from the convolutions on the surface. In the description of the convolutions, it was stated that the first and third orders were united, and that the second and fourth formed another system. Now, it is to the second and fourth orders on the outer surface of the brain that the diverging fibres of the peduncle reach; whilst to the first and third are continued fibres from the fibrous circular band around the root of the hemisphere. Let us trace shortly the arrangement of each set of fibres.

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* *Substantia perforata antica* of anatomical works; *lamina cribrosa* of Reil; and *espace perforé* of Vicq d'Azyr.

† This is described by Reil as the fillet of the lamina cribrosa. (Op. cit.)

Peduncular or *radiating fibres.* In our notice of the crus cerebri, the prolongations to the cerebrum of the three fasciculi of the cord were left to be afterwards taken up. Before tracing these farther, it may be well to premise that each hemisphere consists of a central part or nucleus, the continuation of the cord, and of an enveloping portion connected to the other by fibres. Through the central part, consisting of the corpus striatum and optic thalamus, course the fibres of the crus or peduncle, dividing those two bodies into an upper or ventricular, and a lower or extra-ventricular portion.

The continuation of the posterior fasciculus of the cord, forming the upper part of the crus cerebri, is prolonged forwards by the side of the aqueduct of Sylvius, giving rise to the roof of the aqueduct and to the posterior commissure, and bifurcates opposite the optic thalamus. One portion, the radiating, passes through the thalamus, gives fibres to the optic nerve and corpus callosum, and ends at the outer border by joining the radiating fibres of the middle fasciculus. The remaining part (*partie centripète*) is continued along the third ventricle to the anterior commissure, where it likewise divides, an offset ascending to the lateral ventricle as the tænia semicircularis, the continuation entering the perforated spot below the commissure, joins the fibrous arch (*cintre fibreux*) of that region, and gives attachment in front to the olfactory and behind to the optic nerve. This centripetal portion of the posterior fasciculus is a sort of axis to which the nucleus of the hemisphere is attached; for the corpora quadrigemina, optic thalamus, tænia semicircularis, and corpus striatum, receive prolongations from it.

The portion from the middle fasciculus, occupying in the crus a position intermediate between that from the anterior and the posterior, is joined by the superior peduncle of the cerebellum, and ends, as before said, in a rounded projection near the optic thalamus. From this prominence arise other fibres, of which a few turn forwards to the perforated spot, the remainder enter the nucleus, and emerging on the outer side, join the radiating set to the convolutions.

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* Covered band of Reil.

The prolongation of the anterior fasciculus, stronger than the others, radiates to the outer side of the nucleus, the fibres being arranged like the half-opened tail of a peacock, with the concavity below. This appearance is named by the author fibrous fan (*éventail fibreux*). On the convexity of the fibres are the upper or ventricular masses of the striate body and optic thalamus, and in the concavity are placed the extra-ventricular portions of the same bodies.

The disposition of the fibres of each fasciculus having been followed into the nuclear bodies, it remains only to observe that they radiate from the outer surface of the same. Being arranged in layers they extend to the circumference of the hemisphere, and enter the convolutions on the outer surface, or those of the second and fourth orders. Near the nucleus, fibres to the corpus callosum are transmitted through intervals between them.

Circular fibres. All the convolutions of the first and third orders, or those on the inner surface of the hemisphere, and in the fissure of Sylvius, are attached to the circular instead of the diverging system of fibres. The source of these fibres is the band* (*ruban fibreux*) beneath the convolution of the same name. Like the convolution, the fibrous band consists of two parts, a free or vertical, and an attached or horizontal. From the free portion surrounding the root of the hemisphere, are derived offsets to the convolution covering the band, and to those on the inner surface of the brain. The convolutions of the island of Reil, contained in the fissure of Sylvius, receive their fibres from the attached portion of the band, which represents a fibrous arch (*cintre fibreux*), situate along the outer side of the perforated spot, and corresponds to the outer root of the olfactory nerve. Reserving for the present the notice of this arch, it will be sufficient to observe that from its outer side radiates the layer of fibres for the convolutions of the island.

Perforated spot. The author was very minute in his description of the boundaries, form, &c. of this spot, nor is he less particular in the enumeration of the various structures that enter into it. After the removal of the optic nerve, and the gray substance attaching its commissure to the prolongation (*partie centripète*) of the restiform body, the under-mentioned structures will be found in this portion of the hemisphere. Firstly,† occupying about the centre of the space, the prolongation (*arête centripète*) from the posterior fasciculus of the cord, which is directed outwards to the fibrous arch (*cintre fibreux*) on the outer boundary; to it are united in front the olfactory, and behind the optic nerve. Beneath the first layer is a prolongation, with a similar direction, from the middle fasciculus of the cord. Still deeper is the anterior commissure, with the extra-ventricular mass of the corpus striatum in front, and that of the optic thalamus behind it. Placed on the outer side of the spot, corresponding to the attached part of the convolution of the band, is the fibrous arch (*cintre fibreux*) named olfactory arch by Rolando. Giving attachment to the olfactory nerve, the arch joins by its extremities the fibrous band beneath the convolution before named. Internally, it gives attachment to the prolongation of the posterior fasciculus of the cord, and to the anterior commissure; and externally radiate from it two layers of fibres, one ascends round the outer surface of the corpus striatum, and optic thalamus to join the corpus callosum, the other separated from the former by a mass of gray matter, is destined to the convolutions of the island of Reil.

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* It was described by Vicq d'Azyr.

† The base of the brain being upwards.

Corpus callosum. This body forms the fibrous covering of the cerebral nucleus, and, constituted of diverging fibres,* unites finally in the middle line. It receives fibres of origin from many sources. Firstly, from the fibrous arch on the outer side of the perforated spot. From it the fibres radiate, the anterior to the front, and the posterior to the back of the corpus callosum; but the central, united with fibres of the anterior commissure, ascend external to the central masses of the hemisphere, pass between the diverging or peduncular fibres, and end in the middle part of the corpus callosum.† Secondly, from the prolongation of the posterior fasciculus of the cord; these fibres pass through the corpus striatum and optic thalamus. Thirdly, from the tænia semicircularis, and the ventricular masses of the corpus striatum and optic thalamus. So that, except the origin from the fibrous arch, the other processes are continuous with the centripetal part of the posterior fasciculus. Where the fibres become horizontal, to build the roof of the lateral ventricle, the corpus callosum presents a rounded prominent border.

The following are the generalizations of the author on the prolongations of the fasciculi of the cord to the brain:

"All the free surfaces of the cerebral nucleus, viz., of the ventricles, of the perforated spot, of the extra-ventricular surface of the corpus callosum, are formed from fibrous layers, or gray masses attached to the cerebral prolongation of the posterior fasciculus.

"All the free surface of the hemisphere, that is to say, the surface of the convolutions, belongs to the cortical substance, into which is continued, contributing to its formation, offsets from the posterior fasciculus. . . . .

"The prolongations of the anterior and lateral fasciculi occupy always a deeper situation in the cerebrum. . . .

"If they are examined in either the cerebral nucleus or the hemisphere, they are always surrounded by the developments from the posterior fasciculus. They may approach the surface by their extreme ramifications, but they never expand in that surface.

"The cerebral prolongation of the posterior fasciculus occupies in the brain the same position as the skin and mucous membrane in the body; these being supplied by nerves from the posterior, never from the anterior fasciculus.

"The cerebral prolongations of the anterior fasciculus, contained in the interval of the membranous expansion of the posterior, occupies in the brain a position similar to that of the muscular system in the body, which is excited by nerves of the anterior fasciculus." (p. 488.)

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* This view of the corpus callosum being derived from the radiating fibres of the crus cerebri, was held by Tiedemann.

† Reil states that the central fibres of the corpus callosum meet at an acute angle, and sometimes directly anastomose with the middle fasciculi of the fibrous cone, derived from the inner and outer walls of the capsule of the corpus striatum.

Still unable, notwithstanding an attentive perusal of the work, to entertain the opinion of the author, that a prolongation from the posterior fasciculus of the cord is the elementary part of the cerebrum, we must dissent from the conclusions deduced. By this declaration we do not intend it to be thought that M. Foville is supposed to describe fibres which have not been seen, but rather, to trace to the perforated spot a bundle of fibres belonging to the lateral, instead of the posterior fasciculus of the cord. It is much to be regretted that there is no good representation of the described prolongation (*partie centripète*) in its course in the cerebrum. In plate xviii is the best view, but even this is so far from satisfactory, that it might perhaps render doubtful rather than support the opinion of the author. If it is possible then, as suggested, that M. Foville has pursued in his dissections a portion of the lateral fasciculus, seen in the floor of the fourth ventricle, for a continuation of the restiform body all the offsets from the supposed posterior fasciculus would be given from the lateral fasciculus. In that case it would be needless to urge other objections, or to make further remark upon opinions, which would approach so nearly to those at present held. Nor does it so satisfactorily appear that the view of the anatomy of the corpus callosum is altogether correct. Suppose it true, as stated by Tiedemann, that the fibres of the corpus callosum are derived from those of the crus, neither this opinion, nor that of our author will, we think, explain all the appearances of that structure. The large mass of diverging fibres to the anterior and posterior lobes of the brain are independent of those that take origin from the penduncular fibres, and constitute the middle part of the corpus callosum. With Reil we are disposed to believe that the greater number of the fibres are derived from the surface of the brain, particularly the large anterior and posterior prolongations, but that other central fibres are connected with the diverging system of the crus or peduncle, and with the nucleus of the hemisphere. Though anxious to give our author all credit, we have felt it our duty to criticise his statements; not however, for the purpose of finding fault, but with the wish of arriving at the truth, and stimulating others to inquiry and examination. The length and minuteness of the review will sufficiently mark our opinion of the value of the work.

Origin of nerves. Our limits will not allow more than a mere sketch of this part of the volume: to follow the author in the origin of the cranial nerves, would demand not only much space, but much attention on the part of our readers; for here detail seems to be carried to its utmost limit. All who are anxious to be more particularly informed will consult the work, (p. 491, et seq.) The author rejects the usual classification of spinal and cranial nerves. Assuming that he has proved that the anterior, middle, and posterior fasciculi of the cord are continued by distinct portions into the cranial mass, he divides the nerves accordingly into three classes: nerves of the posterior, anterior, and middle fasciculi of the cord. Those that take origin from the spinal cord are named common, and those from the cranial mass, special nerves.

1. *Nerves of the posterior fasciculus of the cord.* In this class the nerves are connected to gray masses or ganglia. In the spinal cord the ganglia are common; in the skull the cerebellum is the ganglion of the auditory and fifth, and the cerebrum, of the optic and olfactory nerves.

In this division are comprised nerves attached to the posterior fasciculus whilst forming part of the spinal cord, medulla oblongata, cerebrum, and cerebellum.

a. From it in the spinal cord are the posterior roots of the spinal nerves, thirty-one in number, which are united to the superficial fibres of the posterior fasciculus, and to the gray substance of the cord.

b. From it in the medulla oblongata are only two, the pueumo-gastric and glosso-pharyngeal.* Soemmerring's deep origin from the gray matter at the posterior part of the bulb is adopted; and it is said that the nerves are joined by the arciform fibres.

c. From it in the cerebellum are derived the auditory nerve and large portion of the fifth. The auditory, attached to the outer side of the restiform body, has one prolongation to the floor of the fourth ventricle, and another backwards beneath the cortical substance of the cerebellum, which previously unites with a similar part of the fifth; the union of the two offsets gives rise to a membraniform layer, that lines the under surface of the cortical substance. The large portion of the fifth is fixed to the deeper parts of the restiform body; it gives backwards a layer to join with that of the auditory nerve, and forwards transverse fibres to the pons.†

d. From the same axis in the cerebrum are the olfactory and optic nerves. The optic is connected internally to the prolongation of the posterior fasciculus of the cord; externally, to the superficial part of the optic thalamus and the tubercula quadrigemina; and anteriorly, at the commissure, there is an attachment in front and behind to the gray matter of that part. The olfactory nerve is united to the surface of the perforated spot, to the prolongation of the posterior fasciculus of the cord to that spot, and to the gray substance of the hemisphere; still deeper, it is connected with the fibrous band and arch in relation with the convolution of the first order, to the extra-ventricular part of the corpus striatum and the fibrous layer covering it, and to the anterior commissure of the brain.

2. *Nerves of the anterior fasciculus of the cord.* This fasciculus extending upwards into the cerebrum, gives origin also to spinal and cranial nerves. None of the nerves of this class present ganglia. In the length of the cord, the nerves are united with those derived from the posterior fasciculus, but the remainder are separate.

a. From it in the spinal cord, are the posterior roots of the common spinal nerves, which plunge into the substance of the cord, some fibres uniting with the anterior fasciculus, others with the gray substance.

b. From it in the medulla oblongata spring the ninth and sixth nerves, and the portio dura. For the sixth and ninth the common origin is given; but the portio dura is traced beneath the transverse fibres of the pons to the prolongation of the anterior fasciculus.

c. From it in the crus cerebri is the third nerve, which arises from the fasciculated part of the crus, and from the locus niger of Soemmerring.

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* The author includes these amongst the spinal nerves, though they are here separated.

† In fig. 3 of plate ii, fibres are delineated as joining the large portion of the fifth to the auditory nerve.

3. *Nerves of the lateral fasciculus of the cord.* This tract has attached to it both spinal and cranial nerves, but all are special and recognized by particular names.

a. To it in the spinal cord is attached but one nerve, the spinal accessory,—a portion of the eighth cranial nerve of Willis. This arises from the "accessory" portion of the lateral fasciculus, which is continued to the restiform body.

b. To it in the cranium three nerves are connected; a portion of the seventh pair, a part of the fifth, and the fourth nerve. The small part of the seventh (*pars accessoria* Wrisberg) is fixed to the lateral fasciculus at the pons. The masseteric nerve, a portion of the fifth, perforating the transverse fibres of the pons, is united to the lateral fasciculus. The fourth cranial nerve is united to the prolongation issuing from the posterior pair of the corpora quadrigemina to join the lateral fasciculus in the crus cerebri.

With the preceding analysis of this work we must be content to terminate our notice. We do not presume to have extracted from it all its stores, for we have confined ourselves to the consideration of the spinal cord and the cerebrum. Recommending strongly the work to the attentive perusal of all, we must repeat once more our desire that the views brought forward may be candidly but critically examined; and pleased shall we be to find a similar spirit of inquiry spreading its influence over every future attempt to enlarge our extent of knowledge of the brain. It has been our aim in this notice to review those parts which would give the anatomical opinions that were new and important, and tend to develop the physiological views of the author. We have therefore selected the cord and its continuations into the hemisphere of the cerebrum, and have traced the posterior fasciculus, through the medium of the perforated spot, into the convolutions on the surface of the brain. With this knowledge, it will be easy to comprehend some of the future views of the author shadowed forth in a concluding passage of his work:

> "To form a physiological theory from this anatomy of the nervous system, it will suffice to give a direction to the course of the nervous agency. That direction is known. Departing from the peripheral parts of the body it reaches the ganglia and the posterior fasciculus of the nervous centre, which communicate with the interior and the exterior of the cranial mass. In that mass is a part, the cortical substance, which is intermediate between the termination of the posterior and the commencement of the anterior fasciculus. From the adherent surface of the cortical substance spring the anterior and lateral fasciculi of the spinal cord; and from those fasciculi are derived the nerves that excite the muscles." (p. 655.)

We must not conclude without offering to the excellent author, the homage of our highest respect for his talents, industry and perseverance; for the comprehensive views, liberality of sentiment, and honesty of purpose exhibited in his work; and, we must add, for the high honour, generosity, and philanthrophy, conspicuous in his conduct as a man and a physician.

ART. XIII.

The Anatomy and Philosophy of Expression, as connected with the Fine Arts. By SIR CHARLES BELL, K.H. Third Edition, enlarged. —*London*, 1844. Imp. 8vo, Plates, pp. 266.

ANATOMY and Physiology have important relations to the arts of design, less extensive perhaps, but certainly not less immediate and essential, than those which they bear to the arts of medicine and surgery. These relations have been little appreciated by our academies and schools of design. An attentive examination of the beautiful and perfect forms of the antique marbles, or of the works of such men as Michael Angelo Buonarotti, will indeed show that the importance of the study has been sufficiently appreciated in all ages, by men of genius; and that the obstacles which they must have encountered in the acquisition of such a knowledge of anatomy as their works display, were surmounted by efforts of patient study and minute observation, such as genius alone could make. But a reference to the means of anatomical study afforded to our artists both at home and abroad, will satisfy any one, on reflection, that its true value is not understood, and that such a knowledge as those means can supply will never subserve the purpose of the artist in the higher departments of painting and sculpture. "The academies of Europe," says the author of the work before us, "instituted for the improvement of painting, stop short of the science of anatomy, which is so well suited to enlarge the mind, and to train the eye for observing the forms of nature: or if they enforce the study at all, it is only in its more obvious application, that of assisting the drawing of the human figure." The study of "the round," of the smooth, and undulating surfaces of the antique, from plaster casts; or the drawing of the external form, from the spiritless and jaded academy figure; or a few popular lectures on the superficial muscles of the human body, can never confer that knowledge which the artist requires. An accurate knowledge of the skeleton, a familiar acquaintance with the form of every bone and articulation, and of the mechanism of the organs of locomotion, passive as well as active, is essentially necessary to enable him to design with freedom and accuracy the human form in the various actions and attitudes of which it is susceptible. He must not only know the form and situation of muscles and knobs of bone, but the mechanism of each movement, and the associated muscular actions with which each is necessarily accompanied. Nor is this all; he must study the muscular acts which are associated with mental emotions, not those alone which are immediately expressive of them, or called into voluntary exercise by their influence, but those which are excited into instinctive and involuntary action through the influence of the mind on the organs of circulation and respiration.

The application of anatomy to the arts of design extends still further; it includes a knowledge of the peculiarities which distinguish the countenance or body in every situation of interest to the painter and sculptor. "A knowledge of the peculiarities of country, of infancy, youth or age: of sickness or robust health; or of the contrasts between manly and muscular strength and feminine delicacy; or of the appearances which pain or death present, belongs to its province, as much as the study of the muscles of the face when affected in emotion."

These propositions are beautifully and forcibly illustrated in the concluding essays of the elegant and pleasing work under review.

> "When I have seen," says its accomplished author, "one unacquainted with the internal structure, drawing from the naked figure or from a statue, I have remarked the difficulty he experienced in showing the course of a swelling muscle, or the slight depressions and conversities about a joint; and this difficulty might be traced to his ignorance of the relations and actions of the muscles. The same perplexity he often feels in drawing the knobbed ends of bones, or the insertions of the tendons at the articulations; for these parts being covered over by the integuments, and cushions of fat of variable thickness, and sheathed in membranes are but faintly marked on the surface. The delicate and less definite indications of the anatomy, though easily traced by one acquainted with the structure of the limb, appear to the uninformed only unmeaning variations in the outline: he has no means of judging of their importance; and he is subject to continual mistakes in attempting to imitate them. .... Drawing what he does not understand, he falls into lameness, or deviates into caricature. ...... Even for attaining a correct knowledge of the body and limbs, the academy figure is far from being an infallible guide. The display of muscular action in the human figure is but momentary, and cannot be retained and fixed for the imitation of the artist. The effect produced upon the surface of the body and limbs—the swelling and receding of the fleshy parts, and that starting out of the sinews or tendons which accompany exertion or change of posture cannot be observed with sufficient accuracy, unless the artist is able to class the muscles engaged in the action; and he requires some other guide to enable him to recollect these varying forms, than that which is afforded by a transitory view of them." (pp. 218-9.)

It may be doubted, however, whether the study of anatomy and physiology will furnish the artist with rules by which he may attain the highest objects of design, but it will educate his eye for the intelligent observation of those minute details, upon which the expression and truth of his delineations must depend.

The concluding essay of this work, which formed the introductory one of the former editions, is full of important hints, showing the utility of the study of anatomy to the painter and sculptor, and the extent to which the study may assist them in their designs. In the essay which precedes this one, a critique on the noble works of Michael Angelo, the result of the author's visit to the continent in 1840, finely illustrates the utility of anatomy in giving freedom, truth and power, to the designs of the artist.

The aim of the author of these essays was, however, of a higher kind, than that of merely commending the study of anatomy to the artist, and of illustrating its advantages. His design was "to direct attention to the characteristic forms of man and brutes, by an inquiry into the natural functions, with a view to comprehend the *rationale* of those changes in the countenance and figure which are indicative of passion." His work comprises an examination into the sources of beauty in the antique statues, and the theories of beauty, natural and ideal, in the human form. So far his work is a contribution to philosophy and the fine arts. But his design extended beyond this. He endeavoured to discover the laws which regulate the expression of the passions in the muscular movements of the countenance and whole frame. It was in the prosecution of these inquiries that Sir Charles Bell was led to the discovery of the motor and sensific tracts of the nervous system, and to his ingenious and beautiful theory of the respiratory system of nerves. To this work then, begun, as we are informed in the preface, and as he had fondly stated in a delineation designed for his brother, the late Professor of Scots law, "when we studied together before the serious pursuits of life began," we owe the highly important discoveries of Sir Charles Bell on the nervous system, and all to which they have subsequently led.

These essays formed not only the earliest, but through life the favourite, and, as it unfortunately chanced, the latest occupation of the leisure hours of their amiable and accomplished author. Originally published in 1806, long after they were begun, and republished again in 1824, they were almost entirely recomposed after the author's return from abroad. He visited the continent in 1840, for the purpose, as we are informed, of "verifying in Italy the principles of criticism in art by the study of the works of the great masters in painting and sculpture." It was while on the eve of finally revising this work for the press, during the relaxation from his professional duties afforded by the summer recess, that his life was terminated by a sudden illness.

Although the work is now issued to the public under the disadvantages which must always attend a posthumous publication, it has been edited with much care and judgment by his friend and relative, Mr. Shaw, of the Middlesex Hospital. That part of the work which was not rewritten—namely, the essay on the nervous system—has been supplied by an appendix by Mr. Shaw, containing a history of the discoveries and theories of Sir Charles in relation to this subject. Those remarks contained in the author's private journal of his continental tour, not already embodied by himself in the work, but apparently designed for its illustration, have been appended in the form of notes.

Several new illustrative sketches, of great beauty and spirit, from designs by the author, have been introduced into this edition. Of these we would notice in particular the child's head in p. 17, as extremely beautiful. All the illustrations of the former editions which were really good have been retained; while those which were at all objectionable have been withdrawn.

In the introduction the author sketches a comparison between ancient and modern art. Setting aside the influence of climate, and of original difference in genius, he seeks for an explanation of their comparative merits in the influences of government, religion, and external agencies. The free institutions of ancient Greece were favorable to the development of genius; the athletic exercises and the national games of the country afforded the means of study to their artists, and a standard of taste and judgment to the public, while their mythology exercised a peculiar influence over the works of art. In the representation of their deities they were led to exaggerate all that was expressive of the higher and nobler qualities of man, and to subdue all that was expressive of sensual or animal passions, or even of human emotion, as far as it might be associated with human weakness. Hence, according to the theory of the author, the idea of *divinity* fancifully attributed to the beautiful forms of the antique.

The ancient Italian masters studied under different influences. The objects of representation were with them strictly human. With the models before them afforded by the antique marbles, they sought, indeed, in their representations of the Saviour, "to infuse *divinity* into the *human* beauty of that countenance, which, though not without feeling, was superior to passion, and in which benevolence was to be represented unclouded by human infirmity." But still it was the representation of *humanity*, not uninfluenced by human sympathies, infirmities, and sufferings. And in the wide range afforded to them by Scripture subjects, they had full scope for the representation of every variety of human character and expression. They studied, too, under the influence of a ceremonial which presented to their eyes rich and pleasing combinations of light and shade and colour, a grand and imposing architecture, picturesque groups in gorgeous robes, and rites hallowed by sacred associations, solemn music, and the smoke of incense.

Our artists study under less favorable auspices. In these northern climes the naked form is never seen, unless in the pale and shivering academy figure: fashion and civilization have destroyed all that can be considered picturesque or graceful in the colours or forms of our dress; while the religion of the Reformation and the influence of philosophy have removed the aids of external and sensible objects, and left the worshippers to the influence of internal contemplation founded on reason, and the exercise of faith in things unseen.

The first essay is devoted to an examination of the permanent form and relative proportions of the cranium and face; and leads the author, in his preliminary remarks and subsequent deductions, to an inquiry into the sources of beauty in the human countenance. In reviewing the theories of beauty, he rejects altogether the notion that there can exist in the imagination an ideal beauty, superior to or different from anything which may be seen in nature. He regards as easily fanciful the idea that beauty in art depends on avoiding what is human and aiming at the representation of the divine. He observes, with truth, that the idea of representing divinity is palpably absurd; and that we know nothing of form but from the contemplation of *man*. The representation of divinity was accomplished by the ancient sculptors, not by avoiding what was human, but by avoiding what was mean or weak in the human character, and exaggerating the features which were associated with the higher qualities and nobler emotions of the mind. This leads the author to the prevalent opinion, "that beauty of countenance consists in the capacity of expression, and in the harmony of the features consenting to that expression." An opinion more concisely expressed in the generalization so beautifully illustrated in Alison's Essays on the Nature and Principles of Taste, that beauty arises from association: those features or those expressions which are associated by us with the possession of pleasing or ennobling qualities, are beautiful or noble. With how much truth and beauty is the subject illustrated by the following paragraph, so characteristic of its author:

> "A countenance may be distinguished by being expressive of thought; that is, it may indicate the possession of the intellectual powers. It is manly, it is human; and yet not a motion is seen to show what feeling or sentiment prevails. On the other hand, there may be a movement of the features, and the quality of thought—affection, love, joy, sorrow, gratitude, or sympathy with suffering—is immediately expressed. A countenance which, in ordinary conditions, has nothing remarkable, may become beautiful in expression. It is expression which raises

affection, which dwells pleasantly or painfully on the memory. When we look forward to the meeting with those we love, it is the illuminated face we hurry to meet; and none who have lost a friend but must acknowledge that it is the evanescent expression, more than the permanent form, which is painfully dear to them." (Alison, p. 20.)

The theory of Sir Charles, somewhat differently expressed, does not differ essentially from the one just referred to. It is constructed with reference to the mode in which he has examined the subject. "Beauty," he says, "in the human form has relation to the characteristic organs of man." This principle is fully illustrated by an examination of the relative development of the face and cranium in man and in the lower animals, by a reference to the antique, and to the sources of expression characteristic of *human* passion, as contrasted with those expressive of animal propensities.

With regard to the relative development of the cranium and face, Sir Charles shows that the facial angle of Camper is not only deficient, as is generally acknowledged, as a means of estimating this comparative development, but that it is defective as a rule to the artist for enabling him to give dignity or beauty to the countenance, by merely throwing forward the forehead.

> "Camper's position is this: that as by the diminution of the cranium, and the further inclination of the facial line the head is depressed in character to that of the Negro; so, by raising and throwing the skull upwards and forwards, until the facial line reaches the perpendicular, the great object is attained of resemblance to the antique head. But his own figures contradict his conclusion; for although he has thrown the head forward in them, even beyond the perpendicular of the facial line, yet, as he has preserved the features of common nature, we refuse to acknowledge their similarity to the beautiful forms of the antique marbles. It is true that by advancing the forehead it is raised; the face is shortened, and the eye brought to the centre of the head. But, with all this, there is much wanting —that which measurement or mere line will not show us." (pp. 28-9.)

Illustrative sketches are given, affording abundant evidence of the justice of these remarks. The sketches from Camper completely stultify his own theory. The face in which the facial angle has been increased to more than 90°, is no more to be compared with the antique than "Hyperion to a satyr!" As shown in another sketch, deformity of the bones, from rickets, may increase the angle, even beyond the right angle, as is the case in the Jupiter Tonans, without producing any other idea than that of deformity.

From a comparison of the area of the cranium with that of the face, as seen in a vertical section from before backwards, the mode of comparison recommended by Cuvier, the general law deduced by anatomists, is that the cranium and face, in the different varieties of man, and in the mammalia, are developed in an inverse ratio; that the one increases at the expense of the other.

This conclusion would appear to be too general, if the observations of Sir Charles Bell on the subject in question are well founded. From a number of experiments and measurements made on the skull of the Negro, as compared with that of the perfect European, he endeavours to prove "that in the Negro the whole of the face is smaller instead of being actually larger, when compared with the brain-case than that of the European;" the jaws, however, he shows, contrasted with other parts of the face are larger. (pp. 35-43.) This leads him to the conclusion that the character of degradation and inferiority in the Negro face is not due to the greater relative development of the whole face, but to the development of those parts of it only which are associated in our minds with bodily and animal appetites. The expansion of this conclusion leads further to the general principle, that as the development of the cranium, the seat of the organ of thought, marks the perfection of the human head, and is associated by us with the possession of intellectual superiority; so also the perfection of the human face consists in the development of those parts of it which are associated with the higher intellectual qualities and superior sentiments. This principle pertains to the face, not only as an instrument of expression, but as an instrument for sound, speech, and articulation.

"In the face" it is justly observed, "there is a character of nobleness observable, depending on the development of certain organs which indicate the prevalence of the higher qualities allied to thought, and therefore human. A great mistake has prevailed in supposing that the expansion of some organs in the face of man, marks a participation in the character of the brute; that the fully developed nose indicates the grovelling propensities, and the extended mouth, the ferocity of the lower animals. Let us correct this misconception by considering the properties or uses of the mouth. It is for feeding certainly, but it is also for speech. Extend the the jaws, project the teeth, widen the mouth, and a carnivorous propensity is declared; but concentrate the mouth, give to the chin fulness and roundness, and due form to the lips; show through them the quality of eloquence, of intelligence, and of human sentiments, and the nobleness is enhanced, which was only in part indicated by the projection of the forehead. Now, look to the antique head and say, is the mouth for masticating, or for speech and expression of sentiment? So of the nose. Here, even Cuvier mistook the principle. The nose on a man's face has nothing in common with the snout of a beast. The prominence of the nose and of the lower part of the forehead, and the development of the cavities in the centre of the face, are all concerned in the *voice*. This is ascertained by the manliness of the voice coming with the full development of these parts. Nothing sensual is indicated by the form of the human nose; although by depressing it, and joining it to the lip,—the condition of the brute,—as in the satyr, the idea of something sensual is conveyed." (p. 31.)

These principles lead to the conclusion that the ancients attained their ideal beauty, and their representation of divinity, by elevating, or slightly exaggerating those features which are strictly human. Of the head of Mercury, for example, an illustration of what may be regarded as simply beautiful, Sir Charles Bell remarks,

"The principle which has been followed in giving beauty to the head of Mercury is obvious here. Whatever is peculiar to the human countenance as distinguishing it from the brute, is enhanced. Not only is the forehead expanded and projecting, and the facial line more perpendicular, but every feature is modelled on the same principle; the ear is small and round; the nostril is eminently human, and unlike that of the beast; the mouth, the teeth, and lips are not such as belong to the brute, nor are they the mere instruments of mastication, but of speech and human expression. So of every part, take them individually, or as a whole; whatever would lead to the resemblance of the brute is omitted or diminished." (p. 59.)

The same general principle is extended to the lower animals, and beauty in them is shown to consist in the adaptation of their organs to their wants and habits. In them the peculiarities of the face and of the whole body depend upon their instincts and propensities, and the more perfect the development of those organs which are subservient to these peculiar habits, the more perfect their beauty of form.

While we agree with the author in the application of this general principle to the lower animals as well as to man, we cannot reconcile it with observation and plain matter of facts, to disinherit them of all but instincts and propensities. We believe with him, that to give human expression to brutes, as Rubens for example, has done, in his painting of Daniel in the Lion's den, is not less absurd than repulsive to good taste; but while we deny them human emotions in the full extent, we cannot for a moment admit that they are destitute of mental qualities. It was, doubtless, a fanciful analogy with the expanded forehead of man, that led the ancients to dignify the owl as the companion of Minerva,—the bird of wisdom;—but it requires no exercise of fancy, but only a habit of observation and of intercourse, to perceive in the countenances of our humble and faithful companions, the perfect expression of affection and of reason. Their acts are in many instances explicable on no other supposition than that they are influenced and directed by both qualities, and surely their faces rightly understood will indicate their influence as well as that of the natural propensities by which they are peculiarly characterized.

The principles which we have briefly sketched form the basis of the first two essays, through which, we may say, they are scattered. They are illustrated, as indeed, the whole work is, to a greater or less extent, by pleasing and interesting remarks on the celebrated works of the great masters which the author had examined. We cannot but complain of a want of systematic arrangement, and a degree of amplication and recapitulation extending not only to those essays, but throughout the work.

The second essay contains, at its commencement, also a sketch of the changes which the form of the skull undergoes from infancy to old age; and closes with a survey of the national peculiarities in the form of the head to be met with among the different varieties of mankind. With regard to the form of the head of the ancient Romans, the observations of the author are interesting, and coincide entirely with those of Bishop Wiseman. He concludes that the forms delineated in the majority of their statues and bas reliefs, were formed from Grecian models, and give no idea of the true characters of the Roman head. These are to be distinguished in the imperial busts, and reclining statues of the sarcophagi, or, at the present time, in the Burgesses or among the Galleortti, and lead to the conclusion that the characters of the true Roman, contrary to common belief, were " a large flat head, a low and wide forehead, a face broad and square, a short thick neck, and a stout and broad trunk." (p. 76.)

The third essay is devoted to an examination of those sources of expression in the countenance which are not directly expressive of the acts of the mind, but involuntary and sympathetic.

> " We can readily conceive," says Sir Charles, " why a man stands with eyes intently fixed on the object of his fears, the eyebrows elevated to the utmost, and the eye largely uncovered; or why, with hesitating and bewildered steps, his eyes are rapidly and wildly in search of something. In this we only perceive the intense application of his mind to the object of his apprehensions—its direct influence on the outward organ. But observe him further: there is a spasm on his breast, he cannot breathe freely, the chest is elevated, the muscles of his neck and shoulders are in action, his breathing is short and rapid, there is a gasping and a convulsive motion of his lips, a tremor on his hollow cheek, a gulping and catching of his

throat; and why does his heart knock at his ribs, while yet there is no force of circulation? for his lips and cheeks are ashy pale." (p. 88.)

The explanation of those symptoms of emotion, Sir Charles Bell finds in the sympathy between the mind and the heart. The derangement of the heart's action, produced by the influence of passion, disturbs the circulation through the lungs, the breathing becomes affected, the whole apparatus connected with the respiration and the voice is influenced, and thus the muscles of respiration become the organs of expression. In this manner are explained the trembling lips, the dilated nostrils, the loss of voice, and the fixed or expanded chest which accompany strong emotion.

The beautiful and ingenious theory, commonly called "the respiratory system of Sir Charles Bell," in our opinion falls as far short of the truth as an exposition of the involuntary indications of passion, as we believe it goes beyond the truth as an exposition of part of the anatomy of the nervous system. The muscles of respiration are undoubtedly organs of expression, and they are so, probably, through the medium of the heart, in the manner Sir C. Bell has so well explained and illustrated. But these are only examples of a sympathy which affects the whole frame and makes every organ an instrument of expression. The same emotions which produce the palpitations of fear, and the speechless agony of terror, which make us sob with anguish, or laugh with joy, or breathless with delight, produce also the powerless grasp and trembling knees, the tears of woe, and of excess of happiness. Fear relaxes every limb; nor does it stop with the muscles of animal life; it affects those also of involuntary motion. Fear and anger blanch, while shame crimsons the face. One emotion makes the tears fall, another makes the mouth parched, and the voice inarticulate not so much from paralysis of its muscles, as from want of moisture. In short, no organ of the body is beyond the influence of the passions. Of this innumerable illustrations might be adduced. The increased secretion from the skin, or mucous membranes, under the influence of certain emotions; the sudden development of jaundice from fear; the whitening of the hair from grief or anxiety, the influence of the mind in the production or cure of disease, all evince the extent to which the whole frame is affected by the mental emotions. To regard the muscles of respiration as, *par excellence*, the organs of expression, is to take a limited view of the subject. They are affected more readily, and to a greater extent than other muscles, and this, perhaps, because they are exposed to a twofold influence, the direct influence of the passion, and the indirect influence produced by the derangement of the heart's action, and that of the circulation through the lungs.

In connexion with this subject, and as affording an additional illustration of the justice of our criticism, we may refer to the movements of the eye under the influence of emotion. Granting that the fourth pair (*pathetici*) could be traced to the so-called respiratory tract of the nervous system, how does that fact explain the upward glancing of the eyes in certain emotions, as stated by Sir Charles Bell? To resort, as he has done, to the supposition that in this instance the impression conveyed through the fourth nerve to the superior oblique muscles produces *relaxation* of that muscle, even if it could explain the phenomenon, is only another illustration of the extent to which a favorite hobby can be driven, for that movement is produced not by the superior but by the inferior oblique muscle of the eyeball. The inferior oblique is supplied by the common motor nerve of the eyeball: how does this fact consist with the theory that it is the special function of the respiratory muscles and those associated with them, to afford a language to passion.

The Fourth and Fifth Essays are devoted to a description of the muscles of the face in man and the lower animals, in relation to expression. The anatomical descriptions, we think, are somewhat meagre, and the woodcuts by which they are illustrated rather poor. The description of their uses, and their relations to speech and expression, is ingenious and interesting, and is illustrated by references to familiar facts, and to well-known works of art.

Certain muscles in the human face are shown to be exclusively designed for being instrumental in expression. These are mostly the muscles of the eyebrow and forehead: "Frons hominis tristitiæ, hilaritatis, clementiæ, servitatis, index est." (Pliny.) Some muscles are partly instrumental in breathing, but mostly in speech; these also are peculiarly characteristic of man, and main organs of expression. So far as these muscles are connected with respiration, they are common also to the lower animals, and give a peculiar character to certain animals distinguished by their fleetness and activity, as is seen, for example, in the dilated nostrils of the horse. The muscles of the cheeks and lips in the lower animals have special relations to their habits: in the gramnivorous animals they are adapted for covering and uncovering the incisors in gathering food, in the carnivora for retracting the angles of the mouth and uncovering the canine teeth while the animal prepares to seize its prey. The development of those muscles respectively gives to each class of animals their peculiar characters.

Man, being omnivorous, partakes, to a certain degree, of the characters of both these classes of animals in the development of the muscles of the mouth; but in him the mouth is also an instrument of speech, and it is from its adaptation for this function that it derives those characters which are peculiarly human. The human countenance is thus capable of expressing the rage of the most ferocious animals, and the gentleness of the most timid; while at the same time, by a *special* provision for speech, and the expression of mental emotion, it becomes an index of the infinite variety of thoughts and feelings of which the mind is susceptible.

From the illustration of these principles Sir Charles Bell passes, in the subsequent essays, to the description of the outward manifestations of the various passions. His own graphic delineations of passion, and his illustrative sketches are accompanied by references to the celebrated works of the great masters, and citations from the animated and accurate representations of Virgil, Tasso, Ariosto, Spencer, Shakspeare, and Milton. An explanation of the action of the various muscles in producing the expression of those passions, is at the same time given; and some general rules for their representation by the artist are thus elicited.

The utility of such rules and descriptions is, we believe, apt to be very much overrated. To be told that certain muscles which are described elevate, and certain others depress the lips, and that the angles of the mouth are drawn upwards in laughter, downwards in grief, and outwards in rage, may direct the artist to a certain extent. But there is much in the expression of the various passions and their modifications which description can never convey, and which no rules can comprise. A natural talent for imitation, and a habit of careful observation, with a power of realizing in his own mind a vivid conception of the emotion to be portrayed, can alone enable the artist to arrive at eminence in the delineation of the passions. The study of anatomy, and the descriptions of the poets may aid in the study of nature, and quicken the powers of observation; but in relation to the higher departments of art it may be said of the artist as of the poet, "*nascitur, non fit.*" To the artist we believe that even the study and observation of nature in reference to this subject, is of use simply by enabling him to read accurately in the countenance the emotions of the mind. By the aid of certain rules we may at once delineate the general features of any class of emotions; but the details which give spirit and character to the whole, or which indicate the mixed emotions or modifications of feeling which he wishes to depict, are worked out, not by rule or by the recollection of certain lines, but by a method of trial and error, as mathematicians would call it, by which he gradually develops the expression suited to his subject. We cannot look at the admirable sketch at p. 178 of the work before us without a conviction that the description was made for the sketch, and not the sketch for the description. The rules and principles have yet to be discovered by which an artist may be enabled to delineate the workings of the human mind. Notwithstanding all that has been written on the subject, we believe that he must yet rely chiefly on the vivid conceptions of his own mind to guide him intuitively to the delineation of passion.

We have dwelt thus long on this work, not only because it comprises several interesting inquiries of a philosophical and physiological character, and because it was the favorite occupation of a distinguished physiologist, but because we believe the subject is one of interest, and of some practical importance to the physician as well as to the artist. Good taste and sound judgment in relation to the fine arts form part of the necessary accomplishments of a gentleman. They are certainly not less necessary if he is also a physician. The direct and obvious advantages of being able to delineate with the pencil the form and appearances of diseased structures, or the daily objects of scientific observation, require no comment. But the study of the arts of design, pursued even to the highest department—the delineation of human passion—has its advantages; it sharpens the powers of observation, and quickens the eye for the detection of those delicate changes in form, colour, or expression, by which the diagnosis of the accomplished physician is so promptly and correctly made.

In conclusion, we would say that this is a beautiful book, in every sense of the word; and, like everything beautiful, delightful. It is not, to be sure, what in vulgar language—language unworthy of science—is called a "practical book;" but it is one which no physician or surgeon, with any pretensions to literary taste, can pass by unread; and assuredly it is one which no medical library or medical book-club in the United Kingdom can do without.

ART. XIV.

1. *Histoire d'une Opération d'Anus artificiel pratiqué avec succès par un nouveau Procédé, dans un cas d'Absence congéniale de l'Anus; suivie de quelques réflexions sur les Obturations du Rectum.* Par J. Z. AMUSSAT. *Lu à l'Académie des Sciences, le 2e Nov.* 1835. (*Gazette Médicale de Paris, 28th Nov.* 1835.)

History of an Operation for Artificial Anus, successfully performed by a New Method in a case of congenital absence of the Rectum. By J. Z. AMUSSAT. *Read before the Academy of Sciences, 2d Nov.* 1835.

2. *Mémoire sur la Possibilité d'établir un Anus artificiel dans la Région lombaire sans pénétrer dans le Péritoine. Lu à l'Académie Royale de Médecine, le 1er Oct.* 1839. Par J. Z. AMUSSAT.—*Paris*, 1839.

Memoir on the Possibility of establishing an Artificial Anus in the Lumbar Region without opening the Peritoneum. Read before the Royal Academy of Medicine, 1st Oct. 1839. By J. Z. AMUSSAT.—*Paris*, 1839. 8vo, pp. 210.

3. *Deuxième Mémoire sur la Possibilité d'établir un Anus artificiel dans les Régions lombaires sans ouvrir le Péritoine.* Par J. Z. AMUSSAT. *Lu à l'Académie Royale de Médecine, 6 Sept.* 1841.—*Paris*, 1841.

Second Memoir on the Possibility of establishing an Artificial Anus in the Lumbar Regions without opening the Peritoneum. By J. Z. AMUSSAT. *Read before the Royal Academy of Medicine, 6th Sept.* 1841.—*Paris*, 1841. 8vo, pp. 60.

4. *Troisième Mémoire sur la Possibilité d'établir une ouverture artificielle sur le Colon lombaire gauche sans ouvrir le Péritoine, chez les Enfans imperforés. Lu à l'Académie Royale des Sciences, le 4 Juillet*, 1842. Par M. AMUSSAT. (*L'Examinateur Médicale*, Nos. 16, 17, 18; Ann. 1843.)

Third Memoir on the Possibility of establishing an Artificial Opening in the left lumbar Colon without opening the Peritoneum in Infants born with imperforate Anus. Read before the Royal Academy of Sciences, 4th July, 1842. By M. AMUSSAT.

5. *Du Cancer du Rectum et des Opérations qu'il peut réclamer, parallèle des Méthodes de Littré et de Callisen pour l'Anus artificiel.* Par A. VIDAL (DE CASSIS.)—*Paris*, 1842.

On Cancer of the Rectum and the Operations it may require; with a parallel between the Methods of Littré and of Callisen, for establishing an Artificial Anus. By A. VIDAL (DE CASSIS.)—*Paris*, 1842. 8vo, pp. 128.

6. *A Case of Carcinomatous Stricture of the Rectum, in which the descending Colon was opened in the Loin.* By ALFRED JUKES.—*London*, 1842. 4to, pp. 24. With Plates.

THE formation of an artificial anus may be obviously requisite, or more or less doubtfully indicated, under a considerable variety of circumstances; but notwithstanding that the subject is of necessity touched on in almost every systematic work on surgery, and has been also considered in several detached papers and memoirs, and in some special treatises on the surgical diseases of the intestinal canal, the propriety of the operation and the most eligible mode of performing it in certain cases have been but little discussed. The object of M. Amussat's memoirs is to establish settled rules of practice wherever vagueness and uncertainty have hitherto existed, and also to substitute improved methods of operating for those hitherto in use. We shall first consider M. Amussat's views respecting the formation of an artificial anus in the anal region, and then examine the memoirs on the performance of the operation on the lumbar colon.

The malformations commonly termed imperforate anus may be divided into two classes. In one the gut is not impervious, but opens in some preternatural situation, as into the bladder, the urethra, or the vagina. In the other the rectum is really imperforate, being either obliterated to a greater or less extent, or obstructed by a membranous septum, at a variable height above a naturally formed anus; or else it terminates in a cul de sac at an indeterminate height in the pelvis without any external aperture. The operation hitherto performed in all those cases is, M. Amussat maintains, quite insufficient, and ultimately attended with uniform failure. In the first two cases of imperforate anus on which M. Amussat operated, the rectum terminated between one and a half and two inches from the surface; in each case he formed an artificial anus by the usual method of simply cutting down on the rectum, but both infants died jaundiced in a few days, which he attributed to absorption of the bile, and meconium consequent on their coming into contact with a wound of such considerable extent; he therefore determined in future to adopt a method calculated to obviate that inconvenience, which we shall perhaps best explain by giving an abstract of the case in which he first carried it into effect.

A female infant was born with the following malformation: The vulva and anus were both naturally formed externally, but the recto-vaginal septum was deficient above, and only existed inferiorly to the extent of about one third of an inch, so that the finger could be passed from either canal into the other. The upper portion of the rectum had no communication with the cloaca common to the vagina and the anal portion of the rectum; but its closed extremity could be felt at a height of about two inches towards the left sacro-sciatic angle. The anus thus communicated directly with the vagina above the imperfect septum already mentioned, but had *no connexion* with the rectum, which terminated two inches above it, and was in fact, properly speaking, *deficient* to that extent. Under these circumstances M. Amussat determined

> "To make an incision anterior to the coccyx but posterior to and not involving the vaginal anus, to detach the posterior wall of the vagina from the coccyx and sacrum with the finger or the knife, to reach the cul de sac of the rectum, seize it with a hook, detach its entire circumference rather with the finger than by the knife, draw it down to the external wound, open it freely, give exit to the meconium, and secure, by points of interrupted suture, the edges of the opening in the intestine to the lips of the cutaneous wound." (Gaz. Méd. de Paris, 1835, p. 755.)

The operation thus planned was executed with much less difficulty than had been anticipated, and certainly with a most satisfactory result, for the last notice we find of the case is eight and a half years after the performance of the operation, and the patient then enjoyed excellent health. Nothing is said as to how far she possessed command over the retention and evacuation of the feces, and the artificial anus, it would appear, retained a tendency to contract, which commenced twelve days after its formation, as the dilatation of the rectum was maintained by the occasional introduction of a cylinder of wood. (Gaz. Méd. de Paris, 1843, p. 100; Gaz. des Hôp. de Paris, 1843, p. 67.)

The essential feature in the operation performed in this case is the bringing down the upper portion of the rectum to the level of the external wound, and thus obtaining along the entire extent of the artificial trajet the presence of tissues whose organization is adapted to the functions they are called on to discharge, and its execution "is based on the possibility of elongating the extremity of the great intestine from one to two inches, the inferior mesenteric artery alone preventing a still greater elongation, which the sigmoid flexure of the colon would readily admit." But in order to ensure a sufficient elongation of the gut, M. Amussat, in his third memoir, especially insists on the necessity of forming the artificial anus, not in the situation of the natural aperture, but posterior thereto, in the coccygeal region. When the case we have just given an abstract of was communicated to the Royal Academy of Medicine, it was suggested that the rectum should have been connected with the anus in order to take advantage of the presence of the sphincter; and M. Amussat was then inclined to admit the propriety of the suggestion, and regretted that the apprehension of forming a recto-vaginal fistula had deterred him from making the attempt; he subsequently, however, became convinced that the rectum probably could not have been sufficiently elongated for that purpose, and in a similar, or indeed in any other case, would again form the anus in the region of the coccyx, because the rectum has a shorter distance to traverse to reach the surface if drawn directly *backwards*, than if drawn downwards to the situation naturally occupied by the anus; he therefore makes the external incision immediately in front of or even on the left side of the os coccygis, which bone he would excise, if necessary, to gain room; and in one case did remove its extremity for that purpose. (L'Examinateur Méd. 1843, pp. 198-216.)

The cases in which M. Amussat recommends this operation include almost every variety of malformation of the rectum. He would apply it in every case of true imperforation of the rectum, in which it was possible to reach the gut, with the exception of those only in which the anus, otherwise well formed, is obstructed by a mere *superficial* membrane; but if the septum, however thin and yielding, however it may be distended by the accumulation of meconium, is situated *above* the anus, he insists that it is insufficient to destroy the septum, "that no case can be cited in favour of that method," which fails because of the difficulty of keeping an opening above the anus dilated; and M. Amussat therefore lays it down as a rule, that in such cases "we should operate as if there was no anus, as if the rectum was completely deficient throughout the entire extent of its anal extremity, and cut backwards and draw the rectum not downwards to the anus but directly backwards," etc. (L'Examinateur Méd. 1843, p. 198.) When the rectum opens into a neighbouring organ, the indication, according to M. Amussat, is the same as in complete imperforation of the anus, unless the abnormal anus sufficiently discharges the function of defecation; but when the gut communicates with the bladder or with the urethra, its posterior wall only should be drawn down. In those rare cases where the intestine opens on the parietes of the abdomen, the operation is inapplicable, and if the orifice is insufficient and cannot be dilated, an artificial anus should be formed in the lumbar region.

M. Amussat, we have seen, attaches no importance to bringing the rectum into connexion with the sphincter ani; on the contrary, he deprecates the attempt to do so for the reasons already assigned. We shall hereafter see that M. Amussat, frequently and explicitly as he expresses this opinion, is somewhat inconsistent with himself on this point; but for the present we shall only observe that when the anus exists we think it of great importance to preserve it; and the rectum, *if* it is to be drawn down at all, should, if possible, be brought into communication with it. When there is no trace of the anus externally, does the sphincter exist, and if so, can its functions be preserved to the artificial opening? On this subject there is a strange diversity of statement. J. L. Petit (Mém. de l'Acad. de Chirurg.) says that in imperforate anus the sphincter indeed exists, but is so contracted, wasted, and confounded with the surrounding parts, that it is difficult or rather impossible for it to resume its functions with whatever care the operation may be performed. P. F. Blandin (Dict. de Méd. et Chirurg. Prat.) says, "When the anus is completely absent, I have ascertained that the sphincter is absent also, whether the skin does or does not present an indication of the natural situation of the rectum;" and this very respectable authority in surgery actually thence concludes, that as an artificial anus established in the perineum must occasion incontinence of feces in a very inconvenient situation, we should prefer opening the sigmoid flexure of the colon whenever the anus is truly imperforate. Velpeau is also of opinion that the sphincter is always absent in such cases. On the other hand, M. Roux of Brignolles (Mém. de l'Acad. Royale de Méd. 1835, t. iv, p. 183) maintains that imperforate anus, with the deficiency of the lower portion of the rectum, is occasioned by the deviation or the insufficient size of the branch of the pubic artery by which that part of the rectum is supplied, but that the levator ani and sphincter muscles, being supplied from the ischiatic artery, exist independently of the rectum, and are invariably present, and that, consequently, the operation should be so conducted as to preserve them in connexion with the newly-formed anus, and thus enable the patient to have command over the retention of the feces. M. Goyraud (Journ. Hebdom. 1834, t. iii, p. 245) again lays it down as an undeviating rule, that the deeper portion of the sphincter ani is always absent when the lower portion of the rectum is deficient, but that the superficial portion of the muscle not only always exists, but is preternaturally developed in those cases, and he thence comes to the same practical conclusion as M. Roux. The truth of course lies between those general allegations. M. Blandin's and M. Velpeau's dissections prove that the sphincter is sometimes absent; while the experience of MM. Roux and Goyraud show that it is sometimes present; but it remains to be deduced from facts how far the presence or rather the preservation of the sphincter, in connexion with the artificial anus, is essential to prevent incontinence of feces. On the one hand, in MM. Roux and Goyrand's cases, the anus was formed with the greatest care in the mesial line of the sphincter muscle; in the former case the child lived ten days, during which time the anus seemed to be under the influence of the muscle, and even enemata were retained; and in the latter case, the anus, we are told, fulfilled all the functions of a natural anus six weeks after the operation, which is the latest report we have of the case. On the other hand, B. Bell, in his 'System of Surgery,' mentions two cases "in which the gut lay deep," on which he operated, *probably* without preserving the sphincter, as he "cut backwards along the coccyx;" but in both he "was fortunate enough to form an anus which for a good many years has continued to answer the purpose *sufficiently* well;" indeed the difficulty of keeping the passage open is the only circumstance of which Bell complains in those cases. A most valuable case is recorded by Mr. Ferguson, (Edinb. Med. and Surg. Journal, 1831, p. 363; and 'A System of Pract. Surgery,' p. 527,) in which the rectum terminated in the membranous portion of the urethra, and an incision having been made into the gut, we presume, (but, as was supposed at the time, into the neck of the bladder,) the boy lived and throve; and what is very important, possessed command over the evacuation of both urine and feces till he was six years old. As Mr. Ferguson, after a post-mortem examination, expresses a doubt whether the sphincter and levator ani muscles existed, it may be at the least inferred that they were not in a condition to have exerted any efficient control over the function of defecation. M. Amussat, from the fact that patients after excision of the lower extremity of the rectum have retained control over the alvine evacuations, thinks the same power would probably exist after his operation for artificial anus, the more so, as he is of opinion, that the interior outlet of the abdomen is so disposed, independently of its muscular apparatus, as to favour voluntary retention of the feces. It would be satisfactory to know how far his patient enjoyed that power; but this unfortunately is not stated: the inconvenience in this respect, however, if any, would appear to be but slight, for as in Bell's cases, the only annoyance mentioned is the necessity of keeping the anus mechanically dilated. Though we must confess that the few facts we are able here to bring together are insufficient to determine the question we have put, they yet seem to justify the hope, that whether we operate for imperforate anus by the old method or that of M. Amussat, the power of retaining the feces may be preserved, or that, at all events, the infirmity will be greatly less than M. Blandin and others anticipate as inevitable.

It cannot, we conceive, be questioned that the method adopted by M. Amussat in his first case, was a most meritorious and happy improvement of the older operation for forming an artificial anus. The intestine having been within reach of the finger could have been easily opened, (though perhaps not very easily kept pervious,) but from the nature of the malformation in no other way than by the novel method of bringing down the cul de sac of the rectum to the level of the integuments, could an infirmity of the most disgusting kind have been obviated. But though in this and analogous cases M. Amussat's operation is a most admirable improvement, we think he seeks to extend the method too far.

In the first place M. Amussat unquestionably exaggerates the imperfection and failure of the ordinary mode of operating in imperforation of the rectum and of the anus, when he says in some passages that "every," and in others that "almost every" operation has failed in which more than a superficial membrane had to be divided, and even alleges, as we have already observed, that there is no example of the division of a simple septum within the rectum having proved successful. That M. Amussat has fallen into a strange mistake in thus imputing uniform failure to the operation by simple incision sufficiently appears from the cases of B. Bell and Mr. Ferguson, which we have already referred to for another purpose, and we need not occupy space by multiplying quotations to the same effect, but so thoroughly persuaded is M. Amussat that such is the case, that he contrasts this supposed uniform failure with the few cases in which Littré's operation has succeeded, and seems to infer that the latter is the preferable operation, (Gaz. Méd. de Paris, 1835, p. 758.) But though we thought it right thus to notice the inaccuracy of M. Amussat's most cogent argument, (most cogent if true,) for general adoption of his new method, we shall not further enter on the discussion of the various objections which we think might be urged against its application in various cases. We could only suggest theoretical objections, and we prefer making M. Amussat criticise himself, and shall show from his own words that he distrusts his own rules, and also directly violates in practice the precepts which he inculcates as undeviating canons in the didactic parts of his memoirs, that part, we may observe by the way, which many, perhaps most, readers would be the more likely to consult. In the memoir in the 'Examinateur Méd.' 1843, he republishes the case of which we have given an abstract, and terminates it by a caution not appended to the case as originally printed, and moreover, not hinted in the rules for operating laid down in the first part of this *very same* memoir, where it would certainly have been much better placed than as an incidental observation in the report of a case. The caution in question is, "it is in general not right to persevere and count too much on the possibility of depressing the rectum, lest serious or even fatal consequences might result," (Loc. cit. p. 215;) and moreover, we find that in four other cases published in this very memoir, M. Amussat adopted his own method, (which we are told a few pages previously is universally applicable,) in *one only*. Of those four cases, one (second in the memoir) cannot be taken into account, as M. Amussat failed to reach the rectum, though after death the incision was found to have so nearly attained its extremity, that had he persevered, (which the exhausted condition of the patient forbad,) he would probably have achieved his object. In the second case, (third in the memoir,) though the rectum lay at the depth of one inch from the surface, the improved operation was *not* performed, but the gut was opened by a simple incision. The child died two days after. In the third case, (fourth in the memoir,) M. Amussat made a deep incision in the coccygeal region after having excised the extremity of the os coccygis to gain room without discovering the rectum; he then proposed to open the lumbar colon, but the child's parents having refused their consent, he resumed the original operation, found the rectum attached to the upper part of the vagina, and having drawn it down, secured it to the lips of the external wound. Death occurred here also in two days. In the last case, (fifth in the memoir,) there was a well-formed anus, (in a male child,) about three quarters of an inch above which the rectum was obstructed by a septum through which no fluctuation, nothing indicating the existence of the upper portion of the rectum could be felt; behind the anus, however, fluctuation could be detected especially when the child struggled, or when the abdomen was compressed. A deep incision in this situation gave exit to meconium, and the membrane separating the two portions of the rectum (posteriorly,) was then cut away so that the anus and wound formed but one opening. Two months subsequently the child was in perfect health. In some reflections on this case M. Amussat observes, that it is important as regards diagnosis, for when no fluctuation could be felt by the finger passed into the rectum, we would scarcely anticipate detecting it lower down behind the anus, but would rather expect that the other extremity of the intestine was situated high in the pelvis. The circumstance, however, is easily explained by admitting that the upper portion of the rectum being more and more distended with meconium and constantly impelled downwards by the efforts of the infant, descended sufficiently low to be recognized externally. J. L. Petit relates a case which shows that this is the true explanation. A surgeon failed to attain the rectum through an incision in the perineum, but another surgeon three hours subsequently found the closed extremity of the rectum distended with meconium protruding through the wound; and hence the important rule of practice that when the rectum is not discovered in the first instance, we should before resorting to Littré's or Callisen's operation, if the condition of the child admits of delay, wait to see if the rectum shall protrude towards the wound, in which direction it will probably experience least resistance. But to return from this digression: M. Amussat there formally contradicts the rules of practice laid down without any reserve or limitation, a few pages before in this very same memoir, for he says,

> "Previous to this case I always endeavoured to bring down the upper portion of the rectum to the anus, after having incised the lower portion backwards. But obviously the operation performed in this case is much simpler and greatly preferable. It resembles an extensive operation for fistula in ano." (L'Expérience, 1843, p. 217.)

On M. Amussat's own showing then, the operation which, as we have seen, he recommends as universally applicable, must be performed with due caution and reserve, in fact only in exceptional cases, for when the cul de sac of the rectum lies high in the pelvis, we must not "persevere and count too much on the possibility of depressing the rectum, lest serious or even fatal consequences might result;" and in two of the cases above referred to, he considered his new operation unnecessary in one, and improper in the other. Although then M. Amussat's operation may be a most useful modification of the ordinary proceeding in some cases, it certainly is not essential to success to adopt it universally, and if it were so, M. Amussat acknowledges that it might be impracticable when the rectum lay at any depth from the surface, that is to say in the very cases in which on his own principles, it would be most necessary.

We may here observe that M. Amussat, and also MM. Roux and

Goyraud, blame their predecessors for not having sought for the rectum with sufficient boldness, and claim no small merit for introducing as an important novelty, free incisions with the knife guided by the finger in lieu of punctures with a trocar or lancet. We freely admit that the former is decidedly the preferable method, but those gentlemen are greatly mistaken in supposing that it remained for them to suggest. B. Bell long since practised and taught this supposed novelty, which has since received the sanction of numerous authorities down to the present day.

When every effort to attain the rectum from below proves ineffectual, M. Amussat asks, should we endeavour to open the cul de sac of the peritoneum, and seize whatever intestine may present itself, at the risk of drawing down a portion of the small intestine? and to this question he replies in the affirmative! (L'Examinat. Méd. 1843, p. 198.) We here, however, have only another example of our author inculcating a rule from which he shrinks in practice, for we need hardly say that in none of his cases did he act on the precept which he here lays down as his deliberate and unqualified opinion. Indeed M. Amussat's opinions are very unsettled and fluctuating; in the 'Bulletin de l'Acad. Royal de Méd.' (Juin 1842, No. xviii, p. 802,) we find him, in contradiction to the entire tenor of his memoirs both of antecedent and subsequent date, expressing his opinion,

> "That, in cases of imperforate anus, we should not try to reach the intestine from the perineal or coccygeal regions, except when fluctuation is manifestly perceptible in those regions. If fluctuation cannot be detected, we expose ourselves to the risk of performing an useless operation, after which it would be impossible to attempt, with any chance of success, the formation of an artificial anus in the left lumbar region; for the infant exhausted by pain and loss of blood, would rarely endure the second operation with impunity."

Now, not to mention the passage referred to immediately before this last quotation, in which he recommends the extreme and extravagant proceeding of drawing down a portion of small intestine through the pelvic cul de sac of the peritoneum, M. Amussat, in numerous other passages, says, that the operation in the lumbar colon should never be undertaken until we have in vain attempted to reach the rectum from below. And in contrast with the unqualified statement in the last quotation, that the existence or non-existence of fluctuation is to govern our proceedings, we shall give the following extract from 'L'Examinateur Méd.,' 1843, p. 198:

> "But in fine, in what cases should we operate above, that is to say in the abdomen, or in the lumbar region? It is very difficult to answer this question at present, as the problem is not yet solved, and cannot be so save by the results of attempts repeated from below."

We might accumulate similar examples of self-contradiction on the part of M. Amussat, but enough, perhaps more than enough, has been done in this respect, and the last quotations naturally lead us to the consideration of the operations in the abdominal and lumbar regions.

Without referring to what is said respecting the ancients, especially Praxagoras, M. Littré was the first in modern times at least to suggest the practice of forming an artificial anus in the abdomen in cases of im-perforated rectum, otherwise irremediable; his ideas are stated very generally, for it is expressly said that the details are purposely suppressed, inasmuch as any dexterous surgeon may easily imagine them; it is generally said, however, that Littré directs us to cut into the cavity of the peritoneum in the left iliac region, open the sigmoid flexure of the colon, and secure the open intestine in contact with the wound by means of a thread passed through the mesentery. It is very strange that this interpretation of Littré's proposal, though adopted by every writer with whom we are acquainted, including M. Amussat, is quite incorrect. Littré's proposition is contained in the following few words : "Il faudrait faire un incision au ventre, *et recoudre ensemble les deux parties* apres *les* avoir rouvertes, ou *du moins* faire venir *la partie supérieure de l'intestine*, a la plaie du ventre, que l'on ne refermemerait jamais, et qui ferait la fonction d'anus." (Hist. de l'Acad. des Sciences, 1710, p. 36.) And thus it appears that Littré's leading idea was the possibility of uniting the upper and lower portions of the rectum by suture after having opened their closed extremities, (for in the case whch gave rise to his suggestion the rectum was pervious both above and below an obstruction about an inch in length,) and this failing, we should *at least* connect the upper portion of the *rectum* with the external wound. There is not a word about the sigmoid flexure of the colon, the thread in the mesentery, &c. The idea of forming an artificial anus in the abdomen, however, is here very clearly suggested, but appears to have been first acted on in 1776, by Pillore, of Rouen, but under very different circumstances from those contemplated by Littré. Pillore's patient was an adult whose rectum was completely obstructed by a cancerous tumour; and the artificial anus was formed in the *cæcum*, which was opened through the peritoneum: the patient survived twenty-eight days, and the immediate cause of his death seems to have been intense inflammation of the jejunum, caused by two pounds of metallic mercury administered a month previous to the operation, and which had lodged in and displaced that portion of the small intestine. A. Dubois, in 1783, was the first who performed the operation on an imperforate infant, but no particulars of the case are known, save that the child died on the tenth day. Duret, in 1793, comes next in order, and his case is very remarkable: 1st. Because he previously tried Callisen's operation on the dead body of a child fifteen days old; but finding that he had wounded the peritoneum, as the descending colon was attached to the mesentery, he abandoned this operation and performed that of Littré. 2d. Because the operation was completely successful, the patient having certainly survived forty-two years; and we conclude from M. Amussat's first memoir, p. 187, was still alive in 1839.

We cannot notice the succeeding cases in detail, but shall throw into a tabular form the more important particulars of all those that are known, which we borrow from M. Amussat's 'Memoirs,' except where another reference is specified; and although it is anticipating somewhat, we shall here insert tables of the known cases of Callisen's operation, as modified by M. Amussat, as it will be convenient to place the results of the two operations in contrast in a compendious form.

TABLE I.

Cases in which Littré's operation for establishing an artificial anus was performed for malformation of the rectum.

perator, and date of operation.	Sex.	Age.	Malformation.	Situation of artificial anus.	Result.
1. Dubois. 1783		3 days	Imperforate anus		Death in 10 days
2. Duret.* 1793	Boy	2 days	Do.	Sigmoid flexure of the colon	Living and enjoying good health in 1835, being then 42 years of age.
3. Desault. 1794	Do.	Do.	Do.	Do.	Death in 4 days.
4. Voisin. 1798			Do.		Unknown.
5. Desgranges 1800	Girl	4 years	Rectum opened into vagina	Supposed to be in sigmoid flexure of the colon	In good health 8 months after the operation.
6. Voisin. 1802	Boy	10 days	Total absence of large intestines. Ilium terminated in a small fecal fistula in hypogastrium	Ileum	Death in 4 days.
7. Duret.* 1809	Do.	2 days	Imperforate anus	Sigmoid flexure of the colon	Death 4th or 5th day.
8. Legris. 1813			Rectum opened into urethra	Supposed to be sigmoid flexure of the colon	Death in 17 days.
9. A surgeon of Lyons.			Imperforate anus	Sigmoid flexure of the colon	Successful.
10. A surgeon of Brest.	Girl		Do.	Do. The ovary was included in the ligature passed through the mesentery	Death.
11. Serrand. 1813	Do.	60 hours	Rectum imperforate 1½ inch above anus	Sigmoid flexure of the colon	In good health 22 months after.
12. Freer.* 1816			Imperforate anus	Do.	Death in 3 weeks.
13. Miriel. 1816	Girl	2 days	Rectum imperforate ½ inch above anus	Do.	Living and enjoying excellent health in 1835, being then aged 19.
14. Ouvrard. 1820	Do.	Do.	Imperforate anus	Supposed to be sigmoid flexure of the colon	Unknown.
15. Miriel.* 1822	Boy	84 hours	Rectum imperforate 1¼ inch above anus	Sigmoid flexure of the colon	Living 13 months after operation.
16. Do. 1823	Do.	6 days	Imperforate anus	Do.	Lived 27 months.
17. Bizet.* 1830	Do.		Do.	Do.	Lived 1 month.
18. Do.		100 hours	Do.	Do.	Death next day.
19. Kiewig.* 1835	Boy	3½ days	Do.	Do.	Lived 3 years.
20. Roux.			Do.	Do.	Death in 2 hours.
21. Bougon.† 1838		13 days	Do.	Descending colon in left lumbar region	Death in 28 hours.

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* In these cases an attempt was made, in the first instance, to establish an artificial anus in the l region.

† In this case M. Bougon intended to perform Callisen's operation; but as the cavity of the perieum was opened, the case is properly to be classed here. (Gaz. des Hôp. de Paris, 1838, p. 78.)

TABLE II.

Cases in which Littré's operation was performed on the adult, in consequence of obstruction of the intestinal canal.

Operator, and date of operation.		Sex.	Age.	Disease.	Situation of artificial anus.	Result.
1. Pillore.	1776	Man		Cancer of the rectum	Cecum	Death 25th day.
2. Fine.*	1797	Woman	63 years	Do.	Transverse colon	Lived upwards of 3½ months.
3. Freer.	1818	Man	47 years	Stricture of rectum	Sigmoid flexure of the colon	Death 5th day.
4. Pring.	1820	Woman	64 years	Do.	Do.	Alive 6 months after the operation.
5. Martland.	1824	Man	44 years	Do.	Descending colon	Alive 17 years after the operation.
6. Velpeau.	1839	Woman		Do.	Sigmoid flexure of the colon	Death in 2 days
7. Amussat.	1840	Do.	47 years	Do.	Do.	Death in 24 hrs.
8. Thierry.†	1840	Man		Cancer of sigmoid flexure of the colon	Commencement of ascending colon in right iliac fossa	Death in [illegible]

* M. Fine intended to establish the artificial anus in the small intestine, but the transverse colon presented and was opened.

† M. Thierry intended to establish the artificial anus in the cecum, without wounding the peritoneum; but the cavity of the abdomen was opened, and the anus formed in the colon, which intestine lay in front of the cecum, having been displaced by the lodgment of metallic mercury, administered to overcome the obstruction. (L'Expérience; 6th Oct. 1842, p. 209.)

TABLE III.

Cases in which Callisen's operation, modified by M. Amussat, was performed for malformation of the rectum.

Operator, and date of operation.		Sex.	Age.	Malformation.	Result.
1. Amussat.	1842	Boy	2 days	Rectum imperforate 1½ inch above anus	Living upwards of a year after.*
2. Do.	1842	Do.	Do.	Rectum imperforate 1 inch above anus	Death 7th or 8th day.
3. Do.	1843	Do.	Do.	Imperforate anus	Alive and in good health weeks after.*
†		Do.			Attained adult age.
Dupuytren.‡				Imperforate anus	Death from peritonitis.

* Gaz. Méd. de Paris, 1842, p. 267; and 1843, p. 99. Gaz. des Hôp. de Paris, 1843, p. 6 L'Examinateur Méd., 1843, pp. 229-32.

† The reference to this case in MM. Roche and Sanson's work has been already given. The authenticity of the case is doubtful, and M. Amussat's modification was not, it is to be presumed, practised, as the date of the case, if genuine, is long anterior to his memoirs.

‡ This case is briefly mentioned in the 'Leçons Orales.' The artificial anus was formed in the *cecum*, without wounding the peritoneum; and although it consequently does not strictly come under the head of Callisen's operation, yet, as the essential difference between Callisen's and Littré's operation is, that the peritoneum is opened in the latter and not in the former, the case seems to be [illegible] properly introduced here. In all M. Amussat's cases the artificial anus was established in the descending colon.

TABLE IV.

es in which Callisen's operation, modified by M. Amussat, was performed on the adult, in consequence of obstruction of the intestinal canal.

rator, and date of operation.	Sex.	Age.	Disease.	Situation of artificial anus.	Result.
Amussat 1839	Woman	48 years	Cancer of the rectum	Descending colon	Lived 5 months.
Do. 1839	Man	62 years	Do.	Do.	Alive 2 years and 3 months after the operation.
Do. 1841	Woman	50 years	Situation and nature of obstruction unknown	*Ascending* colon	Alive and healthy much improved 2 months after the operation. Feces passed several times by anus.
Do. 1841	Do.	60 years	Cancer of the rectum	Do.	Death 10th day.
Do.* 1841	Man	57 years	Incomplete obstruction from a tumour in left iliac fossa	Do.	Alive 75th day. Health much improved.
Baudens.† 1841	Woman		Stricture of rectum: not malignant	Do.	Death 5th day.
Clements.‡ 1841	Do.		Situation and nature of obstruction unknown at time of operation	Do.	Recovery. At 7th week a quantity of plum-stones discharged from bowels.
Teale.§ 1842	Do.	54 years	Stricture of sigmoid flexure of the colon	Descending colon	Death 7th day.
Jukes. 1842	Do.	30 years	Cancer of the rectum	Do.	Death 16th day.
Amussat. 1844	Woman	53 years	Situation and nature of obstruction unknown	Descending colon	Patient alive and health much improved 1 month after operation.

The operation, commonly termed Callisen's operation, consists in opening the descending colon from the lumbar region without wounding the peritoneum. M. Amussat, besides modifying the steps of the operation, has also extended it to the ascending colon, in which he has been followed by some other practitioners. It is not known who first suggested this proceeding. All that Callisen says is as follows:

> "Si cavum intestinale cultro vel paracentesio attingi nequeat, vix servari poterit æger. Que proposita sub hoc rerum statu fuit incisio intestini cæci vel coli descendentis, sectione in regione lumbari sinistra ad marginem musculi quadrati lumborum facta, ut anus paretur artificialis, remedium præbet omnino incertum, atque hac operatione vix vita miselli servari poterit. Quanquam intestinum in hoc loco facilius attingatur, quam supra regionem inguinalem." (Systema Chirurg. Hodiern. Hafniæ, 1817, t. ii, p. 842.)

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* Gaz. des Hôp. de Paris, 1842. Mars 26, No. 27.
† Gaz. des Hôp. de Paris, 1842. Avril 19, No. 47.
‡ A case of carcinomatous stricture of the rectum, &c., by A. Jukes, p. 16.
§ Prov. Med. and Surg. Journ., March 19th, 1842, p. 486.
‖ Gaz. des Hôp. de Paris, July, 1844, p. 356.

Callisen mentions, then, the operation as having been proposed; he does not say by whom. He neither claims it as his own, nor does he allude to the distinguishing feature of not opening the peritoneum. All that is as yet further known respecting the early history of this operation is, 1st. That Sabatier, *without, however, giving any reference or authority*, says, (Méd. Opérat., 2e ed. t. iii, p. 337,) that Callisen tested the practicability of opening the left lumbar colon without wounding the peritoneum, on the body of a child who died with imperforate anus; that in his first attempt, he opened the peritoneum, but that, by a second incision further back, he attained his object as he had proposed; in effecting which, however, he acknowledged he was much aided by passing his fingers into the first incision, and thus fixing the intestine. 2d. That Duret, as we have already mentioned, made a similarly unsuccessful attempt on the dead subject. 3d. That MM. Roche and Sanson (Nouv. Elem. de Path. Medico-Chirurg., 3d ed. t. v, p. 440) say, a young man had been pointed out to them on whom Callisen's operation had been successfully performed immediately after birth; but that they could neither ascertain the particulars of the operation nor permission to examine the parts. 4th. That M. Bougon (Gazette des Hôp. de Paris, 1838, p. 78) attempted the operation on an imperforate infant, but failed, inasmuch as he opened the peritoneum, as was ascertained by dissection, the child having died. And it may not be out of place to mention, 5th, That Dupuytren unsuccessfully opened the cecum without wounding the peritoneum, in a case of imperforate anus. These, we believe, are the only known facts, and the whole stream of authorities, from Callisen down to the period when M. Amussat resumed the investigation of the subject, condemned Callisen's operation, some maintaining that its performance was impossible, others that it was incomparably more difficult and not less dangerous than Littré's operation. Allan alone (Recueil Périod. de la Soc. de Méd. de Paris, t. i, p. 123) expressed an opinion that Callisen's method was deserving of further consideration. We must refer to M. Amussat's memoirs for the specific quotations which show that Callisen's operation was all but universally condemned; and numerous as they are, they might easily have been augmented. Mr. Ferguson, for example, in the paper we have already referred to, in some observations on Callisen's operation, says,

> "I am inclined to think that a proposal to cut into any portion of the left side of the colon without injuring the peritoneum, either in the young or old subject, must have been made from a very limited anatomical experience; for, from what I have seen, I am positive that not one case in twenty would have admitted of an operation of the nature proposed. I make this statement as to numbers, however, merely at random, not having kept any exact account." (p. 366.)

Such was the received doctrine, such the opinion of the most respectable authorities, when M. Amussat published the series of memoirs, which, at the least, establish beyond any question, that the difficulty and danger of opening the lumbar colon, whether in the old or young subject, had been exaggerated; and further raise a strong presumption in favour of that method, as compared with Littré's. We have entered into these details because some very futile attempts have been made to deny any merit to M. Amussat in this matter, on the score that he has no claim to originality. So far as originality consists in the first rude conception of a method, coupled with an experimental failure, to render that method practicable, certainly M. Amussat has no claim to originality; but his originality, in overcoming difficulties which others failed to surmount and considered insurmountable, cannot be fairly questioned. At the same time we must add, that we consider it quite premature to assume that there is yet anything approaching to a sufficient amount of evidence to authorize us in ranking M. Amussat's operation among the recognized operations in surgery, as an operation, in fact, generally applicable in the cases in which it seems indicated; but if more extended experience speaks in its favour, the entire merit of its introduction is due to M. Amussat.

The lumbar region, in which Callisen's operation is to be performed, is a quadrilateral space, bounded above by the last false rib, below by the crest of the ilium, behind by the longissimus dorsi and sacro-lumbalis mass of muscles, and anteriorly by a vertical line falling on the centre of the crest of the ilium. The colon in this space lies above in front of the kidney, from which it is separated by fat; in the centre of the space it corresponds to the transversalis fascia, by which and a little fat it is alone separated from the quadratus lumborum muscle; below it corresponds to the crest of the ilium; anteriorly and externally it is in contact with the small intestines; its distance from the spine varies as it is contracted or distended. The important point, however, is the relation of the posterior aspect of the colon in this space to the peritoneum; is it constantly or nearly so denuded of peritoneum to a determinate extent, and that both in the adult and in the infant? M. Amussat maintains that in the adult a lumbar mesocolon never exists, that the intestine is denuded of peritoneum, at least on its posterior third, to which extent the cellular tissue, external to the peritoneum, constitutes its outer layer or sheath. This cellular space, which is formed by the separation of the layers of the peritoneum, commences at the angle of union of the transverse and lumbar colon, and has no very constant line of demarcation below, but usually terminates about the crest of the ilium. Its lateral extent is exactly defined by two of the three longitudinal muscular bands which characterize the great intestine, one running in front of the lumbar colon, the other two externally and internally, precisely along the lines whence the peritoneum is reflected on the parietes of the abdomen. But the condition of the parts varies, according to the manner in which the examination is made and with the condition of the intestine; and hence, according to M. Amussat, have arisen the misconceptions respecting the relations of the colon and the peritoneum, and the failures in attempting to open the intestine without wounding the serous membrane. If we open the abdomen in front and draw the colon forwards, the traction will sometimes cause the formation of a mesocolon; but the practically important point is that the extent of space denuded of peritoneum varies with the caliber of the intestine. When the colon is much contracted, he has seen a very small interval between the folds of the peritoneum; but as the colon dilates, for example, if we inflate it with air, the peritoneal folds and small intestines are pushed back, and the cellular space enlarges proportionally to the distension of the gut; and if the intestine is now punctured through, the air escapes, yet the colon does not retract, as the small intestines would, because it adheres, by its posterior surface, to the parietes of the abdomen; and as the same thing occurs when the colon is dilated, from tympanitis or by the feces, the operation is thus facilitated. Such being the disposition of the peritoneum, if a vertical incision along the external border of the quadratus lumborum muscle is carried too far outwards, we may, especially if the intestine is contracted, open the peritoneum, as was done by Callisen and Duret, but a transverse incision will greatly facilitate the discovery of the portion of the intestine denuded of peritoneum. The colour of the intestine sometimes offers an important peculiarity: it is generally of a greenish-yellow, longitudinally disposed, which contrasts with undulated yellow appearance of the small intestines. (First Memoir, pp. 10-17.) If, when the colon has been denuded, we open and draw the edges of the incision to the lips of the external wound, it will be found that the entire caliber of the gut will not prolapse, as would be the case with the small intestine; its posterior wall alone yields, forming a kind of prolonged tube, communicating with the intestine, so that on passing the finger into the gut there is scarcely any salient ridge (*éperon*) opposite the opening. (pp. 12-13.) The same disposition exists on both sides, but the relations of the peritoneum are more variable on the right side. In new-born infants, Sabatier and others say that the existence of a kind of lumbar mesocolon is the rule, its absence the exception, and therefore object to Callisen's operation. M. Amussat, on the contrary, maintains that the disposition of the parts in early life is even more favorable for the operation than in adults; 1st, because the situation of the lumbar colon is more constant in early life; and 2d, because the kidney forms an unerring guide to the colon, which always lies immediately external to that organ. The intestine also seems more firmly adherent to the walls of the abdomen than in adults; and as the operation could only be required in infants for congenital malformation, the gut would be distended to the maximum by the accumulation of meconium. (Third Mem. pp. 216-29.) In one dissection only out of twenty did M. Amussat find a lumbar mesocolon in the infant, and in that case the intestine was empty. (Bullet. de l'Acad. Royale de Méd., 30 Juin, 1842, p. 302.) It appears to be admitted by M. Amussat, that the operation would not be uniformly practicable on the child; as in the Third Memoir, p. 235, he says, "I am more and more satisfied that the anatomical dispositions favorable to the operation are the rule, the reverse the exception." M. Baudens, (Gazette des Hôp. 1842, No. xxvii, p. 127,) on the contrary, without disputing the absence of a lumbar mesocolon in early life, thinks that from the mere fact of the great intestine being less developed in the infant than in the adult, the cellular space in the former must be so small as to occasion great difficulty in reaching it without wounding the peritoneum, and that consequently Callisen's operation should be limited to the adult, unless indeed experience shows, which he thinks possible, that the operation in the loin, even if the peritoneum be opened, is preferable to that in the groin.

As to the mode of performing the operation, we shall advert to but a few points. M. Amussat, instead of the vertical incision mentioned by

Callisen, prefers a transverse incision, four or five fingers' breadth long, midway between the last false rib and the crest of the ilium; and he divides the deeper parts, or even the skin if the patient is fat, crucially, in order to gain room. The advantages of the transverse incision are, 1st, that it makes the operation easier and more certain, and avoids the danger of dividing the lumbar vessels and nerves; 2, that it facilitates finding and opening the intestine without wounding the peritoneum; and 3d, it enables us to establish the artificial anus more anteriorly (First Mem., p. 24), with a view to favour which, the opening in the intestine should be drawn forward and secured to the anterior angle of the wound. (p. 20.) M. Baudens, (Loc. cit., p. 179,) however, objects to the transverse incision, the liability of wounding some of the large branches of the genito-crural and inguino-cutaneous nerves, and also that it exposes too small an extent of the intestine, which should be opened by an incision at least one inch and a half long, as otherwise the anus will contract; M. Amussat, moreover, he says, is obliged to make a crucial incision in the deep parts, which perils the lumbar arteries, is painful, and augments the extent of the wound. In order to combine the advantages of a vertical and transverse incision, without the disadvantages of either, M. Baudens proposes an oblique incision. M. Amussat warns, when operating on the infant, not to expose the kidney too much, and to avoid a crucial incision; but in his first operation on the child, he laid bare the entire kidney, and in the second, divided both the skin and muscles crucially. (Third Memoir, pp. 229-31.)

The most delicate step of the operation is opening the intestine, the great difficulty being to identify the colon. If the intestine is contracted, it may be completely concealed by the quadratus lumborum muscle, whose external border should then be raised, or some of its fibres divided. The greenish colour of the colon may, it is said, occasionally, help us to recognize it; but pressure and percussion with the finger are the best means of ascertaining its presence; and the absence of resistance external to the great intestine is an important sign. (First Memoir, pp. 10-21.) In his third operation, M. Amussat detected the colon from the greater development of its muscular fibres compared with those of the small intestine. M. Baudens objects to all those signs. If the colon is filled with feces, it will be hard, and then may be confounded with the kidney; but if it is supple and elastic, from being distended with gas, then we cannot discriminate it by the touch from the small intestines: M. Baudens, however, boasts that he "has given this operation more certainty, a precision, so to say, mathematical," by the employment of exploratory acupuncturation; before opening what he suspects to be the colon, he introduces an acupuncture needle furnished with a canula, and on withdrawing the needle, either gas escapes or the canula is soiled with feces if it has entered the colon. "Here, then," he exclaims, "is an infallible sign, the merit of inventing which, I trust, will not be disputed with me." (Loc. cit., p. 179.) M. Baudens only forgets to inform us how we are to know whether the instrument has penetrated the large or the small intestine. The kidney, we are told by M. Amussat, is an unerring guide to the intestine in the infant.

M. Amussat acknowledges that his operation is greatly more difficult than Littré's, (Third Memoir, p. 235;) but he elsewhere says that it is not nearly so difficult as might be imagined, being easier and much less dangerous than that for strangulated hernia for example. (First Memoir, p. 27.) Mr. Jukes, too, we find, says the

> "Operation is one of easy performance, and free from those dangers which have hitherto surrounded the surgeon when applying the rules of his art to cases of obstruction in the intestinal canal, or deficiency of the anus from congenital malformation." (p. 16.)

It is essential to examine into this alleged safety and facility of the operation; and first as to its facility:

In M. Amussat's first case no difficulty occurred. In his second case, to use his own words, he was "greatly embarrassed" in detecting the colon, but at length did so, and "was relieved from a most painful uncertainty." (First Memoir, p. 61.) Again, in giving an account of his practising the operation on the dead body, he says:

> "I must acknowledge that, despite all the precautions I took and the care I bestowed on this important operation, I opened the peritoneum without suspecting it. And yet I was well on my guard." (Second Mem. p. 5.)

And a little further, he observes:

> "I must say that this operation is a most trying one (*extrémement pénible*), because of the uncertainty and anxiety experienced by the operator........ *Even when the intestine is distended* there are no sufficient means of identifying it (*rien l'indique suffisament*), although it may be perfectly laid bare; for the only sign by which we can recognize it is the elastic tumefaction, which is the same almost everywhere. The only index, the only guide, the only clue, is the kidney and the adipose cellular tissue beneath it; and when we have actually attained the cellular space on the colon, it is by the touch alone that we can recognize its thick resisting coats and muscular fibres............ At length, however, we must determine on plunging a trocar into the colon. This, I must avow, is a moment of perplexity for the operator. Instead of discouraging surgeons, I wish to inspire them with confidence; but I must also warn them of real dangers, and guard them against the *apparent facility* of this operation, when we only consider *the possibility of sometimes* performing it very quickly and easily when the intestine is greatly distended." (pp. 17-18)

In the fourth case, the anxiety, so well described in the foregoing passage, was fully experienced, and the intestine was opened "with great hesitation." And in the report of the fifth case, (Gaz. des Hôp. de Paris, 1843, p. 170,) we read:

> "It would be difficult to convey an idea of the perplexity experienced by the operator and assistants, when every one now asked was the intestine seen and felt at the bottom of the wound, the colon, or the small intestine?

And again:

> "The difficulty, then, is inherent in the operation itself: it must be performed without any other guide than the adipose cellular tissue of the kidney, and the touch." (Second Mem. p. 57.)

So much for *the facility and simplicity* of this operation on the adult; but we are more than once told that it is easier and more certain in the infant, because of the more constant relations of the colon, and of the guide afforded by the kidney. (Third Memoir, pp. 229-35.) In the account of the first operation on the infant, however, M. Amussat says, "After much uncertainty, *thinking* I recognized the colon, I determined to hook on the tenaculum *what I supposed* to be the gut." (p. 230) In the second case, the supra-renal capsule was supposed to be the colon, and was cut into, and then, after much search, an intestine presented which was *thought* to be the colon, as it really proved to be, "to the great satisfaction of all present." (pp. 231-2.) In the third case the kidney, which is to serve as a guide, was itself taken for the colon; but a slight incision into the gland showed the mistake. The various passages we have referred to may be safely left to speak for themselves; they sufficiently show that this operation, instead of being easy and simple, is eminently difficult and uncertain.

The alleged safety of this operation must be considered in a two-fold point of view: 1st, with respect to the risk of wounding the peritoneum; 2d, with respect to the danger incurred when the operation is performed without injury to the serous membrane.

The risk of wounding the peritoneum must obviously, in a great measure, depend on the relative frequency of the relations between that membrane and the lumbar colon, which are described by M. Amussat as constant or nearly so; into this purely anatomical question we shall not enter, further than stating that M. Amussat's opinions are far from being generally admitted. Many anatomists have found a lumbar mesocolon in the infant, and without going beyond the work of M. Vidal (de Cassis), whose title is prefixed to this article, we find that author stating that he has found the relations of the lumbar colon and peritoneum very variable in the adult, so that frequently, however the intestine might be distended, it could scarcely be opened without wounding the peritoneum. (pp. 113-20.) But admitting that the cellular space is always as large as M. Amussat maintains it to be, still we think it is clear that there must always be considerable danger of opening the cavity of the abdomen, when we recollect the doubt and difficulty so often experienced by M. Amussat in discriminating the colon. But is this a sufficient reason for concluding with M. Vidal that Callisen's operation should be rejected in favour of that of Littré? Certainly not: in the former operation we run the risk of wounding that important membrane; in the latter, we are certain to do so; and if that risk is made an objection to Callisen's operation, it is difficult to understand how the objector can gravely maintain that we should prefer Littré's operation, in which we incur the certainty.

But suppose Callisen's operation is performed without injury to the peritoneum, are we thereby ensured against the occurrence of peritoneal inflammation? M. Amussat, Mr. Jukes, and others seem to think we are. M. Vidal argues that we are not (p. 121), and in this we perfectly agree with him. We think it probable that Amussat's second patient died of peritonitis consequent on the operation, and quite certain that Mr. Jukes's patient did so, and this is only what might be expected. Ligature of the iliac artery, lithotomy, simply leaving a catheter in the bladder, retention of urine, and a varietyof other operations, circumstances, and diseases, in which there is no wound, no primary lesion of the peritoneum, not very unfrequently excite peritonitis; and it would be strange indeed if such an operation as we have described should not occasionally do so also; but can any reasonable man question that whatever may be the risk of peritonitis after Callisen's operation, that risk is incalculably greater after

Littré's? In fine, then, we think both the difficulties and dangers of Callisen's operation have latterly been underrated by some writers; but, on the other hand, still more greatly exaggerated by others. M. Vidal, indeed, attempts to show from the *results of cases*, that Littré's operation has been as successful in practice as Callisen's; but he draws his conclusions from only seven cases of the former and five of the latter method: we refer our readers to the tables in this article, and leave them to form their own conclusion on this point. Whatever the result of future cases may be, the facts at present known are decidedly in favour of Callisen's or rather Amussat's operation.

The advantages of Callisen's operation, as compared with Littré's, are said to be, in addition to avoiding injury of the peritoneum, 1st. That one side of the intestine only is drawn into the wound, whence the internal salient ridge is much less marked than when an entire fold of intestine is drawn down, and consequently there is an additional chance of curing the artificial anus should it become useless. According to M. Baudens, however, this alleged advantage is imaginary. He says we can make the internal ridge sufficiently prominent to block up the entrance to the lower portion of the gut, and recommends with Bourgery that we should do so, inasmuch as the operation should only be performed in cases where we can never hope to see the feces resume their natural passage. 2d. M. Amussat maintains that the lumbar region is a much more convenient situation for an artificial anus than the iliac region; and to judge from his cases it is so, though the very reverse is alleged by M. Vidal and the opponents of Callisen's operation. 3d. It is said that there is less tendency in this operation to prolapse of the intestine. In M. Amussat's first operation on the infant, the mucous membrane of the gut prolapsed the second day, but was easily reduced. 4th. Even if the peritoneum is wounded, yet, as it is opened behind, there is less danger of fecal effusion. We may here mention, that in M. Amussat's second operation on the infant, there was hernia of the kidney in the wound on the fall of the sutures. The chief inconvenience experienced after all his successful operations was a great and incessant tendency to contraction of the artificial opening.

It has been objected to the formation of an artificial anus in the abdomen, that death is preferable to life burdened with such an infirmity. If parents on behalf of their child, or an adult on his own account, think so, it is for them to refuse their consent to the operation; but it is the duty of every surgeon to propose every resource that offers a reasonable chance of preserving life. We cannot consider in detail the indications for this operation, but shall rapidly pass in review the various circumstances in which it has been performed.

Imperforate anus is probably the case in which the propriety of forming an artificial anus in the lumbar colon will be most generally admitted, provided always that an attempt has been previously made to establish an artificial anus, *if possible*, in the anal region; nor should this attempt be lightly abandoned: we have already seen that M. Amussat, in one instance, proposed to discontinue an operation commenced in this situation; the child, it is true, died, but it will scarcely be maintained that an operation in the lumbar region would have been attended with a more favorable result. Some doubtless will agree with MM. Vidal and Baudens, that Littré's operation should be preferred, because the peritoneum is very likely to be wounded in opening the lumbar colon in the infant: be it so; we shall not rely on M. Amussat's dissections and operations to raise the contrary presumption; but as they certainly show that there is at least a chance of effecting our object, that chance should not be thrown away.

An artificial anus has also been established in the abdomen for the relief of invincible constipation, whether produced by an undiscoverable cause or known to depend on obstruction of the great intestine, either from cancerous or non-malignant disease of its own coats, or the pressure of some tumour external thereto. In a few instances also the operation has been performed when the bowels were still pervious, with the view of relieving extreme distress, caused by the difficulty of evacuating the feces, and in the hope that the progress of the organic disease by which that difficulty was caused might be retarded on removing the irritation excited by the pressure and the passage of the feces. Much difference of opinion exists as to how far the operation is indicated under the several circumstances here enumerated. We would certainly abstain from interference when the nature and situation of the obstruction were both unknown, or rather, when the situation of the obstruction could not be approximately determined by percussion, &c. We feel extremely averse to the operation in cancerous disease, but are not prepared to maintain that it could never be justifiable, for we have witnessed cases in which, though the bowels were not completely obstructed, yet the extreme misery endured by the patient would possibly tempt us, in similar circumstances, seeing the favorable result of M. Amussat's fifth case, to give the sufferer the chance of relief; and if the obstruction were complete, it would be perhaps a *duty*, rather than a matter of discretion, to attempt to prolong life. In obstruction depending on non-malignant disease, or disease not known to be malignant, there can, we conceive, be no more question as to the propriety of the operation, than there would be in a case of strangulated hernia, where the intestine was supposed or known to be sphacelated.

The situation selected for the formation of the artificial anus must obviously depend on the determination of the situation of the obstruction above which the opening must, of necessity, be made. M. Baudens maintains that, however low down in the great intestine the obstruction may be, we should always operate on the right lumbar colon, because gas, he says, is exclusively generated in the great intestine; and gas being " the scourge" of a patient with an artificial anus, we should seek to prevent its generation, by permitting the feces to traverse as short a space as possible of the large intestine. We shall not waste time in refuting this strange proposal; nor can we now enter on the very important question of the diagnosis of the nature and position of the various obstructions which may raise the question of operation, but we hope soon to have an opportunity of returning to this part of the subject.

Art. XV.

Life in the Sick-room. Essays by an Invalid. — *London*, 1844. 12mo, pp. 221.

The pathologist thinks no description too minute to discriminate the shades of colour, the varieties of consistence, and the changes of form which the material textures of the body undergo in disease : but have the multiform varieties of morbidity of mind been subjected to a similarly systematic scrutiny ? When this reaches insanity we recognize it as a disease coming within our province, but minor shades of mental infirmity we leave to theologians and moralists. And yet, though it may be convenient to divide diseases into smaller departments, the genus as a whole should not be lost sight of; for it is alike important to recognize the individual members of a family, and their relationship and mutual dependence as a whole. And as all deviations from a healthy state of mind belong to that family of which the most marked condition is insanity, all shades of unsoundness of mind should be included in a comprehensive survey of this disease, and should demand our attention. The improved treatment of insanity in the present day has been owing to our recognition of it as a bodily disease, as well as a mental one, and instead of treating those who are affected with it as beings possessed with devils, and only to be managed by chains and dungeons, bringing them into the same class as those whom we admit to be objects for our hospitals. We are still much in the dark as to the connexion between insanity and organic disease, but we are pursuing the right path. And does not the same argument apply to minor shades of mental disorder? We must be very inattentive observers not hourly to recognize that a very intimate relation does exist between morbid states of the body and minor diseases of the mind—ill-temper, ill-nature, gloom, high spirits, moroseness, fickleness, confidence, despair, passion, and desire; and the due recognition of this fact is very essential. When not duly recognized, and the affection is regarded as a mere spiritual condition, the mistakes committed are of the same kind as in insanity, and less injurious only because the disease was less important. How much good advice, and wise cautions and admonitions and recommendations of good books have not been thrown away, when perhaps the physical remedy which should have preceded the moral one was neglected ! Viewed in this light, all varieties of unsoundness of mind are a subject of great interest, as the knowledge of their connexion with diseased bodily states may direct us to the restoration of a healthy mental condition, by assisting moral remedies with physical ones. In another point of view the study of mere mental conditions is important. They do exist—they are realities to which our patients are very subject, and of which we not unfrequently feel the inconvenience ; and therefore it behoves us to be acquainted with them.

These are our reasons and excuses for directing attention to a work, not professedly medical, but which contains the revelations, to a certain extent, of the morbidity of mind of a nervous invalid.

We gather from the internal evidence of the book itself that the writer is a confirmed invalid, confined to her room, and convinced that she is "under sentence of disease for life." She is a constant sufferer from pain, sometimes of the acutest kind, and the habitual use of opium has become an invariable habit. Debility of body, languor, malaise, incapacity for continuous exertion, with loss of all gaiety of mind and sense of enjoyment; and, with the exception of a few hours twice in a year of light-heartedness, over all the rest a cloud of care—apparently causeless, but not the less real;—are the bodily and mental symptoms which in her case, as in so many of her sex, indicate an exhausted nervous system.

But a very few lines in her book, scattered here and there, are devoted to a description of her bodily ailments; the object of the writer is to unfold the effects of sickness upon the mind, and its manifold helps and hinderances, perils and pains, advantages and compensations, injury and profit: a comprehensive theme, well worthy of deep study, and one which can be treated of, satisfactorily alone by one who has had a long apprenticeship to suffering. This book differs materially from the majority which fall under our notice as critics. Where every word tells it is impossible to condense, and where each sentence is full of matter, and all closely connected into a whole, it is difficult to abridge. It is not our province to go into the whole subject: we shall therefore confine ourselves to those parts only which have some practical bearing, either illustrating morbid states of mind, the existence of which or their connexion with disease of body in invalids it is our province to recognize and to study, or affording hints for the better management of such patients. Those who are further interested in the subject, and who are desirous to know the uses to be made of adversity, we must refer to the book itself.

To man, with his stronger self-reliance, the deep need which women have of sympathy is a matter of observation rather than of experience, and he cannot fully enter into the want. It has been said that man lives in his understanding, woman in her affections; and if so, the fellow feeling which a cultivated man needs is more with his intellect than his feelings. But as the feelings are much more connected with bodily states, the sympathies with feelings which women seem absolutely to require, must necessarily be very considerably influenced by the disorders of the body; and no part of this volume is more strikingly morbid than its revelation of sympathies. There is a separate chapter on sympathy; but the intense craving after this, which is called "a hunger of the heart," "a heavenly solace," and the delight which those afford who have the "true genius for sympathy," who are "archangels of consolation," and the uneasiness, irritability, and vexation which awkward consolers give, are disclosed throughout the book. The following hint to "awkward sympathisers" may be remembered when dealing with this class of patients:

"Going back to the days when I, myself, was the sympathiser, I remember how strong is the temptation to imagine, and to assure the sick one, that his pain will not last; that the time will come when he will be well again; that he is already better; or, if it be impossible to say that, that he will get used to his affliction, and find it more endurable. How was it that I did not see that such offers of consolation must be purely irritating to one who was not feeling better, nor believing that he should ever be better, nor in a state to be cheered by any speculation, as to whether his pain would, or would not, become more endurable with time! Exactly in proportion to the zeal with which such considerations were pressed, must have been the sufferer's clearness of perception of the disguised selfishness which dictated the topics and the words. I was trying to console myself, and not my friend; indulging my own cowardice, my own shrinking from a painful truth, at the expense of the feelings of the sufferer for whom my heart was aching." (p. 14.)

It is difficult for men in health to realize the evident annoyance which such invalids feel at any hope being held out of recovery, but the physician often meets with such. Is it not from the deeper sympathy which is seemingly given, if the cases are considered hopeless? The "genius for consolation" does this.

After expressing her irritability at one of the well-meaning, who had no tact, she adds,

"Far different was my emotion, when one said to me, with a face like the face of an angel, 'Why should we be bent upon your being better, and make up a bright prospect for you? I see no brightness in it; and the time seems past for expecting you ever to be well.' How my spirits rose in a moment at this recognition of the truth!" (p. 15.)

Was it the mere announcement of a truth which caused this exhilaration? Was it not rather the deeper knowledge which this lady-friend had of the invalid's feelings, and of her "keen hunger of the heart," which enabled her to fathom her wishes, and to give just that quantity of the desired food which she craved? Is not this view borne out by this subsequent resolution?

"Never again should the suffering spirit turn from me, as I fear it has often done,—if too gentle to be irritated—yet sickening at hollow words of promise, *when instant fellow-feeling was what was needed.*" (p. 18.)

A man of the highest intellectual power, who was himself an invalid, a severe sufferer from pain, to ease which he habitually took opium, said "that the best physician in nervous diseases was the best inspirer of hope." This was a man's feeling. He resents pity; but how different with a woman; with her "pity is akin to love;" give her no hope and she blesses you.

But how is it possible to soothe the sufferer?

"Speaking the truth in love, is the way. Then the question arises, what sort of truth? Why, that which is appropriate to the one who administers. Let the nurse avow that the medicine is nauseous. Let the physician declare that the treatment will be painful. Let sister, or brother, or friend, tell me that I must never look to be well. When the time approaches that I am to die, let me be told that I am to die, and when. If I encroach thoughtlessly on the time or strength of those about me, let me be reminded; if selfishly, let me be remonstrated with. Thus, to speak the truth in love, is in the power of all." (pp. 26-7.)

The whole chapter will repay perusal. The genial healthy reader will become acquainted with states of feeling which are new to him; the more sickly mind will find some of his own feelings embodied in words, and both will learn something as to the best way of dealing with such patients.

What should be the arrangements of a permanent invalid, with regard to companionship, is an important consideration.

"In most cases, this is no matter of choice, but a point settled by domestic circumstances; where it is not, however, I cannot but wish that more consideration was given to the comfort of being alone in illness. This is so far from being understood, that, though the cases are numerous of sufferers who prefer, and earnestly endeavour to procure solitude, they are, if not resisted, wondered at, and humoured for a supposed peculiarity, rather than seem to be reasonable; whereas, if they are listened to, as the best judges of their own comforts, it may be found that they have reason on their side. (p. 30.) There is no attendance to be compared with that of a servant." (p. 34.)

This unsentimental, but very practical truth, our invalid accounts for metaphysically, and dresses up her reasons becomingly and sentimentally. But many of the reasons with which the work abounds are evidently à posteriori arguments. The matter is first settled, not by any argumentative process whatever, but by the will, and then the decision is accounted for in the most "interesting" way. But, although "sic volo, sic jubeo" is the feminine way of settling such matters, she is not disposed to add "stet pro ratione voluntas," but rather to puzzle herself afterwards, with trying to frame reasons; a mistake; as she often arrives at the truth in such practical matters by a shorter and surer process than ratiocination.

> "When an invalid is under sentence of disease for life, it becomes a duty of first-rate importance to select a proper place of abode. Many a sufferer languishes amidst street noises, or passes year after year in a room whose windows command dead walls or paved courts.
>
> "He will be wise to sacrifice indolence, habit, money, and convenience, at the outset, to place himself where he can command the widest or the most beautiful view that can be had without sacrificing advantages more essential still. There are few things more essential still; but there are some: such as medical attendance, and a command of the ordinary conveniences of life."

Our invalid has chosen a sea view, for its variety, and from the wide expanse of sky it affords. But not too much sea, which would tire; between her house and the sea is a green down, a river, ponds, the ruins of a priory, and beyond a busy harbour; a sandy beach, and a rocky shore. A windmill, a railroad, a colliery, farm-yards and paddocks give a living interest to the whole, and here a telescope is a great additional source of pleasure. This whole chapter, entitled "nature to the invalid," is genial and beautiful. To such invalids, flowers are the most acceptable presents; and "fern-houses" (that is, ferns growing in close glass-cases, devised by a member of our profession) are in a town "a compensation for rural pleasures," and in the country an addition to them.

The good effects of books of travels are alluded to. Such are advisable from carrying the invalid out of himself for a time; they are objective, whilst his mental malady is "subjectivity." The effect of seclusion, in rendering the mind speculative rather than practical, is discussed. (p. 98.) The writer thinks that large and more distant views are taken, and that many doubts and difficulties which beset the mind immersed in actual life, are cleared away: that "moral considerations are all in all," and that seclusion is peculiarly fitted for seeing their beauty and abundance, and recognizing the "deep heaven lying inclosed in the very centre of society, and a genuine divinity residing in the heart of every member of it."

> "All that is most frivolous and insignificant is ever most noisy and obtrusive; all that is most wicked is most boastful and audacious; all that is worst in men and society, has a tendency to come uppermost; and thus, the most superficial observers of life are the most despondent. Meantime, whatever is holy, pure, and peaceable, works silently and unremittingly; and while turbulent passions

relief! But there are sufferings to which this slow relief never comes. Those who have watched the progress of tic douloureux, stone, carcinomatous tumours involving nerves, or cancer of the uterus, will assent to the individual experience of this writer.

> "To almost every kind and to vast degrees of privation—moral and physical—men may become inured; and to chronic sufferings of mind and body; but I am convinced that there is no more possibility of becoming inured to acute agony of body than to paroxysms of remorse—the severest of moral pains. The pain itself becomes more odious, more oppressive, more feared, in proportion to the accumulation of experience of weary hours, in proportion to the aggregate of painful associations which every visitation revives... For the sake of both sufferers and sympathisers it would be well that this should be thoroughly understood, that aid may not fall short, nor relief be looked for in the wrong direction." (pp. 147-8.)

We have thus given the Invalid's own description of some states of mind and feeling produced by permanent and long-continued disease of body. We must refer to the volume itself for the consolations offered, and for the writer's own views on the compensations afforded to such withdrawal from active life, by increasing the power of that prerogative of a thinking being, "looking before and after." But there is another side from which this book may be studied: not merely the writer's own conscious conception of her state of mind, but the display of her own actual condition which every such writer makes, to a great extent unconsciously.

That the writer is a woman would, we think, have been evident from the work itself, had it not been attributed, without contradiction, to one of the best known authoresses of the day. The confession of such faults and failings as create "interest," the hunger after sympathy, the undisguised love of praise, the annoyance at "not being understood," are unmistakable characteristics. Admitting, as we do most fully, the writer's intellectual powers, her lofty and deep thoughts conveyed in a style of consummate clearness, a true feeling for the beautiful and the good, and the power of embodying the poetry of her mind in graceful, captivating prose, aspirations after a high state of moral purity, and a deep conviction that man's moral being should be his prime care—yet we cannot help feeling—and all the stronger, perhaps, for our favorable impressions—to quote the saying of a child to herself, which she repeats more than once: "But there is unhealthiness, and that spoils all."

The head desires ardently to strike the stars; but the poor unwholesome body pulls it down very low, and contradicts its right to any such elevation. The contradiction between the idealism of the abstracted mind, and the actual feelings of that mind when brought into contact with realities, affords the strangest and strongest contrasts. The world and its great parties are seen in a new and clearer light: they are looked at by the invalid with stoical philosophy, as parts of history: what before was dark in the ways of Providence, becomes clear: the common distinctions of rank are "attenuated to a previously inconceivable degree," so that "there would be little more in the sovereign entering the sick-room than any other stranger whom kindness might bring"—"so overpowering in our view are the great interests of life which are common to all—duty, thought, love, joy, sorrow, and death." And yet—alas for poor human nature—if a kind visitor, who has not the organ of order well developed, leaves the chairs awry, and the books unreturned to their shelves, all this lofty philosophy is overthrown, and its possessor cast into a state of miserable irritability. With the clearest supposed insight into the most difficult problems that politicians are working out, there is an incapacity to appreciate correctly the individual's own condition, producing unreasonable dissatisfaction with self, self-distrust, even self-disgust. With the conviction of the paramount importance of being, rather than seeming, there is the painful wincing at a suspicion of being misinterpreted, and not understood—at being thought to be more comfortable, or more miserable as an invalid than in health; at any outward circumstances, in fact, which contradicts at the moment the peculiar feeling of the mind. We do not bring this forward as mere criticism, but as truth in which we are deeply interested as pathologists, as elucidating the effects of disease of body on the mind; and the conclusions we would draw from it are:

1. That the feelings which are certainly most closely connected with the body are the most affected by this disease in which the nervous system is exhausted and depressed, and that they are the most morbid; whereas the intellect remains little disturbed, and, when able to act at all, capable of clear thought.
2. That the advice offered to the individual by his own intellect is very incapable of correcting these disordered states of the feelings.
3. That the mere intellectual powers may be cultivated and improved to a very high extent, and yet, if there is depressing disease of the nervous system, the feelings and what are called the active powers which put us in relation with our fellows, may be in a much lower state than in the healthy whose intellects are comparatively uncultivated—simple-hearted, good people, who are engaged in the active and humblest duties of life.

Self-consciousness has been said to be the disease of the present day; and assuredly a morbid self-consciousness which defeats its object, and becomes blind and puzzled, and bewildered by dwelling too incessantly on the individual's emotions and feelings is one of the morbidities of mind portrayed here. A celebrated thinker of the present day goes so far as to assert that all self-consciousness is disease, that the mind should no more be conscious of its operations than the stomach, and that in either case the consciousness of the work going on, is disease. We cannot go this length. The power by which the mind observes its own acts and judges them as if there were two beings within a man, one the passive spectator of the other's thoughts, feelings, and actions, is the prerogative of the highest class of minds. Much of what we call genius consists in the power of embodying these observations which the soul makes on herself, in words, "giving to airy nothings a local habitation and a name," so that others less highly gifted with expression, on reading them embodied in words exclaim, "I have thought and felt the same thing." "This comes home to me." "This meets me." And although all have not this genius, yet the power of looking inwards is common to all who think at all. It cannot then be called a disease, and be put in the same class with gastrodynia, hypochondriasis, or palpitations of the heart, without a very false and hasty generalization. But, like all other natural powers, it may become diseased when too exclusively cultivated. It becomes a lamentable disease, especially when the feelings and emotions are too constantly self-contemplated, and one to which cultivated minds are peculiarly liable. Its corrective is action; and when invalidism prevents this wholesome counterbalance, a sensitive and thoughtful mind is very apt to become engrossed in its own thoughts and feelings, and to get into a condition of helpless and egotistical selfishness; just in the same degree as a mere hypochondriac becomes engrossed in his own bodily sensations. As we can know our own powers by action only, this condition of self-consciousness is (although seemingly a contradiction) one of great self-ignorance. Whatever becomes the object of constant thought or attention becomes unduly magnified. The individual's own importance is thus greatly increased in his own estimation, and when called into action or into collision with others, the consciousness of his weakness, (often true from want of practice,) combined with his own sense of self-importance makes him irritable to others, or dissatisfied with himself, or both. This unreasonable dissatisfaction with one's self, this "mental weakness which converts the most innocent and ordinary occurrences into occasions of apprehension, or of self-distrust, or self-disgust," is one to which the weaker sex are especially liable, and particularly those who, though compelled to inaction by disease, have active intellects, and quick and strong feelings.

The mode in which self-consciousness becomes a disease, although in itself a healthy and natural mental act, may be explained on known physiological and pathological principles.

The functions of all parts increase by use. By directing attention to an organ under the power of the will an increased supply of blood and of nervous influence is sent there, and the continued action of the same cause in a healthy body may make this condition permanent, or in an unhealthy one, it may produce congestion, with temporary excitement followed by corresponding depression. As the mind acts through bodily organs, when we direct our own attention to our own inward mental feelings or emotions, we are in fact directing it to the material organs of those emotions. We are directing our attention to certain parts of the nervous system, or of the internal bodily organs, and stimulating them. The effect of this in a weak organ may be to increase its powers for a time, by sending it a large supply of blood, and to be followed by collapse and debility, as the reaction from over-excitement. And as we know of other organs, the weaker they really become the more prone they may be to temporary congestions of blood or nervous power, and thus get into a permanently irritable condition,—action with weakness; so, this state at last gets beyond the power of the will which first may have produced it, by directing the attention to it.

And this in conclusion leads us to the importance of a healthy state of body. "Non sine animo corpus, nec sine corpore animus bene valere potest."

It is the full and free recognition and belief that we consist of mind and body; that the mind is the great spiritual power moving the body; the dynamic force making matter its agent in acting upon the outward world, and that in order to exert its full power, it must have a material agent fully capable of obeying its dictates, a perfect machine at its command,—which will lead us to attend to the bodily health as well as the mental, in all stages of our growth and education. And in this way an example of a large class in which the mind has been attended to, and most highly cultivated, and the body neglected, is a useful lesson. We cannot give more force to this conviction than by putting the mind here indicated in contrast with one in whom both body and mind were freely and fairly developed, the healthiest literary man both in body and in mind of our time, drawn by the most forcible and original British writer of the present day:

"Scott was a genuine man, which itself is a great matter; no affectation, fantasticality or distortion dwelt in him, no shadow of cant. Nay withal, was he not a right brave and strong man, according to his kind? What a load of toil, what a measure of felicity he quietly bore along with him; with what quiet strength he both worked on this earth and enjoyed it, invincible to evil fortune and to good! A most composed invincible man: in difficulty and in distress knowing no discouragement, Sampson-like, carrying off on his strong Sampson-shoulders the gates that would imprison him; in danger and menace laughing at the whisper of fear. And then, with such a sunny current of true humour and humanity, a free joyful sympathy with so many things. The truth is, our best definition of Scott were perhaps even this, that he was, if no great man, then, something much pleasanter to be, a robust, thoroughly healthy, and withal, very prosperous and victorious man. An eminently well-conditioned man, healthy in body, healthy in soul; we will call him one of the healthiest of men. Neither is this a small matter: health is a great matter, both to the possessor of it and to others. A healthy body is good, but a soul in right health is the thing beyond others to be prayed for, the blessedest thing this earth receives of heaven. Without artificial-medicament of philosophy, or tightlacing of creeds, the healthy soul discerns what is good, and adheres to it, and retains it,—discerns what is bad, and spontaneously casts it off. In the harmonious adjustment and play of all the faculties, the just balance of one's-self gives a just feeling towards all men and all things. Glad light from within radiates outwards, and enlightens and embellishes. Now all this can be predicated of Walter Scott, and of no British literary man that we remember in these days to any such extent, if it be not perhaps of one, the most opposite imaginable to Scott, but his equal in this quality, and what holds of it,—William Cobbett—Cobbett as the pattern John Bull of his century; strong as the rhinoceros, and with singular humanities and genialities shining through his thick skin, is a most brave phenomenon. So bounteous was nature to us; in the sickliest of recorded ages when British literature lay all puking and sprawling in Werterism, Byronism, and other sentimentalism, nature was kind enough to send us two healthy men. A healthy nature *may* or not be great, but there is no great nature that is not healthy." (Thomas Carlyle.)

Art. XVI.

Recherches et Observations sur les Causes des Maladies Scrofuleuses. Par J. G. A. Lugol, Médecin de l'Hôpital Saint-Louis, &c.—*Paris*, 1844.

Researches and Observations on the Causes of Scrofulous Diseases. By J. G. A. Lugol, &c.—*Paris*, 1844. 8vo, pp. 372.

The name and reputation of M. Lugol, and the extensive opportunities of observing disease which he has enjoyed, from his long connexion with the Hôpital St. Louis, led us, on the first announcement of the work before us, to hope that we should at length find, what has hitherto been a great desideratum in medical literature—a really good and comprehensive work on scrofula. The following pages, in which we shall endeavour to present our readers with a full, though condensed, analysis of the author's opinions, will show to what extent these expectations have been realized.

The work is divided into three parts. In the First, ***hereditary transmission of the disease*** is viewed under a variety of aspects; in the Second, the so-called ***pathological causes*** of scrofula are considered; and the Third treats of ***external causes.*** But throughout the entire volume the same one principle is constantly enunciated, viz. that scrofula is ***always*** hereditary; or, in the words of the motto which stands on the title-page, "*La santé des enfants tire son origine de la santé des parents.*"

We pass over the general observations contained in the preface and introduction, merely premising that M. Lugol agrees with most pathologists in regarding tubercle as the anatomical pathognomonic sign of scrofula; and that, in his opinion, this substance has the same origin and mode of formation as all the organs of the body; that it is itself a sort of organ which has its own particular life, as the liver and the spleen have theirs; that, like them, it is spontaneously developed; that it is a pathological production, which essentially modifies all the organic elements, and consequently their functions, and that it impresses upon the subjects affected by it a peculiar constitution, the tubercular—a constitution from which are subsequently derived the tubercles which may involve all the tissues, and an infinite number of diseases improperly called scrofulous. (Preface, pp. 5-6.) We trust our readers will fully comprehend all points of this profession of belief.

I. OF THE HEREDITARY ORIGIN OF SCROFULOUS DISEASES. This origin is recognized by two principal characters: 1, the commonness of the disease in any family; and 2, the great mortality which it occasions there.

The first is one of the most striking circumstances which meets an observer in studying scrofulous diseases; and even where the affection is not openly developed in all the members, he will notice that they all bear the external signs of a peculiar constitution, the most prominent features of which are here described.

The general conformation of the body is not good; the trunk and the extremities are not well proportioned; the head is too large, and, in general, the joints also. The median line is often not in the centre, and there is not rarely an incompleteness of union there, giving rise to separation of the linea alba and hare-lip, either simple or complicated with fissure of the palate. Some children are pigeon-breasted, the ribs being twisted and the sternum projecting upwards and forwards, so that the antero-posterior diameter of the chest exceeds the transverse. This deformity is sometimes gradually modified between the eighth and twelfth year, or at the time of puberty; but it more commonly persists, and is then a proof that the scrofulous taint is deeply rooted.

Scrofulous subjects are generally of small stature, but occasionally the direct opposite is the case; the teeth, both of the first and second dentition, but especially the latter, make their appearance slowly; they are generally of a dark colour, are prone to decay, and very friable. The spongy tissue of the bones is unduly developed when compared with their compact tissue and the soft parts. In the female pelvis this condition of parts often gives rise to serious difficulties in parturition.

The primæ viæ are in a state of atony, so that many scrofulous children never experience hunger. In M. Lugol's opinion, the cause of this is a catarrhal condition of the mucous membrane, precisely resembling that which in other parts occasions ophthalmia, otitis, bronchitis, &c., and which must be remedied before we can hope for any success in treating the general affection. In a few exceptional cases the appetite is voracious, but still the body is not well nourished. This state of the digestive canal is generally accompanied with habitual paleness of the face, a frequent precursory sign of pulmonary tubercles; with a dark circle round the eyes, fetid breath, and itchiness of the nose, giving rise to the supposition of the existence of worms. The excrementitial functions are irregular, constipation alternating with diarrhea.

The skin and cellular tissue are either abnormally delicate, or affected with an indurated hypertrophy. In many cases the skin is dry, and covered with patches of lichen or prurigo. There is a general deficiency of transpiration, coinciding with partial acescent perspirations of the feet, hands, and axillæ.

In infancy the face has an unnaturally aged look; but as years advance this passes off, and the adult subject appears much younger than he really is.

In many cases there is an extreme dislike to all exertion, and the carriage is very bad. The patients complain much of lassitude, which is not relieved but rather augmented by rest, for they feel more weary in the morning than during the course of the day. It is at this time also that the swelling of the upper lip is most strongly marked, and that ophthalmiæ are most painful. The latter circumstance is probably occasioned by the *sudden* transition from darkness to light, not by the mere influence of the light itself. This fact should be borne in mind, for M. Lugol has frequently observed that, excepting in a few instances of photophobia, much benefit is gained, in the treatment of scrofulous ophthalmia, by avoiding dark chambers, and allowing the eye to receive the impression of light.

Our author here indulges in a digression, into which it will be necessary that we follow him for a short space. He remarks, that it was from observing the general apathy of scrofulous subjects, and the injurious consequences which flowed from indulgence in this respect, that he was induced sixteen years ago to conjoin exercise with the iodine-treatment of white swellings of the lower extremities. Since that time, more than 200 cases have been thus treated, and, as he affirms, with good effect. "Why then, he asks, has not this treatment become general? Why? Because white swellings are still regarded as diseases of local origin, when they are merely the consequences of the tubercular predisposition." (p. 21.)

For ourselves, at the risk of being thought a century behind-hand in therapeutic skill, we must express a hope that this practice will not become general until far more solid reasons in its favour can be adduced, than the mere vague assertion thus laid before us. When it shall have been proved, on close and rigid scrutiny of sufficiently extensive trials, that *gelatinous degeneration of the synovial membrane*, *ulceration of the cartilages*, and *caries of the bones*, all of which, and more, are included under that most comprehensive and indefinite term "white swelling," when, we say, it shall have been proved that these various conditions, accurately diagnosed, are most speedily and effectually remedied by motion, then, and not till then, will we counsel our readers to yield their judgments captive to

M. Lugol, and set their patients promenading through the wards. But to return.

A certain degree of bodily activity is not incompatible with the scrofulous diathesis, but its effects, instead of favoring the growth of the body, rather act injuriously upon it, the system being unable to repair the waste.

The organs of generation are usually slowly developed. In the male, one testis frequently remains in the abdomen—more rarely both. In the female, menstruation is tardily established, and is seldom normal. Dysmenorrhea is very common. The mammary glands and ovaries are equally slow in their growth. But there are exceptions, as every one knows, to this rule.

In many cases, but not so commonly as is often supposed, the face shows evident marks of the constitutional taint, in the tumefaction of the upper lip, of the alæ nasi, the eyelids, especially their free border, and the lobule of the ears. These partial indurations, when they exist, are most significant. In the swollen upper lip M. Lugol states that he has recognized the first invasion of pulmonary tubercle; we suspect it was merely the predisposition, not the actual disease, that was thus revealed.

M. Lugol denies the frequency of *embonpoint* in scrofulous females. We need not say how deceptive is the appearance, when it is manifested—nor how easily a little attention will detect its true nature.

According to our author's experience few scrofulous persons have light hair; in more than half the hair is black, and among the rest a dark chesnut is the most usual colour. The same remark applies to the eyes and the skin, which are both most frequently of a dark tint. In our own country, the opposite is most commonly observed.

All these signs are rarely united in the same individual, their combinations are infinitely varied, and sometimes they are so slightly pronounced that a sharp eye is needed for their detection. Occasionally, too, some members of a decidedly scrofulous family, will pass through the greater portion of their life without exhibiting any marks of their hereditary taint, but at last, at an advanced age, fall victims to the disease, the germs of which they have borne within them, from birth: of this M. Lugol narrates some striking examples. (Vide p. 36 et seq.)

Lastly, we may observe, that, as a general rule, (but to this, though they are unnoticed by our author, there are numerous remarkable exceptions,) the intellectual faculties are below par. There may be some vivacity of mind, or even in youth precocity of intelligence, but there is a want of energy, of steadfastness, and power of concentrating the thoughts, which renders the unhappy inheritors of this temperament incapable of continuous exertion, and therefore of great deeds. In the words of the writer, "they experience no sustained activity of the physical desires, of the intellectual faculties, or of the moral sentiments; they have nothing normal, nothing powerful, nothing durable. All the phases of their existence fall short of their end; they have neither puberty nor manhood; the obstacles to their growth never cease to act; their physical and intellectual development remain unaccomplished."

In reference to the *general existence* of scrofula in any family, M. Lugol affirms, that if one of the children exhibits signs of the disease, we may, even without seeing the others, confidently assert that they also have the same malady. For, he remarks, if there be one thing impossible in pathology, it is that a tuberculous child should have brothers and sisters who are not so. Should any of our readers be inclined to doubt the correctness of this sweeping assertion, we beg to inform them that a very little thing will sign the condemnation of any individual in M. Lugol's eyes: it is quite enough for him if there be a somewhat troublesome ophthalmia or coryza, if the person be of rather stunted growth, or even if his spinal column slightly deviate from the perpendicular.* Apply these rules to all around us, and how many would remain untainted?

Setting aside, however, these exaggerations, which are perhaps natural to one who has made any particular disease his special study, the main fact is unquestionably true, and is a striking feature in the history of scrofulous diseases. We must refer to the work itself for many interesting examples; some of which are also amusingly illustrative of the hearty zeal with which the writer hunts out every token of hereditary taint.

We turn to a more melancholy picture—the *mortality* in scrofulous families. This is often frightful. Half the number of scrofulous children die during the first years of their life. We often see families in which not more than one or two children remain out of six or eight, or even a larger number: in many cases, none survive. M. Lugol is correct in affirming that such results as these can only originate from hereditary influence; external causes would scarcely effect such waste of life, and certainly would not work such mischief by inducing scrofulous diseases *alone*.

The *second section* in this chapter is occupied with the narration of numerous examples of the simultaneous manifestation of scrofula in the different branches originating from the same family stock; but on this point we need not delay, as our readers must have often witnessed parallel instances.

In the *third section* we are presented with another proof of hereditary transmission, in the health of the offspring of different marriages. Thus, if a man of sound constitution marry a scrofulous woman, he will have scrofulous children by her. Should he afterwards have a family by another, in good health, these will be free from taint, and vice versâ. Or, again, if a scrofulous individual be twice married, all the progeny will inherit the disease, whatever be the condition of the other partner. We suspect that to this rule also exceptions are not uncommon.

Having thus shown, that when scrofula exists in a family, it generally attacks all the children, and proves fatal to a large number during the early years of their existence, our author next proceeds to search for the origin of this hereditary taint, by examining into *the health of the parents who produce scrofulous children*. And here he remarks that he is far from affirming that all scrofulous persons are descended from parents who are themselves the subjects of the same disease: he believes that there are many causes by which a previously sound individual's health may be so effectually lowered, that his progeny will be born inheritors of this sad malady. In prosecuting our inquiries, therefore, it will be necessary to examine the subject under two aspects: first, as regards the *original*, and, secondly, as regards the *acquired* health of the parents.

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* We should mention here, that throughout the volume *rachitis* is spoken of as a scrofulous disease; and that very frequently the presence of *scirrhus* is appealed to as evidence of the strumous taint. The same remark will apply to *bronchocele* and *cretinism*, both of which are traced, we believe erroneously, to the same general cause.

1. *Of the original health of parents who produce scrofulous children.* When the children's disease depends upon the original health of the parents, the latter are always scrofulous or tubercular. In most instances the external manifestations are sufficiently evident, and M. Lugol narrates a number of interesting cases, in which the disease was repeated in the children, under precisely the same form as it existed in the parents. He has frequently traced the contamination through three generations, and observes that when this is the case, the members of the last generally die almost at birth. He remarks also that he has never met with an instance in which the law of transmission has failed, when the father has been at fault, but has noticed a few rare exceptions when the taint was confined to the mother. This appears to us somewhat at variance with the dogmatic assertions of other parts of the work, though we make no question of its truth.

The next article touches upon a subject of the deepest interest and importance—the connexion between scrofula and phthisis. M. Lugol believes them to be identical; and laments grievously and, as we think, without just reason, that the profession should so generally regard the latter disease as a mere local affection, arising from inflammation. We are much mistaken if this be the *common* opinion; and assuredly Laennec is not, as the author insinuates, guilty of having either originated or built it up. In truth, he directly argues against it. "A multitude of facts," he says, "prove that the development of tubercles is the result of a *general condition of the body;* that it takes place without previous inflammation; and that when inflammation coincides with tuberculous affection, it is most frequently posterior to it in its origin." (Forbes's Laennec, 3d edit. p. 303.) But to proceed. M. Lugol enunciates three propositions: 1, that scrofula has most frequently a tuberculous origin; 2, that these two diseases generally coexist in the same family; and, 3, that all scrofulous persons are the subjects of pulmonary tubercles: these he seeks to establish in the following manner.

More than half the number of scrofulous subjects are descended from phthisical persons. Scrofula, under all its various forms of diseased cervical and mesenteric glands, white swellings, caries, ophthalmia, bronchitis, intestinal worms, hydrocephalus, &c. rages in a family for no other reason than that one of the parents has pulmonary tubercles. And how is this proved? Simply from the fact, that in two wards containing eighty-four beds, M. Lugol has commonly ascertained from more than half the patients that their parents were phthisical. How far these patients were accurately informed regarding the disease of which they spoke, we know not; but we can see that the inquirer was easily satisfied, for he remarks: "In most of these cases, in fact, we were told that the parents had been of a feeble constitution, and that they died young: two circumstances from which it is extremely probable that they died of pulmonary consumption." (p. 92.) We leave our readers to decide upon the philosophical caution displayed in this passage, and would beg them to contrast with it the words employed by his countryman, M. Louis, in referring to a parallel investigation.*

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* Louis; Researches on Phthisis—Sydenham Society's edition, p. 485.

In support of the *second* proposition, he narrates several cases in which the fact was observed.

In regard to the *third*, he says: Phthisis is the natural death of scrofulous persons; it might even be affirmed that they die in no other way. The subcutaneous tubercles, ophthalmiæ, ulcers of the skin, caries of the bones, &c. only terminate fatally after the lungs have become tuberculous. This is a bold assertion—one that should be made only on the strongest grounds; but the sole confirmation with which we are favoured is the narration of two or three cases, and the statement that in *nearly all* the autopsies of scrofulous patients at St. Louis, tubercles have been found in the lungs. The exceptional instances are quickly dismissed, with the satisfactory remark, that when these morbid products are not discovered—the predisposition existing—it is because the time for their invasion had not yet arrived! (p. 96.) We are, however, promised a special work on tubercle, in which, perhaps, better reasons may be adduced.

It is not possible in every case to detect, at the first glance, the evidences of constitutional taint in the parents of scrofulous persons; and many, therefore, from confining their inquiries to a few superficial questions, have fallen into the error of attributing to external causes an influence which they did not really possess. The self-love of parents is also extremely apt to lead the investigator astray; and in numerous instances, even when his suspicions are most fully aroused, he will find it no easy matter to bring them to confess, that they themselves, or any of their family, have been previously afflicted with so hated and terrible a malady. It is necessary to bear these facts in mind; and we would direct the attention of our readers to the two next articles of the work before us, which contain some interesting examples of the propagation of scrofula by individuals who had suffered from the disease in youth, though at the time of the birth of their children they were in good health; or who had themselves never been affected, but merely belonged to a family, other members of which were scrofulous. These cases are also deserving of notice, because they exhibit in a very favorable light the practical acumen which the author frequently displays.

2. *On the acquired health of parents who produce scrofulous children.* Healthy persons have a healthy progeny. But there are many circumstances which, by their deteriorating influence upon the constitution, render an individual incapable of generating sound descendants. In the foremost rank of these M. Lugol places *syphilis.* He is no believer in the identity of the two diseases, though he recognizes their many points of resemblance; nor in the *general* origin of scrofula from the syphilitic taint; but he has frequently observed instances in which no other cause could be assigned, and which he therefore attributes to this. From among the few cases narrated we select one, as the most striking illustration. The youngest of a family of three children was scrofulous, the others being in perfect health. Their father had led a regular life, and enjoyed good health, until after the birth of the two eldest. He subsequently acquired disorderly habits, and contracted syphilis, with which disease he infected his wife. The child born under these circumstances presented evident marks of scrofula. Our author believes that it is the sy-

philitic cachexia alone that can produce this effect: primary syphilis can only propagate itself.

Venereal excesses are often followed by the same result. This M. Lugol attributes to the fact, that the seminal fluid is ejaculated immediately after its secretion, and without having been fully elaborated in the vesiculæ seminales.

We can merely indicate the other causes enumerated. They are, too early and too late marriages, disproportion of ages, want of relative physical force in the father, paralysis, epilepsy, insanity, &c. On many of these the evidence adduced is anything but satisfactory, and the reasoning most loose. We refer especially to one case, that of a young lady who exhibited signs of scrofula, after suffering from a mucous fever. M. Lugol totally rejects the idea that this fever had anything to do with the subsequent disease, though the only other cause he can assign is, that the father, a healthy man, was of a modest complexion, appeared feeble when compared with the " rich, abundant energetic organization of his wife!" (p. 142.)

In the next chapter, which is little more than an appendix to the preceding, we find a few points which demand a passing notice. In some cases, parents present no appearance of scrofula until some time after the disease has shown itself, or even proved fatal in their offspring. We do not controvert the fact, but must submit that, as a proof of the doctrine at which our author continually aims—the exclusively hereditary origin,—it is essentially inconclusive, for we are in no way enlightened as to the cause of the disease in the parents. For anything we know, the whole family may have been subjected to such external influences as, in the opinion of most pathologists, would suffice to develope scrofula, independently of any constitutional taint.

M. Lugol totally denies that scrofula ever passes over one generation, and reappears in the next. If this were the case, he says, a man would give what he does not possess—there would be effects without a cause. How, then, we would ask, do syphilitic persons and others produce scrofulous children, as he has endeavoured to show they do?

In an investigation of this nature, the observer must not always expect to find one cause alone the efficient agent; he will meet with many cases in which several have united their injurious influences. This was especially noticed in France, at the time when the conscription was in full operation, and our author draws a lamentable picture of the disastrous effects upon the nation at large which this mode of repairing the losses in the field produced. We must refer to the work itself for a fuller view of this subject, and also for an exemplification of the noxious effects not uncommonly produced upon scrofulous individuals by the excitements incidental to the married state. M. Lugol argues strongly, but we suspect without much chance of success, upon the expediency of a law to prevent the marriage of such as are affected with the disease in question.

Scrofula is peculiarly rife among the inmates of the foundling and orphan institutions of Paris. It is quite clear that, in the great majority of these cases, it is altogether impossible to adduce anything like evidence of hereditary transmission; while at the same time the external circumstances in which these miserable outcasts are placed are especially fitted to engender disease. True to his principle, however, M. Lugol, though admitting the influence of the latter in developing the affection, still affirms that it has been impressed upon these individuals from their birth, assuming that the parents were in such conditions as to render them incapable of having a sound progeny.

The subject next brought before our notice—viz. the production of scrofula in an originally healthy child, by using the milk of a scrofulous nurse—would, we think, find its more correct place in the succeeding portion of the work; for it is clearly the operation of an external influence. That the milk of a scrofulous woman should be unfit for the healthy nourishment of a child, is extremely probable; but we can see no analogy between this and the propagation of a disease by generation. In the one case the primitive elements of the future being are subjected to a morbid impression from the first moment of their receiving the vital impulse; in the other there is merely an imperfect elaboration of the materials destined to nourish a being already fully organized. The case, indeed, would be widely different, could we believe in the existence of a scrofulous *virus*, capable, like the syphilitic, of being propagated by infection; but all the results of experiment and observation are totally opposed to such an idea, as will be seen hereafter.

II. Of the pathological causes of scrofula. It has been frequently remarked, that the first development of scrofula can be dated from an attack of some other disease, and more especially of variola, rubeola, or hooping-cough; and many have regarded these as the real generating causes of the subsequent malady. We need scarcely say that M. Lugol utterly denies the correctness of such an opinion. In his view, they are merely the agents which call into activity an already existing predisposition. We have no intention of pronouncing any deliverance upon the point at issue; but we must record it as our decided opinion, that in the pages before us there is no *proof* whatever. It is not by bold assertions, and the narration of a few isolated cases, that a question of this nature can be determined to the satisfaction of an inquiring mind; and we appeal to all who may read the original, if we are presented with anything more than this. By far the greatest portion of the paragraphs allotted to the subject are occupied with an exemplification of what nobody denies, viz. that these diseases affect most severely and fatally those who possess a scrofulous constitution.

But there are other affections to which the same influence has been attributed; as mucous or catarrhal fever, the fever of growth, difficult dentition, intestinal worms, &c.—these M. Lugol regards, and probably with justice, not as causes, but as particular forms of the disease itself.

Again, when a woman becomes scrofulous during pregnancy, or after abortion or labour, it is merely the constitutional disease that is called into activity by these different states. Indeed he believes that the great majority of spontaneous abortions and of difficult labours are caused by the existence of the scrofulous taint.

Erysipelas is a frequent precursor of scrofula, especially of that form in which the skin and subcutaneous cellular tissue are chiefly affected. It is also a very common complication occasionally proving fatal, but most generally exerting a favorable influence upon the pre-existing disease. When occurring under these circumstances, its progress is usually tedious, and the favorable termination is always ushered in by the appearance of some critical evacuation, or of purulent collections in the glands or cellular tissue. In M. Lugol's opinion, it is never a *cause* of scrofula. May not its extreme frequency in the wards of St. Louis be in some measure connected with the iodine-treatment, to which it appears all the patients are subjected?

Syphilis and *gonorrhea* are among the worst complications of scrofula. They often light up the disease in those who are predisposed; they render it much more severe and intractable in its nature; and they frequently produce relapses in such as seemed to have been cured. Does M. Lugol regard these two affections as identical, that he includes them both under the one term, syphilis?

III. OF THE EXTERNAL CAUSES OF SCROFULA. To the discussion of this subject, the third and concluding portion of the work, is devoted.

There are some localities where the disease prevails to such an extent, that it may be said to be *endemic*. Authors are much at variance as to the cause or causes of this, but humidity has been generally supposed to play an important part. Against this doctrine our author strenuously, and, it appears to us, successfully contends, by showing that the disease is endemic throughout provinces of considerable extent which are not damp, but have a dry climate; and that it does not prevail in many places which are extremely humid. Recent observation of the sort of antagonism between intermittent fever and tubercular disease of the lungs, also opposes such an idea. But while we are thus shut out from one explanation, the researches of M. Lugol, furnish us with no other; for though he severally accuses importation, the non-mercurial treatment of syphilis, and inter-marriage of relatives, as the cause of these local peculiarities, we must, in the absence of all proof, decline acknowledging these as the originating powers.

Yet, although humidity *alone* is thus inoperative, may it not, when acting in concert with other depressing agencies, such as variableness of climate, insufficient food and clothing, too severe, or too prolonged toil, bad air, &c., occasionally engender scrofula? To this also, M. Lugol replies in the negative; but we cannot regard his evidence as conclusive. For, though he narrates several cases which at first sight appeared to favour the affirmative but on a more rigorous examination proved to be merely examples of hereditary transmission; and though he affirms, certainly upon slender grounds, that scrofula is not common among the inmates of prisons, of camps, and of ships, who are often exposed to all these influences, we feel that something more is wanting, and that in the absence of official returns,* or of systematic personal investigations, carried on for a sufficient length of time, the *positive* results of other observers must weigh most heavily in the balance.

In like manner, and with equal looseness of argumentation, he affirms that *climate* can have no influence in the production of scrofula, because the disease exists in all countries, and everywhere presents the same phenomena. At the same time, he acknowledges that the natives of tropical climates very commonly fall victims to strumous diseases, when brought into our temperate regions. Are we then to assume at once that all these had the congenital predisposition? That the monkeys and other animals continually dying in our menageries and zoological gardens, have all had scrofulous parents? and that Mr. Newport succeeded in tuberculizing his larvæ, only because these unfortunates shared in an hereditary taint? The idea is too ridiculous to be entertained for one moment.

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* Are there no *medical officers* connected with the prison of Poissy who could have answered M. Lugol's inquiries regarding the prevalence of scrofula more satisfactorily than the one prisoner whom he consulted, or even than the person who was in the habit of employing the prisoners as his workmen? We receive such constant proofs of the ignorance of patients concerning their own diseases, that we place small confidence in their reports of the diseases of others.

The other supposed causes of scrofula, viz., vaccination, onanism, and conception during the menstrual period, may be passed by unnoticed. Nor is it necessary to do more than remark that we entirely agree with M. Lugol in his belief, that the disease with which we have been occupied is *non-contagious*, and that we most heartily join in his condemnation of the unwarrantable practice of inoculating healthy individuals with matter taken from diseased subjects.

We should observe, that of all the seasons of the year, spring is the most obnoxious to scrofulous subjects; the first attack of the disease is often developed at that time, and it is during the same period that relapses are most common. A knowledge of this fact may assist our prognosis, for when in the course of any case, it is observed that each spring exacerbation diminishes in severity, we may anticipate an eventual cure.

In regard to *treatment* we can say but little, for it is scarcely alluded to by the author; but from occasional passing remarks we can gather that *iodine* is still his sheet-anchor; and as no notice to the contrary is given, we presume the plan adopted now is the same which has been already for many years before the profession.

Our analytical task ends here, and it only remains for us to record our general opinion of the work with which we have been engaged. That opinion, we regret to say it, is not favorable. As a scientific production, the book is equally unworthy of M. Lugol's reputation, and of his position. We have examined it, as our readers will perceive, with care; but have failed to discover that he has thrown any new light upon the pathology of the disease, or developed any views that might have guided us to more successful treatment. And in regard to that one doctrine, in support of which his strength is so lavishly expended, the exclusively hereditary origin of scrofula, we remain still unconvinced. Freely conceding that it may have been often overlooked where it really did exist, and that in all probability it is more generally operative than many suppose, we should yet require much stronger proofs than any here adduced, ere we are led to banish all other causes from our thoughts. For reasons best known to himself, M. Lugol totally disregards the numerical method of investigation; in the whole volume there is not one calculation. And yet we are assured no question of this kind can ever be settled in any other way. Such vague expressions as *most*, *a large number*, *a few*, &c., well enough in ordinary conversation, and passable, perhaps, in academic prelections, are altogether out of place in a treatise that claims the title of 'Researches.' They carry conviction to none but those whose approbation is without value.

To one other thing we must refer, because, though bad at all times, it is peculiarly reprehensible in professional writings; we mean the tone of special pleading, which has so constantly jarred upon our feelings in the perusal of these pages. Trivial circumstances are magnified beyond due bounds, when by so doing the author's purpose might be served; and the very same are passed by without comment, when to have insisted on them would injure his argument. Take one example. Leucorrhea, uterine hemorrhages, &c. are included among the signs of scrofula, and a woman subject to these would, in other parts of the work, be accused of transmitting the disease from herself to her child; but when the same symptoms are mentioned as resulting from the mode of life of some females in Paris, (p. 154,) these females are not regarded as strumous, because if they were so, it would be an example of the generation of scrofula from external causes. One-sidedness of this kind is no less injudicious than unfair; the sole effect it can produce is to weaken our confidence in other statements, and cause us to regard with suspicion what may be really deserving of belief.

ART. XVII.

1. *First Report of the Commissioners for inquiring into the State of large Towns and populous Districts.* 1844. Folio, pp. 682.
2. *Facts and Observations on the Sanitory State of Glasgow during the last year; with Statistical Tables of the late Epidemic, showing the connexion existing between Poverty, Disease, and Crime.* By ROBERT PERRY, M.D., President of the Faculty of Physicians and Surgeons, &c.—*Glasgow,* 1844. 8vo, pp. 38.
3. *The Glasgow Bills of Mortality for* 1841 *and* 1842, *drawn up by appointment and under the authority of the Lord Provost, Magistrates, and Town Council.* By ALEXANDER WATT, LL.D. City Statist, &c. 1844. 8vo, pp. 106.
4. *Observations on the Epidemic Fever of* 1843 *in Scotland, and its connexion with the destitute condition of the Poor.* By WILLIAM PULTENEY ALISON, M.D. &c. &c.—*Edinburgh and London,* 1844. 8vo, pp. 80.

I. IN May of last year a royal commission was issued for an inquiry into the state of large towns and populous districts. The principal points embraced in the commission were, "the causes of disease among the inhabitants; the best means of promoting and securing the public health, under the operation of the laws and regulations now in force, and the usages at present prevailing with regard to—the drainage of lands; the erection, drainage, and ventilation of buildings; and the supply of water in such towns and districts, whether for purposes of health, or for the better protection of property from fire; and how far the public health and the condition of the poorer classes of the people of this realm, and the salubrity and safety of their dwellings may be promoted by the amendment of such laws, regulations, and usages."

It will be seen that her Majesty has, in fact, committed to the Duke of Buccleuch and his colleagues the important duty of devising a system of public hygiene for the United Kingdom. May we comment on that simple word, *important?* What should not a system of public hygiene be for Great Britain?—the land of mines and machinery; of railways and steam-ships; the land of an overflowing, industrious, energetic population; the land of strong intellect in unceasing action;—the sun and centre of modern civilization;—the beacon set up as a guide to all nations? What should not a system of public hygiene be, to harmonize with so wondrous a development of social life, and to equal so high a destiny? The duty of the commissioners is indeed important; and *that* we know they feel it to be.

This, their first Report, is confined to a statement of their proceedings, and a notice of such portions of the evidence, reports, and documentary information which best display the course and progress of the inquiry. Among the principal witnesses on the general subject are physicians. The commissioners observe:

> "We would refer, in the first instance, to the evidence of Dr. N. Arnott and Dr. Southwood Smith, who have stated to us the results of their continued and latest observations; and also to the evidence of Dr. Guy, Dr. Aldis, Dr. Rigby, and Mr. Toynbee, who have had extensive practice in hospitals and dispensaries. This evidence, with that of Mr. Ward, displays their opinion and experience, that defective drainage, neglect of house and street cleansing, ventilation, and imperfect supplies of water, contribute to produce atmospheric impurities, which affect the general health and physical condition of the population, generating acute, chronic, and ultimately organic disease, especially scrofulous affections and consumption, in addition to the fevers and other forms of disease to which public attention has hitherto been chiefly directed by previous sanitary inquiries, and which are more distinctly noticed in the returns annually laid before Parliament under the provisions of the registration Act." (p. vii.)

The drainage of the metropolis was inquired into, in the first place; and a series of questions, embracing the subjects of inquiry, were also transmitted "to the municipal and other public officers in fifty towns where the rate of mortality appeared, by the returns of the registers of deaths, with a few exceptions, to be the highest. These include the largest manufacturing towns and the principal ports after London, and contain a population of more than three millions of persons. Each of these towns was afterwards visited by one of the commissioners, who examined on the spot the general condition of the town, and of the most crowded and the most unhealthy districts, making personal inquiries of the inhabitants, and hearing such statements as were made by them, or respecting them, by medical or other officers." In addition to these investigations, special inquiries were "promoted by others into the sanitary state of several towns and populous districts, more especially of those places where the growth of the population has been attended by a high rate of mortality. Some of these renewed inquiries have been of a closer and of a more comprehensive nature than those previously made, and have been conducted by persons of special qualifications, from long attention to the subject, and acquaintance with the habits and condition of the population, thus possessing the best means of insuring approximation to accuracy." A series of reports were thus obtained: on Liverpool, by Dr. Duncan; on Preston, by the Rev. J. Clay; on Nottingham, by Mr. Hawksley, civil engineer; and on York, by Dr. Laycock. In the latter, to use the words of the commissioners, "a further advance made in the investigation of the causes of mortality, is displayed in the report tracing back, for upwards of two centuries, the operation of like physical causes, in the production of different forms of epidemic disease prevalent under similar conditions, always in the greatest intensity in the same quarters in the ancient metropolitan city and county town of York."

The inquiries of the commissioners as to drainage, commence with an examination of the existing laws and of the local acts. The usages prevailing and the bye-laws in force were found, with one or two metropolitan exceptions, to be framed with a view to the maintenance of the drainage of surface water only, and without reference to house drainage and sewerage, and the constant removal of all decomposing vegetable or animal refuse. In some of the larger and more crowded towns, all entrance into the sewers by house-drains, or drains from water-closets or cesspools, is prohibited under a penalty! In scarcely one of the fifty towns examined by the commissioners can the drainage be said to be complete and good; in forty-two it is decidedly bad. The commissioners found that municipal patriots are apt to overrate the purity and excellence of the drainage in their respective towns, and to pronounce that good, which the commissioners found was only a limited drainage of a few principal streets; the more crowded portions having, in fact, no drains whatever.

An important point specially noted by the commissioners is this—that main-drains or sewers may be tolerably well formed, and subordinate or house-drains attached, but from the want of proper supplies of water, both house-drains and main-sewer act only as extended cess-pools. Of course, the miasm from many miles of extended cess-pools must be highly injurious to the health. The commissioners, therefore, next inquired into the mode in which water was supplied to towns, and also into its purity and quantity. "Upon the examination of the statements and answers from the towns to which our inquiries have been directed, it appears that only in six instances could the arrangements and the supplies be deemed in any comprehensive sense good; while in thirteen they appear to be indifferent; and in thirty-one so deficient as to be pronounced bad, and, so far as yet examined, frequently inferior in purity." Preston and Nottingham are the towns best supplied with water and at the cheapest rate.

Finding matters so bad as respects drainage and cleansing, and the necessity for amendment so obvious, the commissioners next inquired into the practicability of carrying out sanitary improvements. On the part of science and practical skill nothing seems wanting; the great obstacle is the cost of the necessary arrangements. We will allow the commissioners to speak for themselves on this point.

"We have inquired carefully as to the practicability of reducing the expenses of works for house and main-drainage, and for carrying supplies of pure water into all houses, so as to bring them within the pecuniary means of the poorest class of inhabitants. Mr. Anderton, manager of the Preston water-works, shows that the cost of new supplies may be reduced to one sixth of the former expense, if the use of water-butts be dispensed with in new districts, by the adoption of the principle of a constant instead of the present intermittent supply, and if the tenants' communication-pipes be comprehended in one contract for construction and maintenance. Mr. Quick, engineer of the Southwark Water Company, states in his evidence founded on data from experience in the metropolis, that the expense for the immediate outlay might be reduced to one fourth of the existing charge. The evidence of Mr. Hawksley exhibits the nature of the data for his important conclusion, that the result, accomplished in the town of Nottingham, is of possible attainment in many other extensive town districts in this country, and that an abundant supply of pure water may be carried into each of the lowest class of tenements, at a charge (giving a fair remuneration for the capital invested) which might not exceed 5*s*. a year, or about one penny weekly for each tenement. The same witness states, the small additional cost at which the water may be filtered, when requisite, and describes the precautions necessary to insure its purity." (p. xii.)

The same witnesses state, with reference to house-drainage, that a saving may be effected of from one half to one third of the existing charges by the substitution of impermeable tube tile drains of a superior construction, for the common brick drains, which allow the decomposing liquid refuse to permeate through the foundations.

In the Holborn and Finsbury division, " by the adoption of a system of cleansing by flushing or flooding with water, the accumulation of deposits of decomposing substances has been prevented in a large proportion of the sewers; and by rendering unnecessary the mode of cleansing by hard labour and cartage, (at once unhealthy and expensive,) 50 per cent. of the former expense has been saved."

Some interesting instances of the application of the refuse of towns thus obtained to agricultural purposes is given. The instance of Edinburgh is already familiar to the public; a similar application has been in long and successful practice at Milan, and less extensively at Ashburton in Devonshire. Mr. James Dean, who has considered the means of applying all or part of the sewerage of London to agricultural purposes, states that land worth from 30*s*. to 40*s*. per acre, when so improved, is worth from £8 to £12 per acre. At Ashburton, they have grass for the ewes and lambs a full month earlier than in other lands not so manured, at a time, of course, when lambs are the most valuable. We have reason to believe that steps are being taken to collect and apply the underground wealth of the metropolis, now lost in the Thames, to agricultural purposes. Mr. Dean's plan is to carry the sewerage of parts of London, north and east of Whitechapel road, Leadenhall street, &c. into the marshes north of West Ham and Plaistow, and thence to the river Thames below Woolwich. There he would form a series of filtering ponds, say twelve in number, of six acres and a half each. The fluid portions would be let out so as to irrigate the marshes below, of which there are several thousand acres, and the solid " barrowed" from the bottom of the settling ponds into barges. Captain Vetch devised a plan of this kind for Leeds, and estimated the *clear* annual gain at 9000*l*. sterling. Mr. Roe strongly recommended the adoption of this method to the authorities of Derby. " My opinion is," he says, " that if the sewer water were properly applied there, it would pay the expenses of the sanitary improvement of the town."

The minutes of evidence follow the Report, and occupy 470 folio pages; the appendix contains the special reports before alluded to, and other documents. We shall consider the facts they contain as contributions to hygiene, and place them as such before our readers.

Water. The evidence of Dr. Clark, professor of chemistry in Marischal College, Aberdeen, is first detailed. Dr. Clark distinguishes the whole saline contents of water as the neutral and the alkaline. The latter consist entirely of bicarbonates—those of lime and magnesia, and of potash and soda; the former of neutral salts of earths and alkalies. The earthy salts are, of course, the cause of hardness, but Dr. Clark has had occasion to observe that carbonic acid, in excess, renders water hard. Exposure to the air softens it, and boiling too; but this does not affect the water that is hard from neutral earthy salts. The hardness and softness have important pecuniary relations. The wear and tear of linen is much greater when washed in hard water, and, of course, the cost of soap is greater. Dr. Clark has invented and patented a method of testing the hardness of water in ten minutes. He adopts a graduated scale, in which the hardness, caused by one grain of chalk to a gallon, is expressed by one degree. He examined some of the London water; the result of his inquiry was that the hardness differed according to the state of the weather. In May 1841, the water of the Thames at Mortlake was 14·4°; of the New River 13·2°; of the East London Company's water 16·1°. In August the proportion of earthy salts was less, the degrees being 11·8°, 12·0°, and 14·1° respectively. Dr. Clark distinguishes between neutrally and alkalinely hard waters. Thus, the East London water being 16·1° of hardness, is of alkalinity 15·6°, that is, it requires as much acid to neutralize its alkalinity as 15·6 grains of chalk per gallon would require. The Thames and New River stand generally at 12° hardness and 11° alkalinity; while that of Manchester is 12° hardness and 7° alkalinity. The degrees of alkalinity express the degrees in which the water is softened by boiling. The well waters of the metropolis are much harder than that of the Thames. Torrington-square well stood at 80° of hardness; that at the gate of University college at 32°; the water from Red Lion-square pump indicates 61·5°. The artesian wells are much softer. Those at Apothecaries' Hall, at Combe's, and at Truman and Co.'s (breweries), were each about 5·5°. These last are decidedly alkaline, even to the taste when boiled: they are, in fact, solutions of carbonates of soda or potash. Dr. Clark describes his process of purifying water from bicarbonate of lime by the addition of lime water. He estimates the whole amount that might be thus precipitated from the water supplied to the metropolis by the water companies at 9000 tons per annum. The water thus purified falls to 4·2° of hardness, and acquires the same softness as if one part of the original water were mixed with three of distilled water. One hundred imperial gallons of the purified and unpurified water would require the following proportions of soap to form a lather:

	Unpurified.	Purified.
East London	31 oz.	10 oz.
Thames	28 —	10 —
New River	26 —	9 —
Gade, near Watford . . .	36 —	9 —

Dr. Clark estimates the annual value of soap consumed per head in London at 6*s*. 8*d*. The total value, including carbonate of soda, is £630,000, so that the difference of cost, between the supply of hard and soft water, in the matter of soap, may be estimated for London at £200,000 per annum.

Organic remains in water. Dr. Clark states (no doubt correctly), that the organic matter which passes into the water from the sewers is not separable by filtration. He is of opinion that undue weight has been given to its presence, and that it is assuming far too much to say that such animal matter as passes into the river remains there unchanged, especially when it is remembered how freely a river is exposed to the action of the air, and that there are other substances present, as the alkalies, so well adapted in such circumstances to increase chemical action. Professor Brande frequently found the *well* water of London contaminated by organic matter and ammoniacal salts; and this appears to be the case in other towns, as in York, for example, in the neighbourhood of churchyards. Dr. Clark found animalculæ to abound in the water of all the London companies. His attention was accidentally directed to them, and he counted, with the naked eye, no fewer than fifteen monoculi in a quarter of a pint. The impure water supplied to London is indirectly the cause of drunkenness, by exciting disgust for the pure element, and a taste for fermented drinks.

Filtration of water. A great variety of information under this head is accumulated in the report. Dr. Clark describes two principal modes, the one being performed by the natural filter, the other by the Lancashire filter. The first was applied on an extensive scale at Glasgow; a few miles above which city, on the bank of the Clyde, there is a very extensive convex round bank of sand used for the purpose. The Lancashire filter is the common one, with sand and gravel. Mr. Thom describes the self-cleaning filters, erected by him at Greenock, Paisley, and Ayr, which differ very considerably from the Lancashire. Over the puddled bottom of the filter a hollow floor, made of perforated tiles, is laid. Upon these are placed six successive layers of gravel, each an inch in thickness, and increasing in fineness upwards, the last being, in fact, coarse sand. Over this there is laid very fine sand to the depth of two feet, about six or eight inches at the top, being mixed with animal charcoal roughly powdered. Coloured drawings and sections are given in detail. Mr. Thom, having discovered that moss water was rendered pure and colourless by flowing over or through a particular species of lava or trap-rock (amygdaloid), was induced to substitute this rock for the animal charcoal, which he did with complete success. In an essay inserted in the appendix, Mr. Sloper argues strongly against the utility of animal charcoal, while he recommends Maurras's system.

Supply of water to towns. From the varied evidence of civil engineers, there appears no reason to doubt the easy practicability of supplying every house with an abundance of water at a cheap rate. In the first place, water may be kept night and day, in the pipes, in every house, at high pressure, so that no cistern would be required either for domestic purposes or water-closets; nor would there be any need for a fire-engine. At Greenock, Paisley, Ayr, Nottingham, Preston, New York, and Philadelphia, the mains being always full and at high pressure, nothing more is requisite for the extinction of a fire, than to put the hose of a fire-engine on one of the fire-plugs, which are attached to the pipes at short distances along all the streets. The water is then projected at once over the highest house. In New York, the fire insurance is 25 per cent. lower, and in Philadelphia the risk is reduced in the proportion of one to three, since the old system was abandoned. In Preston, the owners of mills are introducing water into every story, a plug and hose being placed in each. The watchman can manage the hose quite well, and the mill-owners are now so confident of the success of the plan, that they do not insure the building. In Philadelphia, the servants wash the streets and fronts of the houses by hose attached to the plugs. There can be no doubt that with a proper system of hydraulics, the whole of the refuse from the houses and streets might be washed away at very little comparative cost, or in fact with a profit, if the system of irrigation with the sewage be adopted. The desolating effects resulting from a deficient supply of water for the purpose of cleansing, as depicted by Dr. Arnott, Dr. S. Smith, and others, are strongly in contrast with the statements of Mr. Ashton, a manufacturer of Hyde, who having about 320 houses there, supplied every dwelling with water. The morals and domestic comforts of the people immediately improved.

The atmosphere. The vitiation of the atmosphere is manifestly one of the most important subjects of inquiry in the Report.

Effects of deficient ventilation. On this point we have first to notice the evidence of Dr. Arnott. Dr. Arnott reiterated the opinion he has formerly given, and gives statistical data in proof that one great cause of fever is deficient ventilation. The greater number of cases occur among workpeople, at a time when they have plenty of work (and of course plenty of food, so that starvation cannot be a cause of fever,) but at a time, too, when they are confined in their workshops, and breathe impure air for many hours daily. Evidence to this effect is quoted from the statements of the medical officer of the Spitalfields Union. At Glasgow also, Dr. Davidson observed that by far the greater proportion of cases of fever admitted into the infirmary occurred in persons in ordinary condition, to use a jockey term; indeed, only 65 were spare and 10 emaciated of 429 individuals attacked. The mortality from epidemics in Manchester and Liverpool, as in Spitalfields, diminished, not as work and consequently the means of procuring food increased, but in times of distress and as the means diminished, or, in other words, in proportion as the people, being out of work, were not confined to their badly-ventilated workshops. Of course, "distress in itself cannot be a cause of good health, but some of the consequences, such as absence from the crowded and ill-ventilated workrooms, or from ill-ventilated and ill-drained houses, and the inability to gratify hurtful and costly propensities, may, for a time, be more influential in preventing disease, than the scanty supply of food and clothing is in inducing it."

Deficient ventilation increases the general mortality. This subject is treated very effectively and in extenso, by Dr. Duncan, in his sanitary report on Liverpool. The mortality from fever increases with the density of the population, or, in other words, with deficient ventilation. We subjoin the following table, made up from Dr. Duncan's data, in proof. There is one portion of Liverpool in which Dr. Duncan estimates the density of the population at 657,963 persons to the square mile, nearly 2¾ greater than the maximum density of London, as stated by Mr. Farr:

Towns.	Ratio of density of population.	Inhabitants to one death annually.		Per centage, proportion of fever deaths to others.
		From consumption.	From fever.	
Birmingham . . .	40	207	917	4·10
Leeds (Parliamentary Borough)	87	209	849	4·48
Metropolis . . .	50	246	690	4·83
Manchester (three years) .	100	172	498	5·61
Liverpool and West Derby .	—	—	488	6·23
Liverpool (Parish) . .	138	156	407	6·78

Influence of occupation on the health with reference to ventilation. The evidence of Dr. Guy is principally as to the injurious effects of employments, with special reference to deficient ventilation. Dr. Guy has personally inspected the workshops of the metropolis, and carefully entered the sex, age, and occupation of several thousands of adults, presenting themselves as out-patients of King's College hospital, throwing the occupations into classes, and contrasting the results with the mortuary registers for 1838-9. The result of his researches, as they respect employments, are the following:

> "1st. Consumption is relatively more frequent in persons working indoors than in those employed out of doors. 2d. In those employed within doors, it is most frequent in men using little exertion. 3d. It makes its attack earlier where it is of most frequent occurrence. 4th. It is very common in the intemperate, and in those exposed to the inhalation of dust. 5th. It is more frequent in men than in women, at least in the metropolis." (Minutes of Evidence, p 343.)

Dr. Guy states that "1st. The proportion of consumptive cases in the three classes of gentlemen (including professional men), tradesmen, and artisans (including all classes of labouring men), are respectively 1 to 5, 1 to 2·60, and 1 to 2·29; or about 16, 28, and 30 respectively in the hundred. 2d. The average age of death from consumption is earlier in the last two classes than in the first; but the artisan, who dies of consumption, dies at a somewhat later age than the tradesman." So that the tradesman of the metropolis are nearly twice as liable to consumption as the gentry, and the greater mortality, Dr. Guy thinks, is mainly attributable to their confinement during so many hours of every day in ill-ventilated shops. The slightly less liability of the artisan class is attributed to the fact that many of the latter follow employments requiring active exertion out of doors. The deaths from consumption of persons under thirty years of age, were tradesmen, 33 per cent.; labourers, in-door, 37½ per cent., out-door, 25 per cent. An indoor sedentary employment predisposes much more to consumption than all in-door active employment. Of the former, the deaths from this disease were 44 per cent., of the latter only 31½ per cent., less than the trade-class. As a special illustration of this general fact, Dr. Guy mentions the case of pressmen and compositors, "of whom the former use strong exertion in their employment, while the latter make many small movements of the hands and arms. Their rooms are generally warmed and lighted in the same way, and they both have but little space to work in, though the pressman requires more room. The comparison is greatly to the disadvantage of the compositor, who is extremely liable to consumption, and at a comparatively early period of life. This is the more striking, as the pressman is notoriously more given to habits of intoxication than the compositor."

We doubt the validity of this conclusion. The compositor follows the employment probably because being originally of delicate constitution, "light-work" was thought to be more suited to him. The compositor, too, requires more knowledge of letters (we do not mean to pun), and it is often in delicate organizations that a precocious taste for literature is developed. The bias given by congenital qualifications, whether of mind or body, to the adoption of a trade or profession, is generally forgotten: but we believe it will be found to have an important influence in determining the comparative mortality of professions. We suspect the (asserted) higher mortality of the medical profession may be explained in this way; for we can conceive nothing more conducive to health than the varied avocations of the medical practitioner.

Scrofulous diseases result from deficient ventilation. Mr. Toynbee gives some very important evidence on this point:

> "The defective ventilation appears to me to be the principal cause of the scrofulous affections, which abound to an enormous extent among our patients. When I have had a scrofulous patient come before me, I have always been able to trace this as one of the agents. I am not prepared to state that other causes may not produce the disease, but I am prepared to state that I believe this is the greatest cause in our district. We find, as accessories, the want of personal cleanliness, badly-chosen and badly-cooked food, and defective clothing. My observation is very generally corroborative, however, of the views taken by Mons. Baudelocque, who, in a treatise, 'Etudes de la Maladie Scrofuleuse,' states that the repeated respiration of the same atmosphere is the cause of scrofula; that if there be entirely pure air, there may be bad food, bad clothing, and want of personal cleanliness; but that scrofulous disease cannot exist. He gives such facts as the following." (Minutes of Evidence, p. 333.)

Mr. Toynbee then quotes facts from Baudelocque, "who owes the details to M. Andrieux," who owes a fact, by the way, to "M. Regnault," and who quotes what Professor Alibert "has well observed." Mr. Toynbee also refers to Dr. Duncan, as adopting similar views, and as quoting "the authority of her Majesty's physician." We cordially concur in the opinion that Sir James Clark has written the best monograph on consumption in our language; but we venture to affirm that he has seen too many examples of its hereditary transmission and fatal termination in individuals who, from the moment of their birth, have had every care taken to supply them with pure air, sufficient but plain food, suitable exercise, &c., to adopt the extreme opinion of either Mr. Toynbee or M. Baudelocque. Sir James, it is true, attributes much to the want of pure air, and most correctly, but he attributes just so much and even so correctly (as Dr. Guy's statistics show), to the want of active exercise. We are not quite convinced that all the details of the school at Norwood and the Bluecoat school at Manchester are rightly stated. It is well known to practitioners connected with charity-schools, that many of their inmates are tainted with scrofula when they enter; they are the orphans of sickly parents. Mr. Toynbee's facts are, however, worthy attention, as containing much truth, and exhibit talent and praiseworthy industry. We quote a portion of his own summary, as presenting some new points. After attributing almost all infantile diseases to atmospheric impurity from defective ventilation, he says:

"Amongst other forms of disease which I think ascribable to the influence of vitiated air, is a large amount of what has not hitherto been ascribed to it, namely, deafness. In justification of this opinion, I may state that I have already made between 500 and 600 dissections of ears, with a view of determining the seat of this particular disease. 120 of these cases I have submitted to the consideration of the profession in the Twenty-sixth volume of the 'Medico-Chirurgical Transactions.' The general effect observable, is the thickening of the membrane of the middle ear. This membrane is semi-transparent, and being extremely sensitive and delicate, is, I believe, injuriously affected by the contact of vitiated air, and debilitated by it; inflammation and other diseases are induced by the access and pressure of cold air on leaving heated rooms to go out into the colder atmosphere. The delicate membrane of the ear, it is to be recollected, is longer exposed to the depressing influence of the vitiated air than any part of the body. On leaving a room the surface of the body is relieved from the continued access of the vitiated air, whilst the quantity of vitiated air contained in the middle ear remains for a considerable time, and is only slowly removed. The suspicion which I had formed from the dissections, that the cause of deafness is dependent upon the contact of foul air, appears to me to be corroborated by the fact, that at least double the number of children of the labouring classes are affected by earach and deafness than children of the rich and better conditioned classes less exposed to the like influences."

Means of improving the ventilation of dwellings. Mr. Toynbee and Dr. Guy have introduced plates of perforated zinc into the windows of the poor, and into some of the reading-rooms of the printing offices with the best effect. Mr. Toynbee has also introduced a cheap chimney-ventilator into rooms, contrived by Dr. Arnott. This is a square iron tube, from four to six inches in length, and three to six in diameter; one end opens into the chimney, the other into the room, and is covered with perforated zinc. Mr. Hosking objects to openings in the top of rooms; but Dr. Arnott, a great authority on these subjects, states that it is impossible to ventilate a room properly without such an opening. He has a plan in contemplation for adapting an air-pump or bellows to ventilation, not on the erroneous principle, however, of "wire-drawing" the air through small openings for its exit and entrance. Dr. Arnott thinks it would be possible to move a mechanical ventilating apparatus, at almost as little cost as to move a large clock, by winding it up, and so, for instance, supply the pure air required for a crowded evening party. A water company might supply the power by filling a cistern at the top of the houses, or a man might turn a wheel. Dr. Arnott states, (and his opinion is corroborated by Dr. Willis,) that there is much ignorance among all classes respecting the necessity and means of obtaining proper ventilation. Popular instruction is wanted. The poor stop up every crevice in winter by which cold air can be admitted. "My old teacher, Dr. Gregory of Edinburgh," Dr. Willis testifies, "in visiting the poor, used often to begin his prescription by breaking a pane or two of the window with his walking-stick, which he would make good again at the end of the illness." Men in workshops (e. g. tailors) will also stop up the ventilators. Of course, pure warm air should be thrown into the room. Few architects understand the true principles of ventilation.

Injurious effects of an atmosphere rendered impure by miasmata. The most obvious result of bad sewerage and drainage, by rendering the air impure, is to increase mortality and shorten life. Thus, in Leicester, in the culverted streets, the mean age of the dying was 25½ years; in those partly culverted, it was 21 years; in those unculverted, 17 years. At Chorlton-upon-Medlock, the deaths in the best-drained streets amounted to 2 per cent., or just one half of the mortality of the worst conditioned streets. In the unsewered districts in Nottingham, the annual mortality is 3¼ per cent. That much of this excessive mortality is owing to miasmata is shown by the following statement of Mr. Holland of Chorlton-upon-Medlock:

> "In the third-class houses, the rate of mortality has been 2·7 per cent. in the first, and 2·8 per cent in the second-class of streets, and 4 per cent. in the worst class of streets and houses. Part of this excess must (as has been before remarked) be attributed to other causes than the bad condition of the streets; and it is difficult to determine how much ought to be attributed to this latter, the most easily removable of the causes of disease. With the view of in some degree answering such inquiry, I have ascertained what has been the mortality in twenty streets which were a few years ago in a very bad condition, and which have since been improved, and I find that in some few the rate has been higher after improvement than before, but in most of them it has been lower. The streets in question are inhabited by about 3500 persons, among whom the rate of mortality was about 3·1 per cent., or 1 in 32, before the streets were paved, and 2·53, or 1 in 39, since the improvement. There seems to be no reason for supposing that the rank of the inhabitants has altered or their number diminished. It would appear, then, that the deaths are diminished more than 20 a year out of 110, by putting the streets into proper condition." (Appendix, p. 63.)

A miasmatic atmosphere increases the mortality from febrile diseases. In all instances where there is an excessive general mortality and bad drainage, there is also an excessive mortality from endemic and epidemic fevers. In the culverted districts of Lancaster, for example, the deaths from this class of diseases during the year 1840 amounted to 25 per cent. of the total deaths; but in the partly culverted districts, the proportion was 33 per cent., and in the districts altogether unsewered, 50 per cent. In his report on York, Dr. Laycock adopts a juste milieu opinion; the impure air consequent on deficient drainage and ventilation acts on the lungs, predisposing them to disease, and on the system generally, predisposing it to epidemics.

> "There can be no doubt that deficient ventilation, even in well-drained districts, will increase the mortality from epidemical and pulmonary diseases; but it is not generally remembered that the effects of deficient drainage are not to be avoided by good ventilation. In proof of this, the following facts will probably be deemed conclusive. The village of Rufforth, near York, is very badly drained, a wide stagnant ditch passing through the village. It is situate in a slight hollow, on a level plain, bounded by Marston moor, Askham bogs, &c. The village of Acomb is about two miles distant from Rufforth, and is situated on an eminence overlooking the level. Both of course are well ventilated, as country villages usually are; but Rufforth being less populous and more agricultural, has of the two the advantage in this respect.

	Population in 1841.	Altitude in feet.	Inhabitants living to one annual death, from		Mean age at death.
			All causes.	Epidemics.	
Rufforth . .	276	61	34	69	28
Acomb . .	774	110	41	258	35½

> "The scarlatina, when epidemic at Rufforth, was so malignant as to be fatal in a few hours, and was termed by the villagers the 'black' fever."

The general summary of York exhibits similar results:

	Mean Altitude.	Population to square Rood.	Mean age at Death.	Inhabitants to one death annually.			Deaths of Labouring Class per cent.
				From all causes.	From Epidemics.	From Pulmonary Disease.	
Best drained and ventilated Parishes	50	27	35·32	54·32	347·72	334·22	40·2
Intermediate Parishes . .	43	40	27·79	41·41	247·20	219·70	52·5
Worst drained and ventilated Parishes	33	63	22·57	32·15	129·48	153·00	62·8

In addition to these modern instances, Dr. Laycock has appealed to historical data, and has drawn up a report on the epidemics of York, especially those prevalent in the sixteenth, seventeenth, and eighteenth centuries. "From this it is manifest," to quote Dr. Laycock's conclusions, "that the frightful mortality of these plagues and 'visitations,' as they were termed, was altogether dependent on the malaria generated in the city, partly from absolute uncleanliness, partly from deficient sewerage and drainage. It also shows that the seasons of prevalence, and the localities of previous epidemics (the plague in 1551, the plague of 1604, and the cholera of 1832,) are, to a great extent, identical with those in which they still flourish in a mitigated form, although the insalubrious localities now principally occupied by the poor were then the residences of the wealthier classes of society." Under what hygienic conditions does the fever of our large towns prove most fatal? It will have been observed that deficient ventilation was considered to be the principal cause of fever by Dr. Arnott; we shall now adduce some facts to show that the more usual and efficient cause is an impure atmosphere in consequence of deficient drainage, either alone, or combined with deficient ventilation. Dr. Southwood Smith, physician to the London Fever hospital, was asked if his statement was correct, that fever constantly prevails, generated by the dirt and filth in which the people habitually live? "That is too true," was the pithy answer.

> "In every district in which fever returns frequently, and prevails extensively, there is uniformly bad sewerage, a bad supply of water, a bad supply of scavengers, and a constant accumulation of filth; and I have observed this to be so uniformly and generally the case, that I have been accustomed to express the fact in this way. If you trace down the fever districts on a map, and then compare that map with the map of the commissioners of sewers, you will find that, wherever the commissioners of sewers have not been, there fever is prevalent, and on the contrary, wherever they have been, there fever is comparatively absent." (Minutes of Evidence, p. 69.)

The miasm is blown with the wind in a certain direction, and fever breaks out to leeward, and travels by jumps like the cholera.

> "Every now and then, we meet with a positive and precise fact, which shows that fever prevails in a neighbourhood just as the wind blows from an infected place in the direction of that neighbourhood, and that the production of any new

cases of fever entirely ceases as soon as the wind shifts. Many most instructive observations of this kind are on record, and I have given one or two examples of them in the report alluded to; but a large and crowded town does not afford favorable opportunities for such observations. What we more commonly observe in the metropolis is, that when once fever breaks out in one of the districts we have been speaking of, it continues to rage there during the prevalence of the epidemic; it does not spread in any very clear and remarkable manner to an adjoining district, but runs over it, and breaks out in a distant oue.

"As in cases of cholera? Very much so." (Minutes of Evidence, p. 76.)

Although Dr. Duncan (in his report on Liverpool,) is inclined to doubt whether the malaria arising from decomposing animal and vegetable matter "is an efficient cause of fever, independently of other circumstances," we shall press his facts into our service, and turn his own guns upon himself. Moisture, of course, is requisite to the formation of malaria or miasm.

"The following table shows that the proportion of damp and wet cellars is greatest, and least in the same districts respectively, when fever reaches its maximum and minimum; and that the three most unhealthy districts generally are the most unfavorably situated in this respect; the proportion of damp and wet cellars in the former being 44⅓ per cent., and in the latter 28⅓ per cent. of the whole. Other diseases are probably also more prevalent in cellars, for the total number of dispensary patients residing under ground is certainly much larger than the cellar proportions of the working classes should give; but that fever especially infests these abodes, is shown by the fact (so far as a few hundred cases can be trusted to for the purpose,) that while of every 100 dispensary patients of all descriptions attended by Mr. Higginson and myself, 31·22 resided in cellars, there were 36·22 in cellars, out of every 100 cases of fever." (Appendix, p. 26.)

The table that follows shows, in fact, what Dr. Duncan asserts. Then again Dr. Duncan makes this statement, clinching it with a table: "The prevalence of fever, and the rate of mortality proceed inversely as the efficiency of the sewerage," and yet adds, "I am inclined to look upon the absence of sewerage, although certainly one element of mortality, as less influential in its action than some others which have been noticed." But why so inclined? His facts are good and true; are corroborated by other observers, and are in accordance with the general pathology of all malarious fevers. Perhaps Dr. Duncan has set up an idolon specus, an idol of the den.

Injurious effects of a miasmatic atmosphere on the morals. Infanticide. There are other injurious effects of a malarious atmosphere generated in towns. The most striking of these is the infantile mortality among the poorer classes. The cause appears to operate by rendering the infantile epidemics more virulent, or by inducing a general sickly state, and so increasing the general mortality from natural causes. This sickly state also increases the mortality from poisonous doses of opiates. The child is peevish by day and by night; the nurse, hired by the mother to tend her infant, while she labours at the factory, is peevish too, and wants peace; or at night, the parents are wearied and want rest. Recourse is therefore had by both nurse and mother, to "infant's quietness, one teaspoonful at the age of three months," "Godfrey's quietness, dose twelve to fourteen drops," "soothing syrup," &c. Sales of these opiate mixtures for infants by fifteen venders residing in Ashton-under-Lyne, amount to the enormous quantity of nearly seven gallons weekly!

Drunkenness is another of the moral effects of a malarious atmosphere in towns. In quoting the following interesting statement from Dr. Southwood Smith's evidence we put in a caveat to this effect, that the peculiar and strong desire for stimulants observed to be endemic among a poor civic population, is due in some degree to their more excitable nervous system; and that this excitability is constitutional and developed not simply by exposure to injurious agents, but also by the greater mental, and less muscular activity of the citizen.

> "The poison generated in these neglected districts, and to which these poor persons are habitually exposed, is a sedative poison, among the most distinctive characters of which are the depressing effects produced by it both on body and mind. This is one of the main causes, not only of the mental apathy, of which I have already spoken, but also of that physical listlessness which makes them incapable of any great exertion. Every one who has observed his own sensations during the few days which precede an acute attack of fever, can well appreciate that feeling of *malaise*, more intolerable than pain; and it is no wonder that they fly to anything which affords a prospect of temporary relief from it.
>
> "When you speak of stimulants, do you mean that it drives them to the use of ardent spirits? Yes, and some to opium.
>
> "Is not opium now used to a great extent? To a much greater extent than any one could credit who is not aware of the fact." (Min. of Evidence, p. 73.)

Here our notice of the contents of this Report must end. We need scarcely say that we have done it only inadequate justice. There are several statistical important documents to which we have made no allusion whatever. One of these is a table exhibiting the standard of vitality actually attained in middle-class life. The society of Friends was adopted for this inquiry, and from the table, it appears that the mean age of the dying, during the years 1841-1843, belonging to this society was 48·80 years for males, and 55·13 years for females. We have reason to believe that Mr. Chadwick undertook the necessary inquiries. We ought to state that an octavo edition of the Report has been printed.

II. Our readers have been already made acquainted with the course and nature of the epidemic which prevailed during last year in Edinburgh, Glasgow, and other cities and towns of Scotland. The causal dependence of this fever upon poverty is the theme of Dr. Perry's pamphlet. The following proem to Dr. Perry's argument may be considered as containing its substance.

> "The question has been frequently asked, what is the cause of the present epidemic? Respecting the immediate cause of any epidemic disease with which a community may be visited, very little satisfactory information has hitherto been obtained; so that to give a definite answer in our present state of knowledge is beyond our power. Attempts have indeed been made, and those who have the least experience generally speak with most certainty on the subject. At present it is the fashion to ascribe every epidemic, whether malignant fever, cholera, dysentery, or influenza, to malaria arising from decaying animal or vegetable substances, owing to the want of sewers for carrying off such substances, and the scanty supply of water. There are perhaps few places better supplied with water than Glasgow; and I have observed on more than one occasion during the prevalence of malignant fever, that its progress was equally rapid and violent during a period of intense frost, when everything has been covered with snow, and the whole liquid substances in the streets firmly bound up for weeks together, with-

out the possibility of any putrefaction going on. This was particularly the case in 1837, when the frost continued very intense for upwards of six weeks, when the number of fever cases was greatest; the same thing was observed at Moscow during the prevalence of cholera. It appears that some epidemics are *spread solely by means of infection;* these are specific poisons, generated in the bodies of those who are undergoing the disease, and spread either by contact or by the emanations from the bodies of those affected by the specific disease. This is the case with all that class of infectious fevers called exanthemata; as small-pox, measles, scarlatina, and typhus. There are other epidemics which seem to depend on some peculiar state of the atmosphere predisposing the bodies of those whose constitutions are weak to suffer under their influence, of which the present epidemic affords a good example. This brings us to the more practical part of the question; viz., the causes which predispose persons to be affected by any prevailing epidemic, and in this inquiry very little difficulty presents itself, there are so many facts which attest that it is the poor and indigent part of the population who furnish the earliest, and by far the greatest amount of victims." (p. 6.)

As we differ in part from Dr. Perry, and as we think it of great importance that those who like our author, and (we may venture to add) ourselves, are labouring to ameliorate the sad and disgraceful condition of the labouring population of the kingdom should have union of sentiment and object, we will shortly state the points on which we differ: premising, however, that we admire and fully appreciate the ardent philanthropy and active benevolence displayed in this as well as in Dr. Alison's and Dr. Watt's pamphlet. In the first place, we think that Dr. Perry has not exactly comprehended the views of those who, like ourselves, trace an intimate connexion between malaria or miasmatic emanation and epidemical diseases. These views have been stated in the preceding pages. We consider that that peculiar state of the atmosphere alluded to by Dr. Perry, as predisposing the bodies of those whose constitutions are weak to suffer from epidemics, consists in the presence of malaria or miasmata. If it were an accurately ascertained fact, (which it is not,) that the late epidemic in Glasgow, and epidemics in general, was or are equally rapid and violent during a period of intense frost, it would not be absolutely opposed to our views, since persons who have resided in a malarious atmosphere, retain the morbid predisposition alluded to, during a period more or less prolonged after a removal to a purer air. The ague, for example, will first appear several days, in the traveller, after exposure to malaria, who has only traversed a marsh for an hour or two. We readily grant that the exanthemata spread solely by infection; but the malignancy of their type and their mortality will be in a direct ratio, cæteris paribus, to the impurity of the atmosphere breathed by those suffering from them. It is for this cause, that the value of fever hospitals is doubtful in proportion to their magnitude, although we were hardly prepared to find, as is stated by the district surgeons, that the mortality was less in the filthy hovels of Glasgow than in the fever hospital of that city. Glasgow may be absolutely better supplied with water than most other places, yet not better supplied relatively. And if ever so well supplied, of what value can the supply be, if not used? What do the district surgeons say on this point—the use of water in removing decomposing organic remains? Of district No. 4, Mr. Smith, the surgeon, remarks,

"The tenements in which I have visited are occupied from the cellars to the attics, and almost altogether kept for lodging-houses, many of them being more fit for pig-styes than dwellings for human beings; and in not a few the donkey and pigs rest at night in the same apartment with the family. The entrance to these abodes is generally through a close, not unfrequently some inches deep with water, or mud, or the fluid part of every kind of filth, carelessly thrown down, from unwillingness to go with it to the common receptacles; and in every close there is at least one of these places situated, often immediately under the windows of the dwelling-houses, or together with byres, stables, &c., forming the ground-floor; while the stench arising therefrom in summer, pollutes the neighbourhood, and more especially renders the habitations above almost intolerable." (p. 20.)

Dr. Brown, of district 11, observes,

> "The whole of the Burnside, especially the ground-floors, are not fit places for pigs; height of ceiling about four feet, and at almost every flood in the Clyde, they are inundated by the Molindinnar Burn; every inhabitant of these dens has had fever." (p. 25.)

Dr. Fisher, of district 12, states,

> "No. 23, a long dirty close. In one house at the top of it several severe cases occurred. Access is obtained to this house, or rather apartment, by an outside stair, by the side of which is a dunghill, the stench from which is intolerable. I have seen the dung reach the landing-place on the top of the stair. I attended for fever almost every individual residing in the front land. The number was very great." (p. 27.)

While the above extracts strikingly exhibit the coexistence of the sources of malaria and fever, the following shows that by removing the sources, the district-surgeon, Mr. Clark, checked the spread of the epidemic.

> "In No. 100, I caused the policemen on the station, to summon every tenant in the close before the police magistrate. The result was a thorough cleansing out of the filth which had accumulated for months, and a diminution of the fever cases to less than one half in about a week." (p. 23.)

As decomposing vegetable and animal remains are to be so generally found *within* the abodes of the poor of Glasgow, it might be inferred *à priori*, that the fever would be scarcely less prevalent in the winter than the summer months. On comparing, however, table iii of six districts in Glasgow, in which the fever was most numerous, and in which epidemics generally are most prevalent, with table i, in which the deaths in each month from May to December are given, we find that the numbers attacked monthly, describe a curve, the highest convexity of which corresponds to the two epidemical months of August and September. The number of coffins given out by the poors' house monthly, exhibits a similar curve. The average of May and December, the two extremes being 113, of August and September, 236. From these considerations, we infer that the malarious atmosphere of the closes of Glasgow accelerated the progress and increased the mortality of this epidemic. As usual, the Irish exhibited their national predisposition to suffer from fever. In Dr. Paterson's district at least one third of the patients belonged originally to Ireland. Dr. Perry states, "in 1832 the proportion of Irish to the whole pauper population admitted into the fever hospital, was 33 per cent., in 1842 it amounted to 41 per cent."

As a remedy for the evils he so vividly depicts, Dr. Perry strenuously advocates the adoption of sanitary measures, basing his arguments partly on the mere money saving.

"As comparatively few of the sufferers belong to the middle ranks—being, as already remarked, chiefly confined to the poor and labouring classes, a large proportion of them being adults—some idea will be formed of the amount of suffering among the poor, by considering that, on an average, they were unable to follow their usual employment, or gain anything for their support for five weeks; and, calculating their weekly earnings at the small sum of four shillings weekly, it exhibits a positive loss to the poor, in eight months, of not less than £32,000. Had this been the result of a strike among the workmen, the whole newspaper press would have been filled with lamentations; but being the result of causes over which the poor had no control, the press has been comparatively silent, and little has been done for their relief. When it is considered besides, that Glasgow has not suffered alone, but that the epidemic has visited nearly every large town in this country, the effect ought to call forth some effectual means of relief, and the devising of measures for preventing the recurrence of a similar or even worse calamity, which, in the present circumstances of the country, may with certainty be predicted." (p. 12.)

Dr. Perry enters his solemn protest against the doctrine, that the medical treatment of the poor at their own houses was less expensive than in the hospital. The epidemic attacked 11¼ per cent. of the population; and he asks, "if the public had possessed the means of isolating the first cases as they appeared, by sending them to a hospital, would the epidemic not have been checked? And in place of having 32,000 cases, with all their accompanying sufferings, might it not have been limited to less than 5000?"

Dr. Perry proposes the establishment of a board of health. His plan differs little from those which have already been found to be altogether inefficacious to meet the exigencies of the case. The Report of the Health of Towns' Commission will, we have no doubt, attract Dr. Perry's attention, and probably induce him to modify his views, both with respect to the causes of disease, and the legislation requisite for their removal.

Another point to which Dr. Perry adverts, as intimately connected with the poverty and destitution of the poor, is their mental and moral degradation. This intimate connexion with, or dependence of vice upon extreme poverty, is too obvious to those who have any intercourse with the poor, he justly observes, to require remark. We cordially agree with him in his opinion, that to attempt to remove the one while the other is allowed to remain in large cities, is to attempt impossibilities. He compares the dietaries of English workhouses and of the prisons with the full diet of the Glasgow Infirmary, and shows that it is barely equal to the ordinary diet of the former, and the lowest rate of diet in the latter, namely, 24 oz. of wheaten bread per day. Statements from the report of Mr. Hill, inspector of prisons, are quoted, from which it appears that at one time forty persons, the majority of them able bodied, were undergoing voluntary imprisonment. Indeed it is obvious as an axiom, that the prison must be a palace, in its domestic arrangements, when compared with the dreadful residences of the Glasgow poor.

Many of our readers will have read the first volume of the Sydenham Society, and learnt something of the terrific ravages of the "black death." If a malignant influenza, as that epidemic appears to have been, were to break out in Glasgow we are firmly of opinion that nothing would stay its ravages, except want of victims. It would sweep through that and similar towns like a hurricane, leaving behind it the silence of desolation. A lavish expenditure, extorted by national anguish, would then be useless. Boards of health would wrestle in vain with its gigantic strength. The danger must be anticipated. One thing is to us manifest: whatever evils are consequent on the English poor-law, (and that, like all human institutions, it has its evils, we readily acknowledge,) the evils of *no*-poor-law in Scotland are greater. Wherever a poor-law exists, and is efficiently worked, human nature cannot possibly sink into that abyss of degradation, in which, according to Dr. Perry's statements, the poor of Glasgow are plunged.

We cannot conclude our notice of Dr. Perry's brochure without stating, in his own words, the circumstances under which it was printed:

> "In order to aid the laudable design of Dr. Hutchinson in exercising the mental and bodily faculties of the inmates of the Lunatic Asylum, the printing of this paper, the colouring of the maps, &c., is wholly the work of the inmates. It is hoped that this circumstance will induce the reader to make allowance for any thing he may observe amiss in the manner in which this, their first essay, has been performed."

Although the printing is not first-rate, it is extremely creditable to the printers. The only mishap worthy of notice is "the pie" they have made of the author's academic and professional titles. If they had had better type and paper, the book would really have been a stylish affair.

III. Now and then we meet with works so admirably condensed, as to defy even our powers for greater condensation. Dr. Watt's pamphlet is one of these. Justice could only be done to it by transferring two thirds of its contents, word for word, to our pages. We therefore recommend the vital statist to do well by placing it at once in his library; and to our general readers we will present some of the more important points connected with our present subject—the public hygiene of Great Britain.

Effect of the malarious atmosphere of Glasgow. We ought to premise, that Dr. Watt, like Dr. Duncan, Dr. Perry, and Dr. Alison, is of opinion, that the cause of fever is to be sought for rather in the want of nutritious food than of pure air. But Dr. Watt, although warring on the side of the philanthropic Alison, is a moderate man—a genuine eclectic, like ourselves; and insists that destitution is a principal cause of fever, rather to defeat the opponents of a poor-law for Scotland, than to defend an "idolon specus." As we have no doubt that, sooner or later, a poor-law *must* be introduced into Scotland, we shall, as in the case of Dr. Duncan, turn some of his guns against himself.

Effects of Glasgow malaria on the spread of fever. Dr. Watt makes an estimate (he says it is a moderate one,) that 60,000 persons in Glasgow have suffered from fever during the last seven years. According to our views, heat and moisture, and organic remains in a state suitable to decomposition, are necessary to the production of malaria, and of course to the fevers aggravated by or dependent on malaria. Heat and moisture and dirt may be *inside* a dwelling (as they are in Glasgow in many thousands of dwellings,) during the winter; but in summer, heat and moisture and filth are both outside and in. Then, consequently, *cæteris paribus*, there must necessarily be abundant cases of fever in Glasgow, and more abundantly in summer than in winter or in spring. What says Dr. Watt?

In January 1837, the deaths from fever and influenza were 10·19 per cent. of the whole deaths in Glasgow and its suburbs; but during the five months beginning June 1843, the proportion was 33·38 per cent. We have taken these two years for comparison, for the purpose of illustrating a general principle, overlooked, we observe, by Dr. Alison and his coadjutors, namely, that an epidemic fever will cease to spread, independently of all other considerations, from a want of persons liable to its attacks. Yellow fever and all the exanthemata are of this kind, not omitting specially the exanthematous typhus. Now, in 1837 the fever was at its height in the winter months, 1972 persons having died in January only; but in 1843 it was at its height in the autumnal month of October. In the former year, *relief began to be administered to the destitute just when the fever was beginning to decline*, namely, in February; in the latter year the relief *ceased to be given just when the fever began to increase*, namely, in May. Hence Dr. Watt, adopting the principle of "*post hoc, ergo propter hoc*," is warranted in inferring that fever and destitution were necessarily allied; but, according to our reasoning, the two were only coincident. The fever would have gone on, in 1843, concurrent with the relief, had that been continued.

Effects of the malarious atmosphere of Glasgow on diarrhœa. Cholera, dysentery, and diarrhœa are the plagues of summer camps, and of large towns in which the heats of summer act upon moist and decomposing debris. Glasgow, as we have learnt from Dr. Perry's pamphlet is full of these; and accordingly we expect to find that in the summer months (fever being absent,) there will be an excessive number of deaths from "bowel complaints" in that city. What do Dr. Watt's statistics teach us?

"But among the more important diseases which fall under our consideration, in treating of the vital statistics of Glasgow, are those which are classed under the head of bowel complaints; as the mortality by these is higher in Glasgow than it is in either Dundee, Perth, or Edinburgh, as will be seen by the following abstract; and it will be found that it is to these diseases that the high mortality among children in this city, during the months of August and September, are to be ascribed.

Proportion of Deaths by Bowel Complaints, for a series of years, to the mean population of those years.

	In Glasgow.	In Dundee.	In Edinburgh.	In Perth.
Bowel complaints	0·370	0·263	0·167	0·171

.... "The amount of deaths by bowel complaints, however, is very different in the several months. In January, the deaths by these diseases amount only to 8·57 per cent. of the whole deaths during the month, and to 0·031 per cent. of the mean population. In August, they amount to 18·40 per cent. of the whole deaths, and to 0·047 per cent. of the mean population, which accounts for the mortality under five years of age, this month amounting (table lxx) to 50·47 per cent. of the whole deaths during August, and to 0·130 per cent. of the population. In September, the deaths by bowel complaints amount to 17·53 per cent. of the whole deaths, and to 0·060 per cent. of the population." (p. 87.)

Dr. Watt remarks that "the uniformity in the high amount of mortality by bowel complaints, during the months of August and September, does not appear to be accidental, as the deaths by this disease are in excess for these months for all the years noticed in the Tables, with the exception of 1838." He hints, too, that the preeminence which Glasgow and Dundee have in the number of deaths from these forms of disease may be attributable in some degree to their preeminent filthiness. Dr. Watt must not forget, that the *natural* drainage of a town has a decided influence on the amount of malaria exhaled from the surface. Edinburgh may, therefore, be superior to the other two towns, because it has a superior *natural* drainage.

Effect of the malarious atmosphere of Glasgow on infant life. All the reports made to the health of towns' commission show that bad sewerage, and an excessive number of deaths among children under five years of age are coincident; there is no exception to this principle: Glasgow, of course, is no exception. During the five years ending 1841, 44·58 per cent. died, aged under five years, and 49·16 per cent. in 1842. In Liverpool, during the years 1839-41, according to Dr. Duncan's report, 52·8 per cent. die at these ages. It is in the deaths from epidemics, however, that the effects of civic malaria on infant life are best exhibited. At Glasgow, the months most fatal to children are August and September —the malarious months. Above one half of the whole deaths are children under five years.

> "On an average of five years, 84·06 per cent. of the whole deaths by that disease [bowel complaints,] takes place in Glasgow under two years of age, and, as above stated, 90·69 per cent. under five years of age." (p. 88.)

Dr. Watt's statistics of the exanthematous epidemics are equally instructive. They shall first show the influence of locality:

> "The following facts go far to show that it is to the circumstances of the people, and to the local condition of towns, that we are to look for a greater or less mortality by these diseases. There are many circumstances common to Glasgow and Dundee, both as regards the circumstances of the majority of the people and the local condition of the towns. There are also many circumstances in common to Edinburgh and Perth in these respects; and the mortality in them, by eruptive diseases, is not so very different, as will be seen from the totals of the following abstract, which gives the proportion of deaths per cent. by these diseases in each town for a series of years, to the mean population of these years:

	Edinburgh.	Perth.	Glasgow.	Dundee.
Measles . .	0·075	0·092	0·198	0·177
Scarlet fever .	0·052	0·070	0·096	0·120
Smallpox . .	0·056	0·050	0·144	0·140
Total Eruptive Diseases	0·183	0·212	0·438	0·437" (pp.86-7.)

Next the influence of temperature shall be shown:

> "By summing the proportions, as shown in table lxxii, for the eruptive diseases, measles, scarlatina, and smallpox together, it is found that in January, on the average of these seven years, 13·96 per cent. of the whole deaths are occasioned by them. It will be farther seen, that the proportion of deaths which occur by these diseases gradually diminish till April, when the proportion of deaths by them is only 11·48 per cent. of the whole deaths during that month. It will likewise be observed, that the proportion that they bear to the mean population gradually diminishes also. From April, the proportion of deaths, though somewhat suddenly in May, gradually rises in amount each succeeding month till November, in which month they appear to be excessive, amounting to 19·33 per cent. of the whole deaths, and to 0·048 per cent. of the population. In December, the proportion of deaths by these diseases falls to 16·99 per cent., which is still considerably higher than it is in January." (p. 86.)

Some of Dr. Watt's tables give us data for estimating the influence of climate on epidemics; we have not room for these, but we can assure our readers that Glasgow and Edinburgh are no better than Philadelphia and New York.

We have read Dr. Watt's tables, and remarks thereon, with much interest. The tables of monthly deaths give a much more satisfactory account of the influence of atmospheric changes than our registrar-general's quarterly division. We regret to observe that Mr. Cartwright has adopted the quarters instead of the months in his report on the seasonal mortality of Preston. The comparison of the seasonal statistics of Preston and of Glasgow would have been interesting. Meteorological tables for 1841 and 1842 are appended to the volume.

IV. The object of Dr. Alison's pamplet, is not sufficiently described by its title; its general scope is to show the necessity and economy of a poor-law for Scotland. It is, however, to the connexion of destitution with fever that we shall limit our remarks.

We think much of the controversy respecting the causal relations of poverty (for that is what Dr. Alison means by destitution) to fever, has originated, partly, in an indeterminate use of words, partly in misconceptions. Dr. Alison has, "on different occasions, adduced evidence to prove, not that destitution is an adequate cause for the *generation* of fever, nor that it is the *sole* cause of its extension, but that it is *one* cause of the diffusion of fever: of such power, that an epidemic of that disease, invading a community where the provisions against destitution are inadequate, is very generally found to spread, *cæteris paribus*, to an extent remarkably greater than where adequate provisions of that kind exist." The truth of this proposition, we think, will be granted by all medical practitioners; want of food predisposes the system to receive the febrile contagion. Dr. Alison states, " It will be at once perceived, that the relation which I maintain to exist between destitution and fever is not simply that of cause and effect, but that of *predisposition*, favouring the effect of another cause, *which is essentially variable.* Where destitution exists, it prepares victims for fever, but the fever 'bides its time.'" And yet, in a note in the preceding page, Dr. Alison states:

> "I reply that the question before us is not, what are the causes of destitution, but whether destitution is a cause of fever? Supposing all the destitution which the medical practitioners and benevolent individuals alluded to have seen, to have been the effect of misconduct, still, if they have seen fever spread with unusual rapidity in such families, they are entitled to infer, that destitution (caused by misconduct) is a cause of fever; and if so why not destitution caused by misfortune?—of the still more frequent existence of which with us, I have given, and shall give, more than sufficient evidence." (Note, p. 3.)

That certainly is the question as we have generally understood it, but do not the preceding extracts exhibit some inconsistency? Has Dr. Alison, like lesser men, an *idolon specus*? and sometimes, when facts are presented to him while doing his devoirs to his idolon, are they offered up to it? Thus, in a report by the physicians of the Infirmary it is observed that "the fever which now prevails is *unquestionably connected very closely with circumstances peculiar to the destitute part* of the population;" and from the noun-adjective "destitute," Dr. Alison infers the circumstances to consist in destitution. Yet could not Dr. Arnott say, and say justly, that deficient ventilation constituted *the* "circumstances," and triumphantly point to the identical fact, in support of his views, which Dr. Alison quotes triumphantly in support of his, namely, that the inhabitants of the highest stories offered the greatest proportion of fever victims? Or, again, would not Dr. S. Smith say that *the* circumstances were deficient cleansing and sewerage? because wherever fever prevails most, there is the least amount of sewerage to be found. It appears to us, that the predisposing causes of fever are found in all agencies which debilitate the system, or are allied to the proximate cause, the materies morbi. Several of these may, indeed in towns do, coexist, as the malarious emanations arising from stagnant sewerage, deficient nourishment, and a deficient supply of pure air. Malaria will cause fever we know; deficient ventilation will propagate it; innutrition will render it more fatal. But fever is not the only head of this hydra; need we mention scrofula and phthisis as the other two? We thus recommend to our readers Our idolon specus—the idol of Our den.

When the Queen's commission is fulfilled, and the cause of diseases among the people ascertained, what will the government do? We presume, it will take steps to enforce the supply of sufficient light, pure air, and pure water to the crowded population of our towns: A poor-law for Scotland will enforce a supply of food. From the general tenor of the inquiry already made, we presume, too, that the British system of public hygiene will be made, as far as practicable, by the conversion of the refuse of towns to agriculture, a self-supporting system,—an imitation, in miniature, of nature's own processes, which admit of no waste.

Art. XVIII.

Caloric; its Mechanical, Chemical, and Vital Agencies in the Phenomena of Nature. By Samuel Metcalfe, m.d. of Transylvania University.—*London*, 1843. 8vo, pp. 1100.

The nature and objects of this work are by no means sufficiently expressed in its title; for it is nothing less than an attempt to found an entirely new system of physics and physiology, in which caloric plays the part of prime mover, every other agency in nature being subordinate to it. Instead of speaking of it as an "entirely new" system, we should, perhaps, characterize it as a return to the philosophy of the ancients, who, according to our author, had much better notions on this subject than their modern successors; the former referring all the phenomena of nature to one universal and self-active principle,—elementary fire; whilst the attention of the latter is divided amongst various motor forces, such as gravity, electricity, vital force, &c.,—all of which, according to Dr. Metcalfe, are really attributable to this one agency, caloric.

We have every desire to speak well of Dr. Metcalfe's work. It is, we believe, the product of many years of patient and unrewarded study, pursued under many discouragements; and the man who will so devote himself to the extension of our knowledge of nature, deserves well of his race, even if his endeavours are not regarded by others in the same light as he himself views them. But much as we respect our author's industry and zeal, and much as we desire to serve him to the best of our ability, we respect the sacred interests of truth much more; and these compel us to express an opinion of the work far less favorable than we should be most glad to have given. The limited space which any such subject can claim in the pages of this Review, prevents us from entering into such an extended analysis of it, as we should have otherwise been glad to make, with the view of showing our readers the extensive survey of the phenomena of nature which our author has taken, and the merit of the inductions, if true, which he has founded upon them. We are very unwillingly obliged, therefore, to content ourselves with a statement of his general propositions, and with pointing out the errors into which we think that he has fallen.

That no hypothesis on the science of caloric, (or, as Mr. Whewell designates it, *thermotics*,) which has yet been offered to the world, can be regarded by philosophers as satisfactory, is evident from the present unsettled state of opinion upon the subject. Dr. Metcalfe is mistaken, however, in saying that "no one, either among the ancients or moderns, ever attempted to give a regular and systematic history of the mode in which caloric operates in all the molecular and aggregate forces of matter." Such an attempt was made a few years since, by the Rev. Thomas Exley of Bristol; who published a work entitled a 'New Theory of Physics;' wherein all the phenomena of the physical universe at least were traced up to certain attractions and repulsions between the atoms of matter, and certain ethereal atoms; which last seem to us to stand much in the place of Dr. Metcalfe's caloric. Now this work had two great merits, of which our author's is destitute. In the first place, it was the production of an accomplished mathematician, who brought to bear upon his subject powers of reasoning which had been exercised upon the profoundest investigations in regard to quantity and numbers; and we believe we are right in saying, that no true theory of physics is likely to be framed, without the mental habits which this study tends to cultivate; besides which, the very nature of the subject requires that, for its full development, the aid of mathematical processes should be frequently called in. In these points, we think that Dr. Metcalfe's work must be regarded as deficient. His reasoning is far from possessing the closeness and stringency, which the subject particularly requires; and he makes no attempt whatever to develop his theory in a mathematical form. Moreover we cannot but regard Mr. Exley's plan as superior; inasmuch as he ranks the forces of gravity, electricity, caloric, &c. as the products of one more general agency, which includes them all; instead of erecting any single one of them into *the* power which governs the rest. We apprehend that to the full as good a claim could be set up in behalf of electricity, as the cause of all physical and physiological actions, as that which Dr. Metcalfe endeavours to establish for caloric; and that it would be very easy for a theorist, possessing a tolerable knowledge of these phenomena, to write a very plausible treatise in two goodly volumes, in which this claim should be demonstrated, at least to the satisfaction of its author.

Dr. Metcalfe begins by inquiring into the nature of caloric. That it does not consist (as many have maintained) in mere motion or vibration among the particles of ponderable matter, appears to him sufficiently evident from the following considerations.

"1. That it may be added to and subtracted from other bodies, and measured with mathematical precision, as all good thermometers demonstrate:

"ii. That it augments the volume of bodies, which are again reduced in size by its abstraction.

"iii. That it modifies the forms, properties, and conditions of all other bodies, in an endless variety of ways:

"iv. That it passes by radiation, through the most perfect vacuum that can be formed by means of the air-pump, in which it produces the same effects on the thermometer as in the atmosphere.

"v. That it exerts mechanical and chemical forces which nothing can restrain, as in volcanoes, the explosion of gunpowder and other fulminating compounds.

"vi. That it operates in a sensible manner on the nervous system; producing intense pain, and disorganization of the tissues when in excess.

"But if caloric were a mere property or quality, how could it be taken from one body and added to another? Or if it augment the volume of other bodies must it not itself have volume, occupy space, and therefore be a material agent? Would it not be mere jargon, to speak of the radiation, reflexion, connexion and conduction of a mere quality or immaterial property? And if caloric were only the effect of vibratory motion among the particles of ponderable matter, how could it radiate from hot bodies, without the simultaneous transition of the vibratory particles? But it is certain that when iron, copper, and other metals are heated to any temperature below the point of ignition; like boiling water they give off caloric freely, without any sensible loss of ponderable matter." (pp. 5-7.)

Now we do not mean to set ourselves up as advocates of the vibration-theory, or indeed of any other theory of heat; because we conceive that the time is not yet come for erecting such generalizations; but we feel called on to point out what we conceive to be the fallacies of our author's arguments in favour of the *material* theory. The terms *addition* or *subtraction* of heat, as a distinct entity, to or from other bodies, involve a *petitio principii*; since the possibility of such addition or subtraction is the very thing to be proved. Dr. Metcalfe adverts to the experiments which have been relied on, as showing that an unlimited quantity of heat can be generated by friction; and affirms that caloric is not thus produced *de novo*, but that it is merely "forced out of the pores of bodies, in the same manner that water is disengaged from the pores of a sponge by pressure,"—being only liberated so long as the bodies are undergoing condensation. But this explanation appears to us perfectly inapplicable to the celebrated experiment of Sir H. Davy; who found that enough caloric was generated by the rubbing together of two pieces of ice in an exhausted receiver, to occasion their liquefaction. Dr. Metcalfe endeavours to account for this fact by saying that there is an enormous amount of caloric locked up in a state of chemical combination with ice; and that there is no good reason why caloric should not be separated from ice by friction, as well as from other solid bodies. But he forgets that the change produced in the bodies themselves is precisely the reverse of that, which he affirms to be the essential condition of the liberation of caloric in other instances, namely, their solidification; for whilst that change corresponds with the effect of cold, the liquefaction of the ice only takes place ordinarily under the influence of heat. Or, to put the same thing in another form, if there be absolutely more caloric in water at 32°, than in ice, (a position which Dr. Metcalfe would hardly dispute,) whence was the additional caloric obtained? The water could not return to its original condition, without giving out this additional amount. When again frozen, the friction of the ice would again liquefy it; and so on to an unlimited degree. Here, then, appears an unequivocal case of the production of heat, (to use the ordinary phraseology,) by friction, to an *unlimited* extent; a fact quite opposed, as it seems to us, to the *material* theory of caloric.

That the *calorical* state (we beg pardon for coining so uncouth a word, but we cannot find any other to answer our purpose,) of bodies may be measured by thermometers, or may be rendered evident to our senses, is no proof whatever of the *material* nature of heat; since we know very well, that bodies in a state of vibration (*sonorous*, for instance,) will excite a corresponding state in others. Thus when two strings or pipes are tuned to the same pitch, the setting one in vibration will cause the other, if sufficiently near, to emit sound of an equal pitch; and a certain *intensity* of sound will produce a violence of vibration, which occasions very perceptible effects—as when a wine-glass is broken by the mere sound of the voice, on the note to which it reciprocates. It would not be impossible then to construct an acoustic instrument, which should measure both the pitch and intensity of sound, in the same manner as the thermometer measures the intensity of heat. Moreover, that an effect is produced upon our senses by the former agent, is no more a proof that it is itself a material entity, than that the latter is so. Everybody knows that sound is occasioned by the undulations of the sonorous body, which are propagated by other bodies—gaseous, liquid, or solid, to each other, or to our organs of sense. And although we commonly speak of sounds as if they were real existences, capable of addition and subtraction, multiplication and division, yet every scientific man knows that it is not so; and that the intensity of the sound depends upon the extent of the vibrations, and its *pitch* upon their rapidity. Moreover, sound is capable of radiating, being reflected, conducted, &c., just as heat is; the only essential difference being, that we can *trace* the medium through which this takes place in the former case, and cannot in the latter. But will any philosopher venture to assert that we have created a *perfect vacuum*, when we have exhausted the *air* from a receiver by the air-pump; or that *nothing* exists in the vast regions of space, because our atmosphere extends but a limited distance from the earth? We believe that the current of opinion is at present setting quite in the opposite direction; and it is incumbent on Dr. Metcalfe to *prove* the existence of a complete *vacuum*, before he uses the fact of the radiation of heat through it, as an argument for the *material* character of that agent. The champion of the vibratory hypothesis might equally well say "your air-pump merely exhausts *air*, and your experiment only proves that *air* is not the medium of conveying the calorical vibrations; the very fact of the radiation of heat through your so-called vacuum proves the existence of a medium, by which its undulations may be propagated." This would be quite as great an assumption as Dr. Metcalfe's; but an assumption which appears to us equally justifiable, more especially as Dr. Metcalfe himself in maintaining the radiation of heat in a *material* form, through space, virtually declares that there is no such thing as a real vacuum in nature. This, indeed, he often repeats in the course of his treatise; and to us it appears much the same practically, whether space is filled by an ether whose undulations transmit light and heat, or by material particles. In neither case can a *vacuum* be admitted.

To follow Dr. Metcalfe through the chain of reasoning, by which he attempts to prove that the planetary motions are all immediately dependent upon the calorifying power of the sun, and that the laws of motion as propounded (and we had always thought established) by Newton, are no better than ingenious fictions, would be beyond the scope of our criticism; but we must content ourselves with remarking that the arguments upon which the new theory is based, appear to us equally unsatisfactory with the preceding, and that they are as far from overthrowing the received doctrines, as they are from establishing anything preferable in their stead. That the laws of motion and of gravitation may be ultimately combined with other similar *general expressions* of the phenomena of heat, electricity, chemical affinity, &c., and other molecular actions, into some expression of still higher generality, is, we believe, the expectation of most of those philosophers who have given attention to the subject; but this combination will assuredly not involve any subversion of those laws, which, as *general expressions of phenomena* (and no theory can be more) are perfect, and have stood every test that can possibly be devised for detecting their insufficiency or erroneous character. Dr. Metcalfe accuses those who use the word *inertia* as expressing that quality of matter by which it tends to remain in the state in which it is, whether of rest or motion, of a quibble, a play upon words; and thinks that he substitutes something much better when he tells us that "everything in nature is composed of two descriptions of matter—the one essentially active (caloric), and the other passive; as maintained by many of the most distinguished philosophers of ancient Greece," (p. 16;) and that caloric is "a self-acting principle, capable of moving itself, and of generating motion in all other bodies," (p. 12.) That a mass of matter once set in motion will remain so, unless checked by opposing forces, is a physical *fact*, which is not to be contradicted; and the law of inertia, as it is generally termed, is no more than an expression of this fact. On the other hand, that this phenomenon is due, not to a simple property of what we commonly term matter, but to a "self-acting principle" superadded to it, seems to us but a pure hypothesis; to return to which is to revive a mode of philosophizing that has long since been abandoned as totally unproductive of good results as well as fallacious in its character.

The remainder of the first volume is devoted to an exposition of the author's views, as to the connexion of his theory of caloric with the physical sciences in general. In chapter ii, of the First book, we find discussed the atomic constitution of matter, and the relative quantities of caloric in different bodies; and in chapter iii, the forces of caloric in elastic fluids, their expansion and contraction, and the constitution of liquids. In these we find the startling position maintained, that "every description of ponderable matter is actually convertible into light, by a sufficiently intense heat, or by electricity;" which he considers as proved by the following undeniable facts.

"I. That the quantity of light generated by ordinary combustion, friction, or percussion, is always in proportion to the rapidity with which ponderable matter is ignited and volatilized

"II. That the colour of light thus produced always depends on the species of matter employed.

"III. That the electric spark, (like that produced by the collision of flint and steel,) consists of exceedingly minute portions of ponderable matter in a state of incandescence, as will be shown hereafter by the decisive and beautiful experiments of Fusiniere.

"iv. That when the electric fluid is transmitted through the vacuum of an air-pump, little or no light is produced, as proved by the experiments of De Luc, and afterwards by those of Sir H. Davy.

"v. That the most intense heat of a voltaic battery never produces any light, except when acting on ponderable matter; consequently, that light and heat are not identical, as maintained by some modern theorists; and that neither of them is generated by the mere vibrations of an ethereal medium." (pp. 99-100.)

We are again compelled to express our total dissent from this doctrine; the arguments of Dr. Metcalfe in its favour being, as it seems to us, totally insufficient to establish it; and being in opposition to the well-known fact that, in every case of ignition, combustion, &c., in which light is exhibited, the *matter* of the luminous body is so far from being dissipated or lost, that it may be detected to its full amount either in the body itself or in the new compounds which are formed by the process. Therefore, if there be no loss of ponderable matter, there can be no "conversion" of it into light.

The Second Book contains the application of the same theory of caloric to the phenomena of cohesion, conduction, and radiation—of chemical attraction in general—of solution and of freezing mixtures—and of capillary attraction; with the connexion of molecular with aggregate forces of matter. In many of the views propounded by the author, and in the relations pointed out by him, there is, to say the least, great ingenuity displayed; but we are still compelled, though most unwillingly, to express our conviction that the mode of reasoning which he adopts is very far from being satisfactory. We are continually encountering charges against those who attribute to matter such properties as inertia, attraction, &c.; as having no ground for the assumption of these properties; and as having overlooked the real *cause* of them all, viz., caloric: but the author seems to us to mistake his assumption of this "self-acting principle," which is to account for anything and everything, as a demonstration of its existence; and lays himself open to the sarcasm of Molière fully as much as those at whom he points it, if not more so.

In the Third Book, Dr. Metcalfe attempts to prove the identity of caloric and electricity, or rather, that they are modifications of the same universal element. That the analogies between them, which are ably collected and pointed out by Dr. Metcalfe, are very strong, cannot be denied; but the differences still appear to us stronger, requiring that, for the present at least, their phenomena should be referred to a distinct category. That electrical disturbance should generate heat, and, *vice versâ*, that heat should give rise to electrical disturbance, are facts which may be freely admitted, without the deduction being unavoidable "that if caloric and electricity be not modifications of one power, and the cause of all mechanical and chemical action, the whole of modern science is a perfect chaos of contradictions." (p. 454.) But the analysis of Dr. Metcalfe's arguments on this subject, the ingenuity of which we freely admit, would lead us far astray; and we must content ourselves with a notice of that part of his treatise with which, from its bearing on medical science, we are more immediately concerned.

The Second and larger volume is wholly occupied with the application of Dr. Metcalfe's doctrine of caloric to the phenomena of life; for which, he considers, that this agent satisfactorily accounts. It becomes, in his hands, very much like the vital principle of many physiological writers, capable of so moulding and combining the elements of matter, as to render them capable of performing all the varied and wonderful actions which the animated creation exhibits. Our readers are doubtless familiar with our views on this subject; which we may here briefly recapitulate. We hold it absurd to deny that both physical and chemical powers are at work in the living organism, and have a large share in the production of its actions; but it appears to us equally certain that there is another set of powers also concerned—those to which we give the name of *vital*. These last are exhibited only by organized tissues, possessing a certain chemical composition, and a peculiar arrangement of their structure, which no artificial means can produce; and *in all instances*, the generation of a fabric capable of exhibiting vital phenomena is dependent upon the action of a previously-existing organism. Now the manifestation of these phenomena depends upon certain conditions, the failure of one of which is a total preventive of them. Thus a seed requires for its germination not only heat, but moisture and oxygen; and if the seed have lost its vitality, no amount of these other agents can excite the action in question. It appears, then, that the vital property of the seed, acting under the conditions in question, is the source of the act of germination; or perhaps it might be urged that they are all concurrent causes, equally concerned in producing the effect, because it could not take place in the absence of any one of them. But we cannot single out any one of the physical agents just alluded to as more important than the rest; since germination can no more take place without oxygen or moisture than it can without heat.

Hence we deem it very unphilosophical to assert, as Dr. Metcalfe does, that caloric is *the* efficient cause of the vital actions of living beings; since it is only one of several causes which must concur to produce the results in question. And whilst different actions may be performed under extremely wide varieties of condition, in regard to caloric, oxygen, &c., they all agree in being immediately dependent upon those peculiar properties which an *organized* being can alone furnish. But, according to our author, it is owing to the large amount of caloric which surrounds the elements, at whose expense the living fabric is constructed, that they are capable of uniting into ternary and quaternary compounds, having the aptitude for life. On this we would remark, that the doctrine of "ternary and quaternary compounds," as distinctive of the products of organic chemistry, is now every day becoming less probable; in consequence of the revelations made by the progress of the sciences as to the identity between the laws of affinity which regulate the organic and inorganic products of chemical action. Moreover, one of the principal elements of organic compounds, carbon, has never yet been made to assume any other than the solid form; hence the assumption that it contains a larger quantity of caloric than the metallic bases which Dr. Metcalfe regards as so inert, is quite gratuitous. Besides, Dr. Metcalfe takes no account of those other solid substances which enter, though in small quantity, into some of the most important of all organic compounds; and which seem as necessary to their existence as are the oxygen, hydrogen, and azote, on whose exclusively gaseous form his argument is rested. Thus phosphorus is as essential a constituent of nervous matter as azote or oxygen; yet we never know it to exist in any other state (when uncombined) than as a solid body; and its combustibility is not greater than that of many of the metallic bases.

That the energy and rapidity of vital action depend in great degree upon the temperature at which it takes place is a fact which is familiar to all physiologists; and in the large amount of facts which Dr. Metcalfe has collected in support of this position, we recognize little or nothing that was not previously well known. That caloric is the immediate and operating source of these actions, however, seems to us to be a mere hypothesis, by no means justified by the premises. We see constant indications in Dr. Metcalfe's book of his tendency to grasp at facts which support his peculiar views, and to leave all others out of consideration. Thus, he deduces from the well-known physiological truth—that the energy of the vital actions of an animal are in proportion to the amount of its respiration—the inference that the caloric generated by the respiratory process is the immediate cause of these actions, and that the generation of caloric is the sole object of the respiratory process. In discussing this part of his subject, he returns to a doctrine which we had imagined to be long since exploded—that the carbonic acid set free by the respiratory process is *generated* in the lungs; thinking it quite a sufficient reply to the doctrine now almost universally received, to urge that if the carbonic acid is formed in the systemic capillaries, the temperature of venous blood ought to be higher than that of arterial blood, the reverse of which is the case. He takes no notice whatever of the experiments of Edwards, Müller, and others, who have shown that carbonic acid continues to be exhaled from the lungs, when the animals are made to breathe hydrogen or nitrogen; experiments which we have always thought to be far more decisive of this question than experiments on the comparative amount of gases extricable from venous and arterial blood; the results of which will depend rather upon their relative states of combination with the other constituents of the fluid than upon the absolute quantity of each gas. We see no reason why,—on the supposition that the carbonic acid is generated in the system at large, rather than in the vessels of the lungs,—the temperature of venous blood should be higher than that of arterial; for it is quite a mistake to imagine that the union of the carbon and oxygen (the combustion-process) takes place in the *vessels*. It is a process which goes on in the *tissues;* and the blood, circulating through the vessels, contributes to its maintenance by supplying the requisite oxygen, and by removing the carbonic acid produced. The heat generated, therefore, is expended in *keeping up* the temperature of the solids; not in *raising* that of the fluids. That this is the case appears perfectly evident to us, from the consideration of the respiratory process as performed in certain parts of plants in which, there being no circulation of fluid, the tissues themselves act directly upon the air; and when, owing to the peculiar energy with which changes in their constitution are taking place, the amount of heat generated is very large, and becomes evident to our senses. Dr. Metcalfe passes over altogether, also, the well-ascertained fact, that a large quantity of carbonic acid is set free by the skin. His assertion that arterial blood is hotter than venous rests upon the experiments of Dr. Davy, which he states that he has himself confirmed. We do not object to the experiments themselves, which show a difference of about 3° between the temperature of the left and that of the right ventricle; but we assert that this difference is far from being sufficient to account for the whole quantity of heat generated in the body; for that if the carbonic acid were formed in the lungs, and the heat of the whole body were kept up solely by the combustion in them, the difference would be much greater. This, we think, will be evident, when we bear in mind the large quantity of heat which is being continually removed from the surface by radiation, conduction, and evaporation; a quantity which the aortic stream of blood could not supply, unless it were far hotter than it is. The slight difference in question is fully accounted for, as it seems to us, by the absorption of a quantity of oxygen into the blood, in its passage through the lungs, more than equivalent to the carbonic acid disengaged; a process which is well known to be accompanied by the liberation of caloric. A part of this oxygen may very probably enter into chemical combination with the elements of the blood, producing that difference between venous and arterial fibrin which is well known to exist; whilst another part is probably expended in combining with the sulphur and phosphorus, which are taken in as elements of the food, but which are excreted in the urine in the state of oxygen-acids. Dr. Metcalfe might just as well affirm, as it seems to us, that the conversion of sulphur into sulphuric acid takes place in the kidneys, as that the carbon is converted into carbonic acid in the lungs.

It is with great regret that we find ourselves so continually in collision with Dr. Metcalfe, on the fundamental points of his inquiry; and that we feel it necessary to state, that the errors on which we have felt it our duty to animadvert, pervade more or less the whole remainder of his work. The source of these errors we trace to the constitution of his own mind, which leads him to generalize hastily, and with too little consideration for the facts and arguments which run counter to his views. Yet we can conscientiously recommend the perusal of his work to our readers, as containing a large amount of valuable information, together with many ingenious suggestions and deductions. This is especially the case in regard to the Fifth and Sixth books, which treat of the influence of climate and season on the physical and intellectual condition of the human race; the effects of air, exercise, and aliments; the theory of sleep and of temperaments; of fever, inflammation, hydrophobia, tetanus, and other diseases; and of the *modus operandi* of various remedial agents. Like all enthusiasts and system-makers, Dr. Metcalfe undoubtedly does good, by directing attention to many points which would otherwise escape their due amount of notice. The danger is, lest these should occupy the mind too exclusively, and thus draw off the attention from others, which are of equal or even superior importance. There has probably never been a "system" of any kind, which has not had some truth for its foundation; and the impartial and intelligent reader of Dr. Metcalfe's treatise can feel no doubt, that the foundation of *his* system embodies a vast extent of valuable and undoubted truths. But whilst others of equal value and stability are left altogether out of view, the system erected upon this basis cannot be regarded as secure.

In many parts of his work we have encountered what had the appearance of unreasonable dogmatism; and even of undisguised contempt for the opinions of men whom the world holds in the highest estimation. We feel confident, however, that such expressions are the result of the scientific position which the author has thought himself entitled to assume, rather than of the habitual tone of his mind. Of this, we believe, that the closing paragraphs, with which we must conclude this scanty notice, may be considered as a fair expression.

> "And now that I have brought this laborious undertaking to a close, it remains for competent judges to decide how far the principles developed have been founded on a legitimate and comprehensive induction from facts. If true, they must be realized in all the practical concerns of human life, but more especially in improved methods of preserving health and curing diseases. Animated by the grandeur of the subject, and a deep conviction of its vast importance to the welfare of mankind, I have committed myself with unreserved confidence to the guidance of nature, undismayed by the magnitude of the enterprise; believing with Bacon, that in science, as 'in the affairs of civil government, it is better to change many things than one;' and with Sir Edward Bulwer, that 'there does not exist one prejudice which can be called salutary, nor one error beneficial to perpetuate.'
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> "During the prosecution of this task, I have been often reminded of the deeply-rooted prejudices by which the reformer is surrounded; that the mass of mankind have in all past ages been ungrateful to their best friends; that it is generally a thankless office to oppose opinions long sanctioned by custom, and the authority of distinguished names. To all such admonitions I would reply in the words of Sydenham; that "it is better to assist mankind than to be commended by them;" that if the multitude have been always fond of mysteries, fables, traditions, and quack doctors, it is because their leaders have permitted the great science of nature to remain a sealed book, the profoundest of all mysteries. But when the veil which has so long concealed the beautiful mechanism of the universe shall have been drawn aside, all subordinate mysteries will vanish, and with them a countless multitude of pernicious errors, which have hitherto obstructed every avenue to the temple of wisdom. Nor can there be a rational doubt, that a complete knowledge of the prime Mover would be the perfection of science, and enable us to predict whatever should come to pass in the regular course of nature.
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> "It must not, however, be supposed, that more than a general outline of this immense subject has been attempted in the foregoing work. Nor does the author presume to flatter himself that he has been free from error. Nor should it be expected that a pioneer of unexplored regions can become so fully acquainted with all their various productions as those who follow, and have more leisure for research into details.
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> "When the extreme difficulty of the inquiry is duly considered, and the results obtained are contrasted with the previous state of our knowledge; it is hoped that men of enlarged views will be more studious to correct than to censure." (pp. 1098-1100.)

Art. XIX.

On Dysmenorrhea and other Uterine Affections in connexion with Derangement of the Assimilating Functions. By Edward Rigby, M.D. &c.—*London*, 1844. 12mo, pp. 140.

The liability of puerperal patients to attacks of acute rheumatism has been long observed; and, among the exciting causes of rheumatism, gonorrhea, and the suppression of the catamenia or of leucorrheal discharges, have been frequently noticed. A remarkable connexion between dysmenorrhea and rheumatism has also been discovered, and was particularly pointed out by Dr. Gooch and Dr. Locock, (Cyclopædia of Prac. Medicine—Art. Dysmenorrhea.) The relief afforded by the use of colchicum, and also in a remarkable manner by guaiac, in certain cases of dysmenorrhea, as pointed out by Dr. Dewees and Dr. Gooch, and, as we believe, many years prior to them, by the late Professor Hamilton of Edinburgh University, doubtless led to the discovery of this connexion. The truly valuable researches of Dr. Prout into the morbid products of the urinary secretion in derangement of the digestive organs, and particularly in that form of derangement which is connected with gout and rheumatism, have formed the basis of a further inquiry into the relation between the gouty and rheumatic diathesis and dysmenorrhea. The small work of Dr. Rigby forms a contribution to this inquiry, and a summary of the present extent of our knowledge of this subject.

The first part of the work contains a brief sketch of the results of Dr. Prout's observations on the effect of derangement of the assimilating functions in producing morbid excretions from the skin, mucous membranes, and kidneys. It contains also a short notice of the affections of the different mucous membranes in those states of mal-assimilation which mark the gouty and rheumatic diatheses. This leads to the description of those uterine affections which have been observed in connexion with these diatheses, which forms the second and principal part of the work.

The uterine affections described by Dr. Rigby are of an inflammatory or congestive character. He describes two forms: One of an acute nature, characterized by heat, swelling, redness, and pain of the parts, somewhat sudden in its invasion, and erratic in its movements; occurring mostly in connexion with dysmenorrheal attacks, or at the half-way time between the menstrual periods. Another of a chronic nature, characterized by venous engorgement and swelling; attended mostly with leucorrheal discharge, or subacute inflammation of the cervix uteri; and terminating in induration or ultimately in organic disease. These cases are accompanied also with pain, and sense of fulness and distension in the parts, aggravated by motion or pressure, with frequent discharge of flatus from the vagina, and with well-marked evidence of the gouty or rheumatic diathesis in the urine. A tendency to hemorrhoids, a secretion from the rectum also of gas and an albuminous or mucous fluid similar to the leucorrheal discharge, attends the affection and marks the general congestion of the pelvic viscera.

Of the fibrinous exudations which attend cases of dysmenorrhea, Dr. Rigby confesses himself unable to determine the conditions on which they depend, but expresses his conviction that there is an invariable coexistence of inflammatory action in some neighbouring organ—the kidney, the ovary, or the uterus. Of the immediate cause, we believe, there can be only one opinion, viz. that they depend upon the existence of a certain degree of irritation or inflammatory action in the uterus itself in the production of which, we presume, the state of the ovaries or kidneys can only act secondarily.

We must confess our inability to assent to the statements of Dr. Rigby with regard to the termination of the chronic affections described by him in organic disease, in schirrus of the uterus. We are confident that, on the one hand, such affections may and do continue for an indefinite period without such a termination; and on the other, that cancer invades the uterus without any such previous disease.

We have here to complain, also, of a want of precision in the description of these affections; it is nowhere stated in the work before us what is their precise frequency compared with other affections of a similar kind, and the careless reader might be misled by the general tenor of Dr. Rigby's work to believe that, in his opinion, *all* such affections are of a rheumatic gouty character, and that they are never generated, except under the influence of such a diathesis or habit of body.

In the acute affection Dr. Rigby recommends after free purging with calomel and rhubarb, the application of leeches to the anus or os uteri, emollient and sedative injections and the administration of alkalies. We have seen, in such cases, very decided advantage from free and repeated scarifications of the cervix uteri, a mode of treatment which, from the readiness and certainty with which it can be accomplished after the introduction of a tube or speculum, and the trifling uneasiness it causes, possesses many recommendations, particularly in hospital or dispensary practice.

In the chronic cases characterized by the rheumatic gouty symptoms, Dr. Rigby recommends guiacum and iodine, separately or combined, taraxacum, saline remedies, more particularly the saline mineral waters, lime-water, and muriate of lime; and in the dysmenorrheal paroxysms, the hip warm bath, with pills of camphor, lettuce, and hop, or failing these, opium, as the best means of relieving the sufferings of the patient.

Combined with these remedial means, Dr. Rigby lays down very excellent dietetic rules, and furnishes some valuable practical hints for the management of the different peculiarities of individual cases. The work concludes with brief but very good instructions for the mode in which the urine should be examined, and a description illustrated by plates, of the appearances presented by the products of analysis when placed under the microscope.

The work on the whole, is calculated to be exceedingly useful by directing the profession to rational principles of treatment in the diseases referred to. Although it does not pretend to the development of any original views regarding those diseases, or any new discoveries regarding their mode of treatment, it is well adapted to impress upon the attention of practitioners, the importance of constitutional treatment, in a class of diseases, which are, in an especial manner, dependent upon constitutional causes. Something has been done of late years, and much yet remains to be done, in forming correct notions of the pathology of certain diseases, the most prevalent diseases of civilized life, by an examination into the changes produced on the products of excretion, by disorders in the functions of assimilation. To Dr. Prout, the profession is under deep and lasting obligations for his labours in this field of inquiry, and to all who prosecute investigations in the same direction, we are bound to offer our meed of praise, and hold out every encouragement to proceed.

PART SECOND.

Bibliographical Notices.

Art. I.—*Cases of Deformity from Burns, successfully treated by Plastic Operations.* By Thomas D. Mütter, m.d., Professor of Surgery in Jefferson Medical College, Philadelphia, &c.—*Philadelphia,* 1843. 8vo, pp. 24.

The present, together with two other pamphlets, treating of closely allied branches of the healing art by the same author, have just reached us; and we are not willing to pass by the present opportunity of glancing at the performance of Dr. Mütter, although we have the intention of giving the subject of Plastic Surgery, as brought to bear on "the human face divine," a more critical notice and ampler space in an early Number of our Journal. There are few, if any, deformities consequent on accident, short of the irreparable loss of limbs, which have so successfully set at defiance the *art* of surgery for their removal, as those occasioned by burns; indeed the surgeon has felt that, in the production of the deformity which he could fully anticipate and had ample leisure to watch, nature's antagonising powers have overmatched him. We have painfully proved this in a case recently under our own care, in which a large surface of the skin was destroyed, involving the axilla, upper arm, and side. To meet the anticipated contraction, we had an apparatus constructed by a clever mechanist, with a lengthening screw connected to a pad adapted to the side and to a support for the arm; but we were much disappointed by the occurrence of an evil greater than that we sought to obviate, viz. a commencing curvature of the spine (the patient being a boy), resulting from the constrained position assumed by the child, to relieve the distress consequent on the stretching of the granulating surface. We, therefore, hail with satisfaction any attempt to remedy evils which have been either from neglect or unavoidably produced; and we cordially congratulate Dr. Mütter on the success which seems to have attended his operations. We shall satisfy ourselves (for the reason above stated) with merely noticing, for the present, the *result* of Dr. Mütter's treatment in these cases; the *modus operandi* and other details we shall leave for another opportunity.

The first case of a severe burn, involving the face, throat, and upper part of the thorax, and which occurred during childhood, is thus graphically described, as to its effects, by the young lady who was the subject of it: "I have been unable to throw my head to the left side or backwards, or to close my mouth for more than a few seconds at a time for twenty-three years. My right eye was also drawn down some distance below the other, and when I endeavoured to turn my head, it was entirely closed. My condition has been most humiliating and made life a burden; but having good health, I strove to reconcile myself to my hard lot."

We also give the result and consequences of the operation in the patient's words: "The comfort and satisfaction I feel cannot be expressed; your exertions in my behalf have been blessed far beyond my most sanguine expectations. You have set my head at liberty, so that I can turn it any way at pleasure and without pain; you have relieved the drawings of my eye; and I am also enabled to close my mouth with comfort, a blessing that cannot be described." The accompanying figures amply confirm the accounts, and exhibit a remarkable contrast in the appearance of the patient. The second and third cases detailed were of the same nature, and the results marked by the same benefit and improvement. Some excellent "remarks are added," involving the consideration of "the nature of the tissue to be divided or removed;" "the thickness or profundity of the cicatrix, its location, extent, and age;" and lastly, on the "peculiar deformity of the cicatrix," whether narrow, prominent, or complicated by extensive adhesions.

We shall have great pleasure in resuming a more intimate acquaintance with Dr. Mütter's present productions; and shall be pleased, as we have reason to expect, if they lead to an introduction to some more comprehensive work, by which we may judge of the present state of transatlantic surgery.

ART. II.—1. *Researches on Phthisis, Anatomical, Pathological, and Therapeutical.* By P. C. A. LOUIS, M.D. &c. Second Edition, considerably enlarged. Translated by W. H. WALSHE, M.D., &c.—*London; Printed for the Sydenham Society*, 1844. 8vo, pp. 572.

2. *Thomæ Sydenham*, M.D. *Opera Omnia:* Edidit GULIELMUS ALEX. GREENHILL, M.D.—*Londini, impensis Societatis Sydenhamianæ*, 1844. 8vo, pp. 668.

WE stated on a former occasion (Br. and For. Med. Rev. vol. XVII, p. 522), that it was not our intention to *review*, in the ordinary sense of the word, any of the publications of the Sydenham Society, and for the two obvious and sufficient reasons—that the works are not published for sale, and will come into the hands and libraries of almost every one of our readers. Lest, however, it should be supposed that total silence on our part might imply indifference to the interests of the Society, or disapproval of the works published by it, we think it necessary to say a few words respecting the two volumes whose titles we have just transcribed.

I. We regard the fact of two thousand members of the profession becoming, as members of the Sydenham Society, *possessors* of the classical work of Louis, in a language intelligible to all, as one of great importance. This work, to use the words of Dr. Walshe, is "universally admitted to contain the most profound exposition of the NATURAL HISTORY of chronic disease, of which the literature of any age or country can boast;" and as it cannot be doubted that much of the bad practice, and of the false appreciation of the value of remedies in chronic ailments generally, and especially in phthisis, originates in ignorance of the real natural course of diseases, we look forward to corresponding benefit from the lesson that will be thus extensively taught. Had Dr. Ramadge, Dr. Cronin, Dr. Turnbull, Dr. Hastings, and the host of other blockheads who of late years have come forward as curers of consumption, been acquainted with the natural history of this malady, they would never (supposing them honest) have themselves credited the results they have attempted to pass on the profession and the public as facts. And, although we do not reckon on such men being teachable, we believe that many honorable practitioners will rise from the study of Louis with much juster views of the natural progress of diseases, and of the true value of therapeutic agents, than they possessed before. It is this prevalent ignorance of the *natural history of diseases*, that gives to homœopathy its strong hold in public estimation, and enables it to defy the arguments of its professional opponents. If we knew what *nature* could do, or could not do, in any particular disease, then we could pronounce of the true value of the ten-trillionth part of a grain of any common vegetable juice in curing it; but while we are ignorant of this, we cannot say, with certainty, that the conceivable cure was not effected by the inconceivable dose; though, at the same time, we can say, with the utmost truth, that—thanks to the same ignorance of nature's powers—the homœopathist has not one ten-trillionth of a tittle of proof that such was actually the case. Before we can believe his doctrines, he must show us that nature cannot do what he professes to do.

Our readers are probably aware that Dr. Cowan of Reading published an excellent translation of the first edition of this work. His engagements prevented him acceding to the request of the council of the Sydenham Society, that he should undertake the translation of the new edition. The council were fortunate in finding such a substitute as Dr. Walshe. He has performed the task of translator in an admirable manner; and to all future time the *Researches on Phthisis* will remain, in every sense of the word, one of the English medical classics.

II. Of the new edition of the Works of Sydenham we need say but little. As no one can doubt of the inestimable value of the book, so no one can doubt of the propriety of its being one of the new society's earliest publications. The present volume contains the Latin version only; the English edition will be published next year. As might be expected, from the well-known erudition and talents of Dr. Greenhill, we have here an edition of our great countryman's writings, of the utmost accuracy and elegance. It is, without question, superior to any that has preceded it. The text has been revised throughout with the most scrupulous care, and illustrated by the most judicious notes and references. These are the more to be commended, that they occupy a very small space. A most valuable addition is that of the corresponding modern names of the old medicines and formularies; in the preparation of which the editor acknowledges the valuable assistance of Drs. Pereira and Royle.

The two volumes now noticed, and Dr. Babington's Hecker, noticed in a former Number, constitute the first year's publications of the Sydenham Society. In looking at their intrinsic value as literary productions, at their beauty and elegance as specimens of typography, and at their relative commercial value as mere material books, we have proof that all our anticipations of the great importance of the Sydenham Society to the medical profession have been already more than realized; and we entertain no manner of doubt that, when it is generally known that three such volumes as these now on our table (the selling price of which in the shops would be *at least* two guineas) are promised to the members, in return for their annual subscriptions, the supporters of the Society will soon be doubled or tripled. The present number of members is about two thousand.

ART. III.—*A Treatise on the Use of the Sympathetic Nerve and its Ganglions, with their Influence on various Diseases of the Abdominal and Pelvic Viscera.* By T. B. PROCTER, M.D. M.R.C.S., &c. With Three Lithographic Plates.—*London,* 1844. 4to, pp. 48.

OUR want of precise knowledge in regard to the functions of the sympathetic nerve must be freely admitted; and any contribution to an improved acquaintance with its structure and offices must be gladly hailed by the anatomist and physiologist. Our first glance at this work led us to hope that it might contain some really original facts or deductions; but, we regret to say, that a further examination of it has left our knowledge of the subject very much where it found it. Dr. Procter has added no new fact to the anatomy of the sympathetic nerve; and in regard to its physiology, his dominant idea appears to be, that it exerts a regulative power over the supply of blood to particular organs and its flow through them, (and consequently over the processes of nutrition, secretion, &c.) by its action upon the contractile coats of the arteries. In support of this view, he only adduces a single experiment of his own, and seems ignorant of much that has been done by others. The doctrine in question has been already very clearly stated by Dr. Carpenter in his 'Human Physiology,' (§§ 423, 502, 504;) and much more evidence is there adduced in its favour, within the compass of a few lines, than is contained in Dr. Procter's quarto treatise. His single experiment, however, is sufficiently interesting to deserve mention. "A horse was killed by dividing the medulla in the French way, the bowels turned aside, and the branch of the sympathetic nerve, which joins the ischiatic, laid bare; also one of the arteries of the leg. A wire, applied to the positive pole of a galvanic battery, defended with sponge, was applied to the nerve; and the negative wire to the artery: the positive wire was then drawn slowly along the plates of a fifty-plate battery, and the effect was certainly not only to reproduce the pulsation in the artery, but also clearly to excite circulation in the more minute vessels." As a proof that he was not unduly influenced by his preformed theory, in relating the resuts of his experiment, (which he has subsequently repeated with the same result,) Dr. Procter mentions that one of the knacker's men said, "see how that *pipe* beats when they put on those wires;" and that the rest of the men expressed themselves satisfied of the fact.

Dr. Procter adds, in an appendix, some cases in which strychnia was given, to excite what he deemed the indolent action of the sympathetic nerve in amenorrhea, impotence, &c.; and with successful results. He does not, however, mention the doses in which it was given, nor does he give any *unsuccessful* cases. We recommend him, therefore, to give a fuller account of his practice, for the benefit of his professional brethren.

ART. IV.—*Ueber Wiedererzeugung der Knochen nach Resectionen beim Menschen.* Von D. KAJETAN TEXTOR.—*Würzburg*, 1844.

On Regeneration of Bone in the Human Subject after Excision. By D. KAJETAN TEXTOR.—*Würzburg*, 1844. 8vo, pp. 26.

THIS little *brochure* constitutes a practical exposition of the advantages to be derived from partial excision of bone in many cases both of disease and traumatic injury, preferably to amputation or exarticulation. The author discusses the objections usually deterring surgeons from this operation, regarding them as untenable for the most part, or at least only as what will apply to any important procedure in surgery. In a very brief compass, we have here the value of the operation abundantly illustrated; and the successful results of the cases recorded in the present publication, amply bear out the author's commendations of the practice. The examples given of the reproductive power in bone after exsection, in many instances, are both curious and interesting, as well as important in the practical conclusions to which they lead. Altogether, we have to express our decided approval of this little pamphlet, and to assure its author that the profession will not fail to appreciate the importance of his contribution. We have him particularly to thank for letting us have the *matter* in a few words, and not, as is too much the fashion of the present day, obscuring valuable facts by superfluous garniture in order to make a *book*.

ART. V.—*Traité Analytique de la Digestion; considérée particulièrement dans l'Homme et dans les Animaux vertébrés.* Par N. BLONDLOT, Docteur en Médecine, Professeur (adjoint) de Chimie à l'Ecole de Médecine de Nancy, &c. &c.—*Paris*, 1843.

An Analytical Treatise on Digestion, considered especially in Man and in Vertebrated Animals. By N. BLONDLOT, Doctor of Medicine.—*Paris*, 1843. 8vo, pp. 472.

THIS treatise contains the results of a series of experiments, which were made with the view of following out the line of inquiry that had been so successfully opened by Dr. Beaumont. As cases of fistulous opening in the stomach, however, do not present themselves every day in the human subject,—still less frequently, in a subject possessing the vigorous health of Alexis St. Martin,—Dr. Blondlot proceeded to *establish* them in a couple of unfortunate dogs, by means of incisions through the abdomidal parietes; and he states that one of them is still alive, and in excellent condition, the other having been sacrificed for a particular object. The results which he has obtained do not appear to us sufficiently important to require more than a brief notice. He maintains that the salivary fluid has no influence in accelerating the process of digestion;—a position which is probably far too exclusive. We can as little accord with the conclusions of Dr. Wright, who (in a series of papers communicated to the Lancet not long since) maintains that the saliva is *the essential* agent in the digestive process. We believe that the truth, as usual, lies between the two extremes; and that the peculiar matter of the saliva acts as an incipient *ferment*, especially upon the farinaceous part of the food. The ready solubility of the animal tissues in the gastric fluid, probably renders the preliminary process of insalivation less important in the carnivorous tribes than in the herbivorous.

M. Blondlot's chemical analyses of the gastric fluid of the dog have led him to the remarkable conclusion, that neither the hydrochloric nor the acetic acid exists in it in a free state; and that its acidity is due to the presence of the acid phosphates of lime and ammonia. He founds the former part of this statement upon the fact, that neither of these acids could be obtained from the fluid by distillation. But that this conclusion is not generally applicable, is evident from the fact that, *by the very same method*, Dr. Dunglison obtained *a large quantity* of these acids, especially the former, from the gastric fluid of Alexis St. Martin. M. Blondlot considers the animal matter in the gastric fluid in the light of a *ferment;* in which he accords with the results of the most recent chemical inquiries. He regards starchy matters as properly *dissolved* in the gastric fluid, provided the starch-vesicles are ruptured; whilst protein-compounds, gelatin, and vegetable tissues are only *reduced* to a molecular pulp. We scarcely think that this division will stand the test of further inquiry.

Art. VI.—*Human Physiology; with upwards of Three Hundred Illustrations.* By Robley Dunglison, m.d., Professor of the Institutes of Medicine in Jefferson Medical College, &c. &c. Fifth Edition, greatly modified and improved.—*Philadelphia*, 1844. In Two Vols. pp. 1304.

We have, on two former occasions, brought this excellent work under the notice of our readers, and we have only now to say that, instead of falling behind in the rapid march of physiological science, each succeeding edition brings it nearer to the van. Vithout increasing the bulk of the treatise, the author has contrived to introduce a large quantity of new matter into this edition from the works of Valentin, Bischoff, Henle, Wildbrand, Müller, Wagner, Mandl, Gerber, Liebig, Carpenter, and Todd and Bowman, as well as from various monographs which have appeared in the Cyclopædias, transactions of learned societies, and journals. To make room for this, a great proportion of obsolete matter, which we always considered a blemish in the work, has been removed, and there is now very little that we should care to see omitted. To those who desire a work on physiology more extended than that of Dr. Carpenter, and more readable than that of Professor Müller, we can strongly recommend this treatise of Dr. Dunglison's. The large mass of references which it contains, renders it a most valuable bibliographical record, and bears the highest testimony to the zeal and industry of the author. There is only one department in which we think his work deficient; and that is in regard to subjects of microscopical inquiry. From various hints let fall by him, we infer that he has not been in the habit of using the microscope, in its present improved form at least, and that he is consequently unable to test and verify the observations in which the last few years have been so fertile. Hence we find him expressing doubt on some points which, we think, have been satisfactorily determined; and speaking confidently as to others, on which we should have felt more hesitation.

To the present edition have been added ninety new wood-engravings; most of them being those prepared (by the same publisher) for the illustration of an edition of Dr. Carpenter's Human Physiology, which was reprinted in Philadelphia last year. We are happy to see that, owing to the great demand which at present exists in America for physiological works, there is ample room for both these treatises.

ART. VII.—1. *Chemistry, as exemplifying the Wisdom and Beneficence of God; being the Actonian Prize Essay.* By GEORGE FOWNES, PH.D., Chemical Lecturer in the Middlesex Hospital Medical School.—*London,* 1844. Small 8vo, pp. 184.

2. *An Introduction to Practical Organic Chemistry; with references to the works of Davy, Brande, Liebig, &c.—London,* 1843. 12mo, pp. 90.

3. *The Chemical and Physiological Balance of Organic Nature; an Essay by* MM. DUMAS and BOUSSINGAULT, Members of the Institute of France. Third Edition, with New Documents.—*London,* 1844. 12mo, pp. 166.

OUR readers need not be told, that the recent progress of organic chemistry is rendering it quite the fashionable subject of the day; and so it will probably be, until the brilliance of novelty has faded, or a new discovery in some other branch of science has (to use a vulgar but very expressive phrase,) "taken the shine out of it;" or until it is found as we believe it shortly will be, that there are many things in it which are *new,* without being *true.* Nevertheless, we hold it incumbent upon every medical man to do his best to keep pace with the progress of the science; because, even in its present state, it is capable of affording to the intelligent practitioner many valuable hints in regard to the treatment of disease; and also because the public in general are now becoming so very learned on the subject, (thanks to the articles of Drs. Playfair and Gregory in the Edinburgh and Quarterly Review,) that the name of Liebig is becoming a household word, and the incense he receives is little less than that which was offered to the deities of old.—The first work on our list is not professedly a treatise on organic chemistry; but this department of the subject naturally occupies a large part of the author's attention. The sketch it contains of the present condition of the science, is extremely lucid and comprehensive; but we think that, as a treatise on natural theology, it is in many points deficient.—The second of these little volumes is the fourth of the series of 'Small Books on Great Subjects;' on some former parts of which we have heretofore bestowed our warm commendation. The present treatise fully bears out the character of its predecessors; being like them, a masterly outline of the subject it embraces, drawn with a vigorous hand, and a truly philosophic spirit, and developing the practical applications of the truths it enunciates, as well in regard to the arts of life, as with respect to the mental improvement of the cultivator of science.—The third is a translation of a discourse delivered by M. Dumas at the Ecole de Médecine; which is probably already familiar to most of our readers, through the pages of the British journals, into which it was transferred at the time of its first publication. It now appears in a separate form, with many additions, and with the results of new experiments. Although the discourse was delivered by M. Dumas, and passes by his name, he desires that it should be regarded as the expression of the views, at which M. Boussingault and himself have jointly arrived, from their united researches on this subject. It is pleasing to see that such harmony can exist between two labourers in the same field; and we could wish that it were more prevalent. In particular, we cannot but regret the schism which exists between the leaders of the French and of the German schools; each party accusing the other of having pirated their opinions. Whether M. Dumas, or M. Liebig, was the first to enunciate the doctrines in question, we shall not now attempt to determine; but this much is certain, that both have taken as their foundation the patient and laborious researches of M. Boussingault, to whom a very large share of the merit is therefore due. No student of organic chemistry should be without the last of these treatises; and to those who desire to acquire a general knowledge of what is being done in this department of science, we strongly recommend the perusal of either or both of the two former.

ART. VIII.—*The Principal Offices of the Brain and other Centres.* By JOSEPH SWAN.—*London,* 1844. 8vo, pp. 32.

THIS pamphlet is obviously intended by its author, as a comprehensive sketch of the laws of nervous agency; and coming from one who has made the anatomy of the nervous system an object of so much industrious and extended study, it has a strong claim upon our attention. It contains an excellent summary of the present state of our knowledge in regard to the functions of this system; and is valuable as proceeding from one so well acquainted with its structure. But we fear that the *form* in which the ideas are expressed, and the mode in which terms are employed, will prevent this summary from being as useful as it might otherwise have been. The language is generally of so peculiarly *abstract* a character, that we have been compelled to use no little exercise of thought, to discover the author's meaning on several occasions. And we must strongly object to the employment of the term perception to "an impression made on the nervous system, of which the animal is not conscious." Perception is the act of *perceiving;* and a being can scarcely be said to perceive that of which it is not conscious. Besides, what is the use of using any other term than impression for the act in question? The consciousness of the impression becomes sensation; and the excitement, by the sensation of that elementary idea of the nature of the object, which is altogether different from the sensation itself, constitutes what is generally and very appropriately termed perception, in this country at least. Moreover, Mr. Swan converts the term sensory, which has hitherto had only an adjective sense in our language, into a substantive; using it, we presume, as the representative of the Latin *sensorium.* This last term, however, is so constantly employed by writers upon physiology aud metaphysics, that we cannot perceive any objection to it; and the introduction of the word sensory in any other mode than that in which it has hitherto (we believe) been universally used, is liable to produce an injurious confusion. We believe, however, that these blemishes, when properly regarded, will not be found to detract seriously from the value of Mr. Swan's pamphlet; and that it may be profitably studied by those, who desire to become acquainted with the general views at present attained on the physiology of the nervous centres. On no other point of importance do we feel called upon to express dissent from him.

PART THIRD.

Original Reports and Memoirs.

REPORT ON THE PROGRESS OF TOXICOLOGY,
IN RELATION TO
MEDICAL JURISPRUDENCE, MEDICAL POLICE, CHEMISTRY, AND PHARMACY, FOR THE YEARS 1843-4.

By Alfred S. Taylor,
Lecturer on Medical Jurisprudence and Chemistry in Guy's Hospital.

The object of this paper is to present, in a condensed form, the principal discoveries which have been made in relation to toxicology, during the past year. Many of the facts have been selected from English medical periodicals, as these have a peculiar interest to the British practitioner; but some have been also derived from the two principal medico-legal journals of France and Germany, namely the 'Annales d'Hygiène,' and Henke's 'Zeitschrift der Staatsarzneikunde.' Notices of the more important trials for poisoning that have taken place in Great Britain during this period, are appended. Some cases have necessarily been omitted; the selection being here confined to those facts which appeared to have any practical interest.

IRRITANT POISONS.

Cold liquids. The symptoms produced by cold liquids when swallowed in large quantity, have been often mistaken for those of irritant poisoning, and where death has ensued, this has been ascribed to poison. Facts of this kind, which are of some value in aiding our diagnosis in cases of poisoning, have been hitherto imperfectly observed and but rarely recorded. Mr. Cridland of Chelsea has reported the following case, (Lancet, Oct., 1843,) which occurred in the month of August preceding, at a time when the weather was hot and sultry. A child, aged 9, rose from her bed with her body much heated, and drank off a small cupful of cold water. She immediately fell to the ground in a state of insensibility. Half an hour afterwards, when seen by a medical man, she was unconscious; her skin was cold, the pulse feeble, and the pupils were unaffected by light. There were also convulsive twitchings about the corners of the mouth. She was bled, stimulants were applied, and in about five or six hours she recovered. The effects were here probably due to a shock to the nervous system. Had the nature of the liquid taken been unknown, the symptoms might have been speciously referred to a dose of prussic acid.

In regard to the effects of cold liquids and the medico-legal questions which arise respecting them, the reader may refer to an elaborate paper on the subject by Dr. Guérard, in the 'Annales d'Hygiène' for January 1842, p. 42.

Mechanical irritants. Sponge has been regarded as a mechanical irritant capable of producing inflammation and death. Accidents of this kind have been observed in young infants. In May last, Dr. Chowne reported to the Westminster Medical Society, a case which, however, shows that this substance sometimes exerts no action whatever upon the system. An infant, three months and a half old, swallowed a small piece of sponge, which had been placed in the nipple of a sucking bottle. A dose of castor oil was given, and the sponge was passed per anum fourteen hours after it had been swallowed. There were no symptoms.

A case has been tried in this country, where a woman was charged with the death of a child by the administration of sponge.

Diagnosis of cases of poisoning. It is well known that there are certain diseases which in their symptoms and rapidly fatal character, closely resemble cases of irritant poisoning. The diagnosis is here of great importance, not merely in relation to treatment, but in regard to the defence which may be set up, should the individual die and a person be charged with the crime of poisoning. In the law-reports, many cases are recorded in which, on trials for arsenical poisoning, the poison not having been detected in the body, the parties charged with the crime have been acquitted, owing to the difficulty of distinguishing them from cholera and acute inflammation of the stomach and bowels. Cases of arsenical poisoning which have occurred in autumn, have been often treated as cases of cholera, and the mistake has been discovered only by an examination of the body or by the confession of the criminal. It has been doubted by some pathologists, whether *gastritis*, enteritis, or peritonitis, diseases which in their symptoms somewhat resemble cases of irritant poisoning, can occur spontaneously or without an obvious and apparent cause. It is certainly rare to hear of cases of idiopathic acute *gastritis* occurring in individuals otherwise healthy, but a well-marked instance of the kind has been lately reported by Mr. Berncastle, (Lancet, March, 1844.) The symptoms were of the usual character; constant vomiting, no diarrhea, and rapid sinking. After death, the stomach was found in a high state of inflammation, but all the other viscera were healthy.

The case of the Queen *v.* Hunter, tried at the Liverpool Spring Assizes, 1843, is worthy of the attention of medical witnesses in relation to the appearances produced by irritant poisons, and the diseases above referred to. In this case, a woman was charged with having poisoned her husband by arsenic. The medical evidence rested chiefly on the symptoms and post-mortem appearances, for no arsenic was discovered in the body. The mucous membrane of the stomach and intestines was found, throughout its whole extent, exceedingly inflamed and softened. The medical witnesses for the prosecution referred this condition to the action of arsenic; those for the defence considered that it might be owing to idiopathic gastro-enteritis, independently of the exhibition of any irritant. The circumstances of the case were very suspicious; but the prisoner was acquitted, not merely on account of the variance in the medical evidence, but from the absence of positive proof of poison, i.e. its detection by chemical analysis. This generally weighs much with a court of law, although it is well known that arsenic cannot always be detected in the body of a person who has undoubtedly died from a large dose of that substance. It is right to state, as a warning to medical witnesses, that the judge who tried the case expressed regret that, on the non-discovery of poison in the contents of the stomach and intestines, the soft parts of the body (the muscles) had not been examined according to the processes lately suggested by Orfila. (See the published reports of the case by Mr. Holland and Mr. Dyson.)

Perforation of the stomach and intestines. This disease often leads to death under circumstances resembling irritant poisoning. Any well-ascertained facts, therefore, connected with the subject are of some interest to the medical jurist. In November 1843, Dr. Seymour reported two cases of death from perforation of the stomach to the Medico-Chirurgical Society. The subjects of the two cases were, as usual, females: one about the age of twenty, the other twenty-five. Perforation of the stomach generally proves fatal in from eighteen to thirty-six hours, by inducing peritonitis; but these cases were remarkable in the circumstance, that one of the females lived ten days and the other a fortnight after the probable time of perforation. On inspection, the ulcers in the stomach were found to communicate with cysts.

There is one insidious form in which perforation of the intestines may present itself, and simulate irritant poisoning, although the real cause of death (peritonitis) will be immediately apparent on inspection. I allude to the formation of an aperture by ulceration, in the extreme end of the appendix vermiformis cæci owing to the pressure produced by some calculous concretion. Two cases have been communicated to me, both of which occurred in young men apparently in good health; and they proved speedily fatal under the usual symptoms. In both cases the perforation was produced by a hard substance lodged at the end of the appendix. These substances were sent to me for analysis. In one case the calculus was about the size of a large pea, and it consisted of inspissated mucus, biliary matter, and a large quantity of carbonate of lime; in the other the calculus was smaller, but similar in composition. For the detection of such cases, a careful inspection is required, since, both the aperture and the calculus being small, the source of the fatal effusion might be easily overlooked.

It is not often that cases of narcotic poisoning are mistaken for those of disease, but still, where the facts are intentionally concealed or wilfully misrepresented by the criminal administrator, any medical practitioner is liable to be misled. The diseases which in their symptoms and course resemble narcotic poisoning, are generally well-marked in their characters during life, or in the post-mortem appearances found on inspection. A case has just been tried at the Lincoln assizes, (July, 1844,) which shows that a crafty criminal may easily deceive a medical practitioner, and that the coroner's inquest, as it is at present conducted, is not fitted to detect these secret cases of poisoning. In this case, a confession was made; but how many instances escape detection for want of a confession on the part of a criminal, it is impossible to conjecture. An inspection of a body is not required by many coroners unless there are strong circumstances for suspicion in the shape of public rumour; but in respect to criminals, who have well calculated their plans, these circumstances are not likely to come to light *except from a post-mortem inspection, and an analysis of the contents of the viscera.** It does not appear that any inquisition was held or inspection made in the case alluded to, until some time after the bodies of the deceased had been interred, and then it was too late. A woman was charged with the murder of three children by poisoning one of them with arsenic, and the other two with opium. She pleaded guilty, and confessed the manner in which the crime was perpetrated. She had succeeded in poisoning two of the children without being detected, although suspicion was so strong that she was tried, but acquitted, at the previous assizes, on the charge of having poisoned one of them. In the third case, she admitted having secretly given to the deceased, (her own infant,) about three weeks old, a teaspoonful of laudanum. The child was soon afterwards seized with convulsions; a medical practitioner was sent for, who, deceived by the statement of the woman, treated it as a case of ordinary convulsions in children, and ordered a warm bath. The child died in about twenty hours, continuing, according to the prisoner's statement, in convulsions during the greater part of that time. No suspicion appears to have been entertained of the real cause of death, and the case would probably have remained undiscovered, but for the prisoner's confession. It is remarkable that this child survived so long; the woman, however, prevaricated as to the quantity of laudanum which she gave it, therefore it is difficult to draw any conclusion from her statement, except that the deceased was actually poisoned by opium. (The Queen *v.* Joyce.)

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* In one case just referred to me from the country (Sept. 1844), the jury, under the direction of the coroner, returned a verdict of death from poison ("misadventure,") while the stomach of the deceased was in my custody, and *before it had even been opened, or the seals of the vessels containing it had been broken!* In another, now under examination, in which there is the very strongest reason to suspect death from poison administered by a quack, the coroner and jury declined waiting for an analysis (not yet completed) of the contents of the stomach, although strongly advised by the medical witness who inspected the body,—and returned a verdict of "*natural death.*" It is absurd to talk of coroners being legally responsible for such an abuse of their office. The juries, who act under them, are very unlikely to be the complaining parties; the country magistrates, even if they were made acquainted with the facts, are not likely to conceive the necessity for such an additional charge to the county rates as these chemical analyses would often entail; and lastly, where is the medical practitioner who can afford the time and expense of enforcing legal proceedings against a county coroner, even supposing him to be desirous of filling such an invidious office as that of public prosecutor? In the *theory* of the English law, "there is no wrong without a remedy;" in the *practice*, the wrongs are numerous, and the remedies might as well not exist, for they are in many instances quite unattainable!

Phosphorus. It is not often that we hear of cases of poiscning by phosphorus or its compounds, but the following instance has been reported by Mr. Shephard of Stonehouse. (Lancet, Dec. 1843.) A child, between two and three years of age, had been caught in the act of sucking and swallowing the heads of lucifer matches. Two days afterwards she appeared unwell, there was some feverish excitement, but no active symptoms. The bowels were open, but the child did not suffer from pain, vomiting, or diarrhea. Five hours after she was first seen, she became violently convulsed, and she died three hours afterwards. On inspection, a quantity of mucus mixed with blood, of a coffee-ground colour, was found in the stomach. The mucous membrane of the organ was very vascular throughout, and for the space of about two inches it had a florid red colour, and was covered with mucus. There were no less than ten invaginations in the small intestines, many of which included from two to three inches of intestine, which was inflamed at the invaginated parts. There was no appearance of strangulation, and the bowels were empty. The medical opinion given at the inquest was that phosphorus, in a finely divided state, was the cause of death, and a verdict was returned accordingly.

Arsenic. The subject of arsenical poisoning continues to receive much attention from toxicologists, and suggestions are continually being made for improving the numerous processes which are now employed for the detection of this poison. Among the late announcements on this subject, is a new method of employing Marsh's test, suggested by Mr. Ellis of University College. Instead of burning the arsenuretted hydrogen 'gas, and receiving the deposit of metallic arsenic on cold plates of glass, porcelain, or metal, Mr. Ellis proposes to decompose the gas and deprive it of arsenic by passing it over metallic copper, or one of the oxides of that metal. In passing arsenuretted hydrogen gas, generated in the usual way, over slips of metallic copper 'gently heated, he found that this metal acquired a gray coating of arsenic similar to that procured in the application of Reinsch's test. (See Report, No. XIX of this Journal, p. 275.) If the surface of the copper were oxidated, then no heat was required for the decomposition. Metallic arsenic was immediately deposited when a current of the gas was passed over the oxidized surface, owing to a decomposition of the gas and the oxide. This discovery led Mr. Ellis to employ at once oxide of copper instead of the pure metal, and he found that during this decomposition, the black oxide became gray, and the red oxide black. The well-marked change of colour, rendered the use of the latter preferable, as it at once served to indicate the progress of the decomposition. The arsenuretted hydrogen gas is first dried by passing it over fused chloride of calcium, contained in a bulb attached to Marsh's apparatus; and it is then allowed to pass over the oxide of copper in a tube, the current of gas being controlled by a stop-cock; for if it should pass too rapidly, it escapes decomposition. No heat is required for the change;—the oxide of copper absorbs the gas as it slowly passes and becomes heated;—water is produced, and the arsenic is retained by the oxide, either as arsenuret or arsenite of copper. By occasionally igniting the jet at the end of the tube, it may be seen whether the arsenic has been entirely removed from it or not, and the process arrested or continued accordingly. When the oxide is saturated with arsenic, it may be removed and gently heated in a small reduction tube; a brilliant ring of octohedral crystals of arsenious acid will be thereby obtained. These crystals are equally produced in an atmosphere of carbonic acid or hydrogen, showing that the arsenic must either have existed as arsenious acid in the compound, or have derived its oxygen from the oxide of copper. When a sublimate of arsenious acid has been thus procured, it may be dissolved in water and tested in the usual way.

While it must be admitted that this is a very ingenious process, it cannot be denied that it is open to all the objections to which Marsh's test, as it is commonly employed, is liable. This, however, is apparent, that it is far less liable to lead to a loss of arsenic; and thus, where the quantity of poison is extremely small, we may be more certain of obtaining evidence of its presence by using the oxide of copper, than by merely burning the gas, and exposing a cold plate to the flame. Small sublimates are in this way often dissipated, and a quantity of arsenic is invariably lost at each time the gas is ignited. The objections to the plan are that additional apparatus with very careful manipulation is required. It does not appear to be in any respect preferable to Reinsch's test, and in delicacy is far inferior to the plan proposed by Dr. Clark, of passing the arsenuretted hydrogen gas into a solution of nitrate of silver.

Dr. R. Fresenius of Giessen, has lately caused to be read before the Chemical Society, a series of elaborate papers, since published in the 'Lancet,' (June and July, 1844,) on the detection of poisons generally in medico-legal cases, and on a new method for the detection of *arsenic*. He considers that a method laying claim to general applicability should fulfil the following conditions: " 1. It must admit of detecting arsenic in every form in which this mineral can possibly exist. 2. It must not merely lead to the detection of arsenic, but also to that of the other metallic poisons. 3. It ought to preclude the possibility of confounding arsenic with other substances. 4. It must admit of detecting even very minute quantities of arsenic. 5. The method sought must enable us to obtain at least an approximate quantitative determination of the arsenic detected. 6. It must fulfil all these conditions by the most simple means." However desirable it may be to possess such a method as is here sketched out, the absolute necessity for it is not apparent. Arsenic may be most satisfactorily detected by processes which are not fitted for the detection of other metallic poisons. If each poison has its own particular process, and this is satisfactory so long as it is confined to its proper object, it is impossible to allow that the admissibility of chemical evidence in cases of arsenical poisoning, should be made to rest on the universal application of the same process, and with a like degree of certainty, to other poisons. Dr. Fresenius complains that those who have written innumerable essays upon this subject, have not always had " a very complete or distinct notion of the nature of their task; and they have therefore altered and improved methods *ad infinitum*, which in their very principle could never answer the design that they were intended to subserve."

Out of the many processes suggested for the detection of arsenic in mixtures containing organic matter, there are, according to Dr. Fresenius, only *four* which require to be mentioned. Arsenic may be separated, 1, as arseniate of lime; 2, as sulphuret; 3, from arsenuretted hydrogen; 4, by metallic copper. Of these methods, the second alone is recommended as fulfilling the conditions required, although it is obvious that the poison may be administered in the state of sulphuret, (yellow arsenic;) and therefore this, without some preliminary preparation, cannot fulfil the first condition required, i.e. of detecting arsenic in *every form* in which it can possibly exist. On the separation by lime nothing need be said, as it is now abandoned by toxicologists. Dr. Fresenius considers that the separation from arsenuretted hydrogen (Marsh's test) is absolutely inapplicable to the intended purpose, " because it does not admit of the separation of arsenic in every form in which this substance may exist." Further, it does not contribute to the detection of other metallic poisons; it contaminates the substance under investigation with zinc, which might itself have acted as the poisonous agent; it leads more easily than any other method to mistakes; and does not admit of any correct quantitative determination of the arsenic found. Marsh's test, it is allowed, permits the detection of minute quantities of arsenic in many cases in a very simple manner. Reinsch's process, i.e. the separation by copper, is just as little adapted to the purpose, although, like the preceding test, it may serve to detect very minute quantities of arsenic. " The defects of this method are that it does not admit of the detection of arsenic in every form in which the metal may exist; that it does not at all lead to the detection of other metallic poisons; and that moreover the substance under investigation becomes contaminated by copper. Its success is impeded or prevented by many substances, such as nitrates (?), mercurial and other metallic compounds; so that the advantage of detecting even minute quantities of the poison, can only be conditionally conceded to it. Finally, it does not allow of the quantitative determination of the arsenic present."

It appears to me impossible to assent to the validity of these objections, or to admit that we should be at all justified in entirely discarding the ingenious processes of Marsh and Reinsch, upon such grounds as are here adduced. These tests are fully equal to the separation of arsenic in all the forms in which it is most commonly found in practice; that they do not detect all other metallic poisons, or that their operation on arsenic is occasionally rendered obscure by the presence of other substances, are objections which amount to nothing in the hands of those who limit the application of these tests to the purposes for which they were originally designed; and it is doubtful whether they are more liable to lead to fallacy in skilful hands, than the process by conversion to sulphuret. Neither Marsh's test nor Reinsch's test will answer all the artificial conditions laid down as necessary for a universal method by Dr. Fresenius; but it is questionable how far it is just to measure the practical utility of these tests by such a standard as is here assumed.

Before converting the poison to the state of sulphuret, Dr. Fresenius considers the different plans which have been recommended for obtaining a clear solution, without loss of arsenic, from the organic mixture containing the poison. He gives the preference to the action of chlorine, as a decolorizing agent, and to the process of charring by sulphuric acid. As his method of employing chlorine for removing and destroying organic matter, and at the same time rendering the arsenic soluble, is novel, it is here briefly described with the analytical process.

The substances intended for examination are, if formed of solid and coherent lumps, reduced into small pieces, and under all circumstances carefully intermixed. They are then put into a porcelain basin, and drenched with an amount of pure concentrated hydrochloric acid, either equal or superior to the weight of the dry substances, and with as much water as will give the consistence of thin pap to the whole mass. The basin is then heated in the water-bath, and chlorate of potash, in portions of about half a drachm, is added to the mixture at intervals of about five minutes, and until the contents of the basin have assumed a bright yellow colour, perfectly homogeneous, and a thin liquid consistency. When this point is attained, about two drachms more of chlorate are added, and the basin removed from the water-bath. When cool, the contents are placed on a filter of linen or paper, and are allowed to run off: the residue is washed with hot water until the liquid which passes (which is also collected) is no longer acid. The whole is then concentrated in the water-bath, during which process it changes from a bright yellow to a brownish tint, and a saturated solution of sulphurous acid is added to this residue until the smell of sulphurous acid is clearly perceptible. The whole mixture is then heated for about an hour, until the excess of sulphurous acid is completely expelled. A current of washed sulphuretted hydrogen is now slowly transmitted through this liquid for about twelve hours. The glass containing the precipitated sulphuret is lightly covered, and kept at a temperature of about 86° until the smell of sulphuretted hydrogen has disappeared. The precipitate which, besides sulphuret of arsenic, may contain organic matter and other metallic sulphurets, is collected and washed on a filter, and afterwards dried with it in a porcelain basin heated by a water-bath. Fuming nitric acid is then added drop by drop, until the whole is moistened; it is then evaporated to dryness. Pure hydrated sulphuric acid previously heated is then added to the residue, so as to moisten it uniformly: the mass is again heated in a water-bath for the space of from two to three hours, and finally in a sand-bath, at a temperature of about 300°, until the charred mass begins to crumble. The residue is digested in water, filtered, mixed with hydrochloric acid, and the filtered liquid precipitated by sulphuretted hydrogen. The precipitate thus obtained is drenched with a solution of ammonia, and the ammoniacal fluid is evaporated in a balanced porcelain basin dried and weighed. The arsenic thus obtained as sulphuret may be quantitatively determined in the usual way. The residue, insoluble in ammonia, may be tested for lead, bismuth, copper, and mercury. The metallic arsenic is obtained from the sulphuret by heating it in a current of well-dried carbonic acid, with a mixture of carbonate of soda and cyanide of potassium as the reducing agent.

With the exception of the plan for removing the colour of the organic liquid containing arsenic, there does not appear to be any originality about this process. It is undoubtedly much more complex than some of the others for which it is recommended as a substitute; and unless it were conducted by one well versed in chemical manipulation, it might equally lead to error. The value of the sulphuretted hydrogen as a precipitant has been long known, and is fully appreciated by toxicologists; but this does not preclude the employment of other more ready and equally certain means for detecting arsenic. The process appears to be rather adapted for separating arsenic from its ores, than for detecting it as a poison in medico-legal cases. In practice we do not find the compounds of lead, bismuth, copper, and mercury, mixed with arsenic; and therefore it is useless always to have recourse to a process which invariably presupposes the admixture of these metals with the poison criminally administered. Even in cases of suspected compound poisoning, rare as they are, there is no difficulty in discovering the foreign metal mixed with arsenic by appropriate tests; and a court of law requires to know whether arsenic was present and was the cause of death, rather than whether it was mixed with traces of bismuth or lead, a fact, however interesting in a chemical, is wholly unimportant in a medico-legal view. It is doubtful whether by this process the absorbed arsenic could be detected in the soft parts of the body, with the same readiness as by the test of Marsh or Reinsch, and it is apparent that this is likely to become an important branch of medico-legal research. In short, if this process were to be admitted as the only certain and satisfactory method of detecting arsenic, it would restrict all medico-legal analyses of the poison to a few who are in the daily habit of exercising themselves in chemical manipulation. But the evidence given on numerous criminal trials shows that there are many practitioners who are able to demonstrate satisfactorily the presence of arsenic in mixed liquids, without having recourse to such an operose method as is here pointed out. Dr. Fresenius recommends cyanide of potassium and carbonate of soda as the reducing agent for the sulphuret. I have lately found that one of the best methods for reducing this compound of arsenic, is finely powdered metallic silver. This was recommended about a year since by Dr. A. Frampton for the reduction of corrosive sublimate,—it will be found equally serviceable in the reduction of the sesquisulphuret of arsenic.

In a paper read before the Sheffield Medical Society, and published in the Medical Gazette (May, 1844), Dr. E. Shearman states that the metallic ring, as shown in the reduction-test, can be produced by other metals besides *arsenic*. He also states, that the metallic arsenic may be known by its being deposited in rhomboidal crystals,—that a witness is justified in swearing positively to the presence of arsenic, only, "1, by producing the metal and showing its crystals; 2, by reducing it to the oxide and showing its crystals (octohedral); 3, from these crystals going through all the fluid tests; 4, reducing the sulphuret again to its metallic state, then to the oxide, and again going through the fluid tests." This is like demanding proof *ad infinitum;* for where, it may be asked, is the medical jurist to stop in his analysis? If he obtains the metal, it is said, "Antimony, bismuth, tin, zinc, lead, tellurium, cadmium, selenium, and potassium, sublime in a somewhat similar manner to arsenic, *and may be mistaken for it.*" Medical jurists have hitherto agreed on one point, namely, that the first five metals mentioned never yield, in a small reduction tube and at the ordinary heat of a spirit-lamp, any metallic sublimate whatever. With regard to the other four substances, cadmium is the only body that presents any objection to the reduction-test; and this is rather theoretical than practical. The cadmium-sublimate can never be mistaken for an arsenical sublimate by one who has been accustomed to experiment on the poison. It has a tin-like lustre, and generally a brown fringe of oxide, which, if not developed during the reduction, is readily brought out on reheating the sublimed metallic crust. It is difficult to conceive under what circumstances tellurium, selenium, or potassium, can ever be sublimed in a reduction tube from any solid or liquid substance, handed to a medical jurist for analysis. There is no salt of potash that will yield potassium by heating it with flux in a spirit-flame; and if the metal were used in its pure state, no chemist could proceed to examine it as an arsenical compound without being made immediately aware of its nature. Admitting that the metallic arsenic is deposited in rhomboidal crystals, to be seen only "by a powerful microscope," there can be no better or more satisfactory evidence of its nature, than the production of white octohedral crystals, by reheating it, as suggested long since by Dr. Christison. Dr. Shearman states that metallic antimony sublimes into octohedral crystals like arsenious acid, only differing from this substance in being insoluble in water. In frequently repeating these experiments with a spirit-lamp and tube, such as are used in the analysis of arsenic, nothing of the kind was observed. A powerful microscope was not employed, because this is not needed in these investigations. If such a statement were borne out by observation, Reinsch's test, and the reduction-process for arsenic, would be completely set aside, and chemists would be thrown back upon the liquid tests for the poison. Many toxicologists have submitted Reinsch's test to numerous trials, but in no instance, so far as I am aware, have they ever procured a sublimate in white octohedral crystals from a deposit of antimony on copper. Dr. Shearman is entirely opposed to the opinion of Dr. Fresenius as to the value of the sulphuretted hydrogen test. Dr. Shearman says, "It should be *carefully remembered*, that the *sulphurets of antimony, tin, selenium*, cadmium, and tellurium have nearly the same yellow colour, and are deposited in the same manner as arsenic, and when reduced to the metallic state with black flux, they not only give an appearance so much like arsenic, that it requires a very practised eye (?) to distinguish each, if even that be possible; and tellurium and cadmium also exhale a garlic odour like arsenic. (?)" Dr. Fresenius actually stops at the production of the metal from a precipitate obtained by sulphuretted hydrogen;—he observes that "mirrors of wonderful purity are obtained *without the possibility of confounding them with those produced by any other substance*." Thus Dr. Fresenius regards the medico-legal evidence complete, at the very point at which, according to Dr. Shearman, it becomes doubtful and unworthy of confidence. Toxicologists, it appears to me, will agree that in this respect Dr. Fresenius is correct; and that no practised eye can ever confound the metallic sublimate derived from sulphuret of arsenic or the white octohedral crystals obtained by reheating it,—with the metallic sublimate procured from any other substance precipitated by sulphuretted hydrogen.

Dr. Shearman also remarks, in respect to the alleged antidote for arsenic, "the moist hydrated peroxide of iron," that he has given it to dogs and rabbits after having made them swallow large doses of arsenious acid in powder and solution. On killing them within a short time and examining the bodies, he found in the stomachs, "minute patches of inflammation, but no arsenious acid could be detected by Reinsch's method," nor in any other way. This he considers is "a strong presumption, that the whole of the arsenic was reduced to its metallic state." If a large dose of poison had been given, and none found in the contents of the stomach, either free or mixed with the oxide of iron, it is clear that it must either have been rejected by vomiting, absorbed into the system, or that it must have passed off by the intestines. The fact of the animals having been speedily killed rendered the latter supposition improbable, therefore it is difficult to explain why arsenic was not discovered, if not in the viscera, at least in the soft organs. In experiments on this subject, one point has always been omitted, although it would throw considerable light on the action of the supposed antidote, namely, an analysis of the blood and soft parts of animals to which the oxide of iron has been given after they have been made to swallow large doses of arsenic. There is another way of testing the efficacy of this antidote, which has not been adopted, but which is worthy of a trial, by those who suppose that the oxide of iron is efficacious in cases of arsenical poisoning, namely, to mix powdered arsenious acid with the oxide of iron into a thin paste, and spread it upon a raw ulcerated or wounded surface. The antidote is said to act by preventing the absorption of the poison; if this were so, no evil should follow from the performance of such an experiment, however large the quantity of arsenic. On the contrary, Professor Barzellotti of Pisa found that a compound of this kind acted energetically as a poison on dogs, and the arsenite of copper, which is just as insoluble as arsenite of iron, would very speedily destroy life when thus locally applied. It is, however, impossible to understand how, in any case, the oxide of iron should act as Dr. Shearman suggests, by reducing arsenious acid to the metallic state!

In the year 1843, a great improvement was made in the chemical processes for arsenic by the discovery of *Hugo Reinsch*, that the metal was readily precipitated on *metallic copper*, even from the most complex organic liquids, by boiling them with muriatic acid and then introducing the copper. An account of this test was first published in the 'Journal für Praktische Chemie,' a translation of which will be found in the 'Annales d'Hygiène,' t. xxix, pp. 439-447, with additional observations thereon by M. Gaultier de Claubry, t. xxx, p. 159. A full description of the mode of applying this new and useful test will be found in the 'British and Foreign Medical Review' for July, 1843, (No. XXXI, p. 275). Dr. Christison has also published his observations on the test in the 'Edinburgh Journal of Medical Science,' for September, 1843. The only additional remark which may be made with respect to this test, is, that it will perhaps be found better to employ fine gauze, made of woven copper wire, than plates of copper as originally proposed by Reinsch. The surface of metal is much greater, and the gauze may, after the deposit, be easily rolled up into a small bulk, so as to yield a large quantity of arsenious acid. The copper-gauze which I have used for this experiment, contained 16,400 apertures to the square inch.

Many cases have come to trial during the last year in which parties have been charged with the criminal administration of arsenic. Among these some have presented circumstances interesting to the medical jurist. The case of the Queen *v.* Hunter has already been commented on (ante p. 534.) Another case was tried at the Liverpool Winter Assizes, 1843, (the Queen *v.* M'Cormick), in which the medical evidence was of some importance. A woman was charged with the murder of her child, aged five weeks, by administering to it arsenic. The quantity given was unknown, but the child did not die until twelve days afterwards. The symptoms do not appear to have been very severe or well-marked, indeed, so little so as to have led to some doubt of their cause in the mind of the medical witness. The chemical evidence was, however, very clear: arsenic was not only detected in some of the food given to the child, but also in a portion which had been vomited on its night-dress. It is stated that no arsenic was found in the stomach or duodenum; but the poison was detected in the contents of the intestines as well as in the substance of the brain, liver, lungs, and heart. In cross-examination, the witness was asked respecting the presence of arsenic as a normal constituent of the body. He is reported to have admitted that arsenic might be found in the body, although it had not been administered; but if found in the alimentary canal (free), then it must have been administered. He said that he himself had never found arsenic in the flesh of persons to whom it had not been administered; but he made the statement on the authority of others. He considered that it was not "normal" arsenic which had been found in this case; and that the ten thousandth part of a grain would give perceptible indications of the presence of the poison. The prisoner was convicted.

The mode of examination adopted at this trial clearly shows that barristers are well aware of Orfila's researches; but we have here an instance of the revival of an opinion which has now been for some time abandoned, even by Orfila himself—of the existence of arsenic as a natural constituent of the body. It had no influence on the result of this case; but it might have improperly led to the setting aside of the chemical evidence altogether, had not some free arsenic been found in the intestines.

One of the most important trials which has occurred for some years, was that of Mrs. Cochran or Gilmour, before the High Court of Justiciary at Edinburgh, January, 1844. She was charged with the murder of her husband by arsenic, but acquitted on a verdict of "*not proven*," apparently for want of clear evidence of administration. This trial is interesting from the fact, that the new methods of detecting arsenic were here, for the first time, brought prominently forward. The chemical evidence was complete, and such as left no doubt that the deceased must have died from the effects of the poison. A very full report of this trial has been published, to which I would refer the reader. Drs. Wylie, M'Kinlay, and Christison gave evidence on the occasion. Dr. Christison employed Reinsch's test, and by this means readily detected arsenic in the substance of the stomach and its contents, as well as in the substance of the liver. The same question, relative to the existence of normal arsenic, was put here as in the case of M'Cormick. In answer to it, Dr. Christison says, Arsenic "is not a constituent part of the human body, and is not formed in it. It was once alleged that it was, but that was disproved. The individual (Orfila) who first promulgated this theory only argues now, that small quantities are found in the bones; but in three several experiments before the Academy of Paris he was unable to show it. Arsenic could only have come into the liver by absorption." There can be no doubt that this expresses the truth. The elaborate researches of MM. Danger and Flandin show that there must have been some fallacy attending the original experiments of Orfila, since, in no instance, in operating on the largest quantities of animal matter, and employing the most delicate tests, could they detect the smallest trace of arsenic as a natural constituent of the human body. (See No. XXV of this Journal, p. 183.)

In concluding this account of arsenic, it is right to state, for the information of those engaged in medico-legal analyses, that there is still much sulphuric acid in general use which contains arsenic in large proportion. This is well known to be the acid obtained from pyrites. An impregnation of this kind seriously affects the use of the acid, both as an agent for carbonization and as a means of procuring hydrogen by Marsh's test; and no specimen should be employed for either purpose, until the absence of arsenic has been clearly established by analysis. The best means of detecting this body in sulphuric acid will be by the use of Reinsch's test, or by the action of a current of sulphuretted hydrogen gas on a portion of the acid, diluted and neutralized by potash. The presence of arsenic in sulphuric acid is the cause of the commercial muriatic acid being sometimes contaminated by it. Hence the necessity, in employing Reinsch's test, of being well assured that the muriatic acid used is free from arsenic. M. Scanlan states that the quantity of arsenic in one specimen of English sulphuric acid was so great that it could not be used to obtain hydrogen for the process of antogenous soldering. (Pharm. Journal, August, 1844.) This gentleman found that 2000 grain measures of the acid yielded 1·5 grain of sesquisulphuret of arsenic.

Mercury. Albuminous Antidote. Much discussion has arisen among toxicologists respecting the nature of the compound formed by albumen, when exhibited in cases of poisoning by corrosive sublimate. The great practical question is as to how far it is capable of disarming the poison of its virulence, and upon this most are agreed, namely, that it is a useful counteragent. Orfila has found that the compound may be given in large doses without danger, that it is soluble in a large excess of albumen, and then becomes poisonous, but less so than corrosive sublimate. The common practice in using albumen is to give only the white of egg, but, chemically speaking, the yelk, which is composed of the same principle, with a small quantity of oil, is just as efficacious.

With regard to the compound formed, Orfila's opinion was that the corrosive sublimate was reduced to the state of calomel by albumen, and thereby rendered inert. Lassaigne stated, from his experiments, that the albumen directly combined with the corrosive sublimate and formed an insoluble substance. A writer in the 'Dublin Journal of Medical Science' (May, 1844,) has lately called the attention of toxicologists to the experiments of Professor Rose which correspond in their results with those performed by himself. Prof. Rose considers the compound to consist of albumen united to the peroxide of mercury, and there is no doubt that a compound similar to, if not identical with it, may be at once formed by rubbing up fresh albumen with hydrated peroxide of mercury. The same may be procured by precipitating with albumen "a solution of pure pernitrate of mercury, as nearly neutral as possible." If added to the protonitrate of mercury, the protoxide is thrown down of a grayish-black colour.

In performing lately some experiments on the subject, I have found that the compound, produced directly by the admixture of albumen with the hydrated peroxide of mercury, possesses all the chemical properties of that produced by the action of albumen on corrosive sublimate. Thus it underwent similar changes when treated with chloride of tin, metallic copper, caustic potash and concentrated muriatic acid; but there was one difference, namely, that a small portion of corrosive sublimate was held combined with the precipitate formed in a solution of that poison by the addition of albumen. Albumen was added to a solution of corrosive sublimate, in sufficient quantity to produce the usual dense white precipitate, but not to redissolve it. The clear liquid was poured off, and the precipitate was afterwards thoroughly washed on a filter, until the washings gave no indication of the presence of corrosive sublimate. On adding potash to a portion of the precipitate, there was no apparent change, but on boiling a larger quantity of it in water, filtering and evaporating on a glass plate to crystallization, some minute white prismatic crystals were obtained, which were immediately turned scarlet on touching them with iodide of potassium. They were proved to be corrosive sublimate. The compound was then allowed to dry, when it formed a horny transparent mass. This readily dissolved in boiling concentrated muriatic acid, giving the usual deep purple colour formed by that acid with albumen. On diluting it with water, a precipitation of albumen took place, and the liquid gave an abundant metallic deposit on fine copper gauze. When this was dried and heated in a reduction tube, well-defined globules of mercury were obtained by sublimation. One fact appears to be obvious from this experiment, namely, that admitting the antidotal compound to consist of albumen and peroxide of mercury, it does nevertheless contain some undecomposed corrosive sublimate, not separable by mere washing with cold water, nor detectable by the addition of potash to a small quantity of it, but rendered demonstrable by long boiling in water and subsequent filtration and evaporation.

Alleged poisoning by blue pill. The account of an inquest on a person alleged to have died from the effects of *blue pill*, is reported in a contemporary journal (Medical Gazette, October, 1843.) It appears that the deceased, æt. 40, took some medicine prescribed for him by a practitioner. It consisted of *six grains* of blue pill and three of calomel. This was alleged to have produced salivation and a mercurial fever, of which the man died in about seven weeks. The salivation was probably owing to a remarkable idiosyncrasy, for a smaller dose than that here prescribed has been known to cause fatal salivation. But from the evidence, it was not improbable that the deceased had taken some quack pills which, had their composition been known, might have accounted for the severity of the symptoms. The jury returned a verdict of natural death, but called the remedy administered "an overdose of strong medicine!"

Cancrum oris. A case of cancrum oris in a child, mistaken for *mercurial poisoning*, has been communicated to the 'Medical Gazette' by Mr. Dunn of Norfolk-street. (Vol. xxxiii, p. 57.) Cases of this kind are of great importance, because they often involve practitioners in charges of malapraxis. An abstract is therefore given from Mr. Dunn's report. A girl, aged two years and a quarter, was brought to Mr. Dunn, on the 16th September, 1843. The child had an expression of heaviness about the eyes, the skin was hot, and the pulse quick. The mucous membrane of the mouth was in an unhealthy state, and the gums were spongy; there were blotches upon the body resembling the pustular form of scabies; the child was of a cachectic habit, from residing in an unhealthy locality and from defective nutrition. The following medicines were prescribed: A mixture of carbonate of magnesia and soda; three alterative powders, each containing pulv. rhei, gr. iv, sodæ exsiec. et hyd. c. cretâ (āā gr. ij) alt. noct. sum.; and some camphorated sulphur ointment to be applied to the blotches. One powder only was given; and when the child was seen two days afterwards, the eruption of measles was coming out, but not freely. She was then in a low drowsy state, and there was great prostration of the vital powers. Previously to the child's illness the gums had bled freely, their texture was now of a livid hue and spongy. At the junction of the gums with the lining membrane of the lower lip in front, there were a number of small yellow spots, resembling aphthæ, with a whitish exudation, the intervening mucous membrane being tumefied and red. Mel boracis was used, ten grains of the *compound jalap powder* were given, and a saline mixture with ammonia. Next day the aphthous spots presented irregular ulcerations, with ragged edges; these slowly extended, becoming of a dirty gray colour, and they were covered with a tenacious purulent exudation. The disease extended to the upper lip, right cheek and gums, and the breath was intolerably fetid. In spite of the application of the usual remedies, the disease gradually progressed in the dry form, the gums of the lower jaw were reduced to a black fetid pulp, and the child herself removed the whole of the teeth, one by one. The lower lip and chin became involved, and the first external eschar appeared on the chin, about a week after the appearance of the measles; the lower part of the face then became a black, soft, and homogeneous mass, having a gangrenous fetor. The child died fifteen days after the time at which she was first seen. An inquest was held, and it was alleged that ten grains of the hydr. c. cretâ had been given to the child twelve days before her death, instead of compound jalap powder. There was no proof of this, nor did it appear that the child had throughout her illness taken more than two grains of the hydr. c. cretâ, about a fortnight before her death. The medical evidence satisfactorily proved that there had been no improper treatment. The post-mortem appearances met with were as follows: "The right side of the face, half of the nose, and upper and lower lip were perfectly black. Upon carefully examining the inside of the mouth, there was observed great ulceration of the gums. The alveolar processes were denuded, and the teeth gone. Half of the tongue was black, and the inside of the cheek and fauces gangrenous, the whole exhibiting *the true* "*cancrum oris*," or gangrene of the mouth. The stomach was perfectly healthy, and the small intestines diaphanous."

The fact that this was a case of cancrum oris appears to be established affirmatively, by the well-marked characters of the disease; and negatively, by the small quantity of mercury which had been taken by the child at a long period before the serious symptoms came on. This disease has not received so much attention from practitioners as its importance really deserves. One of the best descriptions of it has been published lately by Dr. Hunt in the 'Medico-Chirurgical Transactions.' (Second Series, vol. viii.) According to this gentleman, it commences by small ulcers, either on the inside of the cheek or at the point of junction of the mucous membrane of the cheek and gums, or in the gums themselves, separating them from the teeth; they are very painful and tender, and accompanied by profuse salivation. The breath soon becomes tainted with an offensive smell, not unlike the mercurial fetor. If the disease be neglected, the ulceration goes on to destroy the gums, the teeth become loose and fall out, and the alveolar processes are laid bare. The brown ragged ulcer spreads rapidly on the inside of the cheek; the integuments over the spot corresponding to the ulcer become hard and swollen—at first white, and afterwards of a dull red colour, and shortly a black spot appears in the centre, which quickly spreads, and destroys more or less of the cheek. Should the child survive, there is much deformity, and it loses the power of opening its mouth, from the unyielding nature of the cicatrix; but more commonly, when the disease has gone to this extent, the child sinks and dies.

Tests for corrosive sublimate. It has been objected to the ingenious test proposed by Dr. Frampton for corrosive sublimate, either solid or in any state of admixture, (Medical Gazette, June, 1843,) that it will not answer in all cases, and that it is inferior in delicacy to tin and the chloride of that metal. Direct experiment, however, shows that these objections are more theoretical than practical. Metallic silver in a finely pulverulent state is easily procurable, and its efficacy as a test may be made evident by the most simple experiments. It has many advantages over the chloride of tin, and acts so perfectly in separating mercury from all solutions of corrosive sublimate, that there is no good reason why metallic tin should be substituted for it. Dr. Frampton, in carrying out his experiments on this subject, has discovered a fact of some importance in a medico-legal view, namely, that when the poison is in an extreme state of dilution, the mercury still admits of separation by boiling the liquid for some time with metallic silver. (Medical Gazette, Oct. 1843.) Dilution, it is well known, materially affects the action of all liquid tests; but in this case, except that the operation goes on more slowly, it does not appear to interfere with the action of silver in separating mercury. Dr. Frampton obtained distinct globules of mercury, by the use of silver, from a mixture containing *one tenth of a grain* of corrosive sublimate in *twenty-five ounces* of river water. As the boiling was continued for some time, there was a loss from evaporation, so that twenty-three ounces of fluid only were drained off; but, at the lowest estimate, there was a sensibility to one part of poison in 115,000 parts of water. A ring of mercury was also obtained in a case where one sixteenth of a grain of corrosive sublimate was mixed with twenty-seven ounces of water, in which case twenty-three and a half ounces were drawn off, thus proving a sensibility to one in 180,000 parts, and probably this is not the extreme limit of its power.

These experiments clearly show that copious dilution does not impair the action of the silver test—a very important fact where the contents of the stomach are very large: but it is to be remarked, that in both of the above cases, the test was brought into contact with the whole quantity of the poison used; this being in one instance one tenth, and in the other one sixteenth of a grain. There is no doubt that the test will detect a much smaller quantity of the poison than was here employed, and that the 100th of a grain may be easily discovered by it. This degree of delicacy is sufficient for all practical purposes. It has been further objected to the test, that in some cases the poison may be reduced to the state of metallic mercury by certain organic principles. Such an objection applies, however, no more to the silver than to the other tests, such as copper or tin and its chloride. The mercury must here be brought into a soluble state, by the well-known usual processes, before any test whatever can be made to act upon it.

Conversion of calomel into corrosive sublimate by alkaline chlorides. A case lately occurred in France, in which a medical practitioner was charged with the death of a child by the administration of a common dose of calomel with muriate of ammonia. It was stated by M. Mialhe, who gave evidence on this occasion, that, by contact with any chloride, such as common salt, calomel was converted to corrosive sublimate; and such a mixture was therefore highly dangerous. Experiments subsequently performed, showed that if this change did take place at all at common temperatures, or at the temperature of the stomach (98°), it was only to a trifling extent, and not likely to endanger life by the usual mode of exhibiting the medicine. The question is of some importance to medical practitioners; for, although it is not customary to give calomel in mixtures with alkaline chlorides, yet common salt is largely employed as an article of food, and the chlorides of sodium and potassium exist in the animal secretions. It is therefore proper to state here the results of some recent experiments on the subject by M. Larveque, especially since these tend to show that the statements of M. Mialhe are not strictly borne out by observation. An account of these experiments will be found in the 'London and Edinburgh Phil. Mag.' Sept. 1843. The principal facts merely are here selected. In one experiment a mixture was made of 45 grains of calomel, 90 grains of chloride of sodium, and 1875 grains of distilled water. The mixture was frequently shaken, but it was only after the lapse of a week that the supernatant clear liquid was at all discoloured by sulphuretted hydrogen gas. This was not, however, owing to the presence of any corrosive sublimate, for none could be separated by sulphuric ether. The effect of the gas was doubtless due to the presence of a minute portion of calomel held dissolved by the alkaline chloride. When nearly double the quantity of common salt was used with half the quantity of water, still no evidence of the production of any corrosive sublimate could be obtained. The chlorides of potassium, barium, calcium, and magnesium gave precisely similar results. When the mixtures were heated to 212°, then a portion of corrosive sublimate, easily separable by ether, was uniformly produced. Muriate of ammonia was found, even at common temperatures, to convert a portion of calomel to corrosive sublimate. This, however, is only likely to occur where the quantities of calomel and muriate are infinitely larger than it is probable they would ever be prescribed for medicinal purposes. Practically speaking, this conversion by common salt can never give rise to any dangerous consequences; because it is not found to take place at common temperatures, nor at the temperature of the body. The change produced by muriate of ammonia at common temperatures is so slight as to be of no importance.

Lead.—In November, 1843, an interesting trial took place at the assizes of the Puy de Dôme, in France, involving the rare question whether or not the death of a person had been caused by the criminal administration of a salt of lead. The whole of the proceedings are reported with much unnecessary prolixity (extending to 158 pages) in the 'Annales d'Hygiène' for January, 1844. The deceased died under suspicious circumstances; on examination of the body, there was nothing found indicative of the action of poison, while the stomach was ulcerated and in an otherwise diseased state. No salt of lead was found in the contents, but traces of the metal were discovered on incinerating the viscera. A question then arose, whether this metal was a natural constituent of the body, or the result of a portion which had been swallowed and had acted as a poison. The medical opinions were much divided. Orfila thought that it was very probable, if not certain, that the deceased had died from the effects of lead. There was so much doubt about the case, that, in an English court of law, it would probably have been speedily dismissed for want of clear medical proof of the cause of death. The details are not of sufficient general interest to justify quotation, but the medico-legal reader will find, in the controversy between MM. Dupasquier, Danger and Flandin on the one side, and M. Orfila on the other, that the art of conducting a medical prosecution and a *medical* defence is well understood in France.

Carbonate of lead. Shot in bottles. A case is related in the 'Annales d'Hygiène,' (April, 1844,) which shows that serious accidents may sometimes happen from the shot used in cleaning bottles being left, and afterwards becoming chemically acted on by the wine or liquid introduced. The practice of thus cleaning bottles is very common in England and also in France, and the small pellets often become fixed in the narrow part of the base of the bottle, and thus escape notice.

A person after having swallowed a few glasses of liqueur, suffered from the most violent colicky pains, and all the symptoms of irritant poisoning. Dr. Hanle, who was immediately called, having observed that the liquor remaining in the bottle was very turbid, poured it off for analysis, when he found, firmly wedged in at the bottom of the bottle, ten leaden pellets, which had become so completely transformed to carbonate of lead, that there was only a small nucleus of the metal left. So long as the liquor was clear, no accident had arisen from its use; but the symptoms of poisoning appeared immediately when the turbid portion, at the bottom of the bottle, containing the salt of lead either suspended or dissolved, was swallowed.

It is singular that the lead should have been found in this case in the state of insoluble carbonate; for, in general, the vegetable acids contained in wine (if we except the tartaric) form soluble salts of the metal. With acescent wines, such as those made in this country, which owe their acidity chiefly to citric acid, accidents of this kind are very liable to occur; but with good Spanish wines this is not so common. The acidity here is chiefly due to tartaric acid; and it is only slowly that tartrate of lead is formed, even when the quantity of shot left in the bottle is large.

Action of water on lead. A very important communication has been made on this subject by Dr. Christison to the Royal Society of Edinburgh, which has been since published in their 'Transactions' (Vol. xv, Part 2). A few years ago, the Doctor was led to examine the water introduced into a dwelling by a lead-pipe, from a distance of three quarters of a mile. It was remarked that the water fresh drawn from the pipe *was perfectly transparent at first*, but on exposure to air, it quickly presented a white film, afterwards ascertained to be carbonate of lead.

This water had been previously examined by Dr. Christison, and he had concluded that it contained salts enough to prevent corrosion of the lead, from the circumstance that several pieces of the fresh-cut metal retained their lustre when immersed in it for a period of fourteen days! "I did not," he observes, "at the time, advert to the difference between an experiment, in which some ounces of water were left at rest on a few square inches of lead, and one in which a column of water, only three quarters of an inch in diameter, flowed constantly over a surface of nearly 800 square feet." On analysis, this water was found to contain but a very small portion of saline matter (the 21,400th part), and the salts were of such a nature as to have the least protective influence. The remedy adopted, was to leave the spring water at complete repose in the pipe for a period of four months, so as to allow the carbonate to crystallize slowly and firmly on its interior. "This experiment was attended with complete success. The water was then found to flow without any impregnation of lead, and has done so ever since."

In another case, the water gave rise to the effect of slow poisoning by lead; and here again it was observed,—a fact hitherto not noticed by toxicologists,—that it was quite transparent when first drawn, and only acquired an incrustation of carbonate of lead after being exposed to air for some time. In this instance, the water contained a large proportion of saline matter (the 4460th part); but on analysis this chiefly consisted of chlorides,—the salts which have the least effect in preventing the action of water on lead; while the really preventive salts, commonly contained in terrestrial waters (sulphates and carbonates), were present only in the minutest traces. Polished lead was found to be tarnished by it in a few hours. The remedy adopted in this case was to keep the pipes constantly full of a solution containing a 27,000th of phosphate of soda. After the lapse of about three months, it was found that the water contained no traces of lead.

This last case establishes, that it is impossible to determine merely from the quantity of saline matter contained in water, whether it is liable to become impregnated with lead or not. The salts may be of a nature to have little or no *protective influence* in the proportion in which they exist.

Dr. Christison has found by his experiments, that the compound produced under these circumstances is not, as it is commonly supposed to be, a pure crystalline carbonate of lead, but that it is formed of two equivalents of carbonate of lead with one equivalent of hydrated oxide of lead. This compound is permanent in the air, and is only converted to neutral carbonate by suspending it in water and treating it with a stream of carbonic acid gas. It would appear from the analysis of Mulder, confirmed by that of Christison, that even the whitelead of commerce is not a pure carbonate, but a compound of four equivalents of carbonate to one equivalent of hydrate. The most simple method of preventing water from acquiring a poisonous impregnation by lead, available under all circumstances, is that of allowing it to remain for some months before use in the pipe or cistern. This gives time for a firm crystalline deposit of carbonate to attach itself to the surface of the metal, whereby all further action is prevented. It is not uncommon to find this deposit regarded by ignorant persons as the result of the hardness of the water;—it is scraped off and a fresh surface of metal exposed, so that in a case of this kind, water from a particular cistern may have been for a long time used with impunity,—and yet suddenly give rise to symptoms of lead-poisoning, probably to the surprise of the medical attendant and the parties affected.

The conclusions of Dr. Christison are of such importance in a medical view, that we here subjoin them. 1. Lead pipes ought not to be used for the purpose of conveying water, at least where the distance is considerable, without a careful chemical examination of the water to be transmitted. 2. The risk of a dangerous impregnation of lead is greatest in the instance of the *purest* waters. 3. Water which tarnishes polished lead when left at rest upon it in a glass vessel for a few hours, cannot be safely transmitted through lead-pipes without certain precautions. Conversely, it is probable, though not yet proved, that if polished lead remain untarnished, or nearly so, for twenty-four hours in a glass of water, the water may be safely conducted through lead-pipes. 4. Water which contains less than about an

8000th of salts in solution, cannot be safely conducted in lead-pipes, without certain precautions. 5. Even this proportion will prove insufficient to prevent corrosion, unless a considerable part of the saline matter consist of carbonates and sulphates, especially the former. 6. So large a proportion as a 4000th, probably even a considerably larger proportion, will be insufficient, if the salts in solution be in a great measure muriates. 7. It is right to add, that in all cases, even though the composition of the water seems to bring it within the conditions of safety now stated, a chemical examination should be made of it after it has been running for a few days through the pipes. For it is not improbable that other circumstances, besides those hitherto ascertained, may regulate the preventive influence of the neutral salts. (It may be here suggested whether organic matter in water, which has a strong tendency to combine with oxide of lead, may not have some influence.) 8. When the water is judged to be of a kind which is likely to attack lead pipes, or when it actually flows through them, impregnated with lead, a remedy may be found either in leaving the pipes full of the water and at rest for three or four months, or by substituting temporarily for the water a weak solution of phosphate of soda, in the proportion of about a 25,000th part.

I have found that sulphate of lime, when it forms about the proportion of a 5000th part, also acts as a good preservative. This salt, as plaster of Paris, is easily procurable in most localities.

A few years since, Dr. Clark of Aberdeen, suggested a very ingenious chemical process for depriving common water of two thirds of its solid saline matter, thus rendering it more soft and better adapted for many purposes to which it cannot now be applied. Two thirds of the saline matter, contained in the Thames river-water, consists of carbonate of lime held dissolved by carbonic acid; the remaining third is chiefly composed of sulphate of lime. Dr. Clark proposed to add caustic lime to water, in order to remove the free carbonic acid, and thereby to precipitate it, and the carbonate of lime held dissolved by it, entirely. The experiment perfectly succeeds when a few ounces of lime-water are added to a gallon of river-water;—any surplus lime is got rid of by exposure to air. When the precipitation has occurred, (occasionally not for some hours, or even a day,) the clear water may be poured off, and it will be found much softer. Having produced a quantity of this water, I was desirous of seeing how far, by the loss of so much saline matter, it would now be affected by contact with lead. Many experiments were performed on a small scale, and it was found that the preservative properties of the water were not at all diminished by the separation of the carbonate of lime. This seems to show that the sulphate of lime (the residuary salt) is mainly concerned in counteracting this chemical action between water and lead.

Mr. Scanlan has lately found that recently distilled water, condensed in a leaden pipe, holds dissolved a quantity of carbonate of lead, being turned brown by sulphuretted hydrogen. This is important in analysis, whether chemical or medico-legal. (Pharm. Journal, August, 1844.)

The most convenient plan, in Dr. Christison's opinion, for detecting lead in water, a duty which may occasionally fall on a general practitioner, is,—1. To examine what separates on exposure to the air, by dissolving it in warm acetic acid, and testing the solution with sulphuretted hydrogen, iodide of potassium, and bichromate of potash. 2. If this process fail,—To concentrate the water to an eighth part, and again test any insoluble matter which separates; and lastly, failing this procedure also, to evaporate the water to dryness, subject the residue along with charcoal to a red heat, act on what remains with warm diluted nitric acid, and test the solution when filtered and neutralized by an alkali. It may admit of question, whether in the event of lead being indicated in the last way only, the very minute quantity which would then be present, can prove detrimental.

We have assigned this space to one of the most valuable monographs on lead-poisoning which has appeared within the last few years. It is a subject of daily interest to the medical practitioner; and there are few cases in which chemistry has been brought to bear on medical police with more satisfactory results than in this. The cause of the mischief and the remedy are clearly pointed out. There is no doubt, that the dangerous effects produced by the contact of water with lead was well known to the Romans,—that this was the real cause of their abandoning its use, and, in the absence of iron,—resorting to those expensive structures of masonry which are now seen stretching in gigantic piles, and to enormous distances, over the Campagna di Roma. One of their leaden pipes has been found, and is now preserved in the Museum of Arles, with the name of the Roman plumber at every juncture. It is much to be regretted that there is even in our own day, among architects and builders, a degree of ignorance on this subject as great as that which existed two thousand years ago, and that notwithstanding the discoveries of modern science, serious accidents are frequently occurring in families from the want of the most simple precautions in the use of this metal.

Colica pictonum. In a paper read before the Academy of Sciences in November 1843, M. Ruolz proposed that the use of white-lead as a pigment should be abandoned, and that the white oxide of antimony should be substituted for it. Subsequently M. Rousseau suggested a process for the economical manufacture of the white oxide of antimony. It is not improbable that such a change might be attended with the benefit proposed—of extirpating colica pictonum and those disorders which affect individuals employed in white-lead works; but it is questionable how far the finely-divided oxide of antimony could be received into the system with impunity. Further, in respect to the arts, no substance has yet been found which has possessed, as a pigment, the degree of opacity which is known to belong to the carbonate of lead.

Copper. In July, 1843, a communication was made to the Academy of Sciences by MM. Danger and Flandin, on a new process for detecting copper, by the incineration of organic matter in cases of poisoning. By this means they have been enabled to detect the metal when it formed only the 100,000th part of the organic compound. (Annales d'Hygiène, t. xxx, p. 449.) The organic matter is simply dried and carbonized by heating it in a porcelain capsule, with one third of its weight of strong sulphuric acid. After being heated to dull redness, the charcoal is reduced to powder, drenched with sulphuric acid, again heated, but not to dryness, and then digested in water. By this means a clear solution is obtained, in which sulphate of copper is easily detected by the usual tests. The same process is applicable, according to the authors, to the detection of the compounds of lead, silver, bismuth, tin and gold, with the exception that, in the case of lead, muriatic, and of gold and tin, nitro-muriatic should be substituted for sulphuric acid, in acting upon the incinerated ashes. By direct analysis and physiological experiments, the authors have arrived at the conclusion, that neither lead nor copper forms any constituent part of the healthy animal body. In animals to which they had exhibited a salt of copper in divided doses, they found that the metal was not excreted with the urine, as in the case of arsenic and antimony; but with the bronchial secretion, in which they detected it by analysis. Mercury appears to take the same course in its exit from the body, since it has been found in the saliva of persons who have been taking mercurial pills. The compounds of silver and gold were observed to pass off both by the pulmonary and urinary excretions; but while the chloride of gold passed more readily by the kidneys than the lungs, it was exactly the reverse with the chloride of silver. After death from poisoning by a salt of copper, that portion of the metal which has been absorbed can be detected only in the intestines and in the liver; and about two ounces of this last-mentioned viscus were found to be sufficient for obtaining satisfactory medico-legal proof of the presence of the poison.

The question whether copper and lead constitute part of the healthy animal organs, in cases where neither of these substances can have been taken in a poisonous form, is a point which does not appear to be decidedly cleared up; although the balance of opinion is in favour of the results obtained by MM. Danger and Flandin, namely, that they neither enter into the composition of the body nor of the food of man; and that where they are said to have been detected, their presence must be ascribed to their adventitious introduction during the analysis. The question is of some interest in toxicology, for it has been already raised and thrown out as an objection to medical evidence in a late trial, already adverted to. (Ante, p. 538.)

In August, 1843, M. Barse communicated to the Academy the results of some analyses made on the bodies of two subjects taken from the hospitals of Paris. They had died from ordinary disease. M. Barse states that he detected copper and lead in both subjects. The copper was obtained in the metallic state and identified by all its characters; the lead was not obtained as a metal, but its presence was indicated by the usual tests. These metals may be detected in the liver, according to M. Barse, 1, by Orfila's process of carbonization; 2, by simple carbonization, incineration of the ash, and afterwards digesting it in nitro-muriatic acid; 3, in carbonizing by sulphuric acid and *incinerating the charcoal,* for the mere carbonizing action of sulphuric acid will not of itself suffice to allow of the detection of these metals.

In September, 1843, M. Rossignon of Lyons addressed a note to the Academy of Sciences on copper, as it exists in the organic tissues of many vegetables and animals. M. Rossignon states that he detected copper in all his experiments on the human body: he found it in the blood and muscular fibre of man, in the tissues of many domestic animals (the dog), and in the common vegetables used as food. The gelatin used as soup at the hospital of St. Louis yielded per cent. 0·03 of pure copper. Common sorrel gave 2 per cent. of neutral oxalate of copper; chocolate from 0·07 to 0·5 per cent. The bread generally used in Paris gave, in 1000 parts of incinerated residue, from 0·05 to 0·08 of copper (fraudulently introduced as sulphate?) Coffee, chicory, madder, and sugar yielded traces of the metal, in the latter case mixed with lead. Barley-sugar contains copper; and in the sugar of starch it forms 4 per cent. by weight, of the carbonized residue! M. Rossignon further states, that by calcining the substances in close vessels, he was enabled to detect appreciable traces of the metal in human semen, in the excrement of the fowl, in the egg, and in the eye of the ox!

It appears difficult to reconcile these results with those obtained by MM. Danger and Flandin and others. It is intimated that the failure of these experimentalists in detecting copper was owing to their not having incinerated the carbon derived from the action of sulphuric acid on organic matter; but this does not sufficiently account for the difference, since, by pursuing the same process with the pulmonary exhalations of animals poisoned with its salts, they detected the metal readily, although here it was only found in traces. Besides, when we consider the very positive manner in which it was for a long time stated that arsenic was a normal constituent of the human body, by a higher authority than either M. Barse or M. Rossignon, and that this statement has been since entirely disproved before a committee of the Academy, we may well hesitate to assent to the assertion that copper is a natural constituent of the body. These chemical mistakes are very liable to occur in researches conducted by individuals; and it is often only by the presence of several engaged in watching the process, that the real source of fallacy is detected. *Quod volumus facilè credimus.*

Verdigris. One case of poisoning by this substance is related in the 'Journal des Conn. Med. Chir.,' December, 1843. It is reported in the 'Edinburgh Medical and Surgical Journal' for July, 1844 A woman, ætat. 28, swallowed a large dose of verdigris. She was soon afterwards seized with great anxiety, vomiting, acute pains, and swelling of the abdomen, sensation of burning heat in the throat, coldness, and severe cramp in the extremities, a labouring pulse, swelling of the face, with the eyes sparkling. An emetic brought away some half-digested food, without any traces of poison. The next morning there was painful deglutition, swelling of the throat, the abdomen tympanitic and painful on the least pressure, the countenance heavy, the face flushed, and the pulse oppressed. About two pounds of a distinctly-greenish fluid, with some blood were ejected. The symptoms became aggravated; the face and eyelids swollen and red, the eyes prominent, the abdomen flattened but sensible, the rectum so irritable and painful that enemata could not be administered. On the second day there was a tendency to coma, the face was pale, the lips swollen, the gums ulcerated, and there was an abundant discharge of viscid saliva. A copious stool was passed—the first since the poisoning; and acetate of copper was detected in it in pretty large quantity. There were several spasmodic fits. On the third day some viscid glairy matter, of a greenish colour and tinged with blood, was vomited, and the spasms continued. On the fourth day epistaxis with general cramps came on, and the urine and fæces were suppressed. There was coldness of the surface, with convulsions. After the lapse of about a week she still had vomitings of greenish glairy matters, with uneasiness in the abdomen; but from this date she gradually recovered.

This case is interesting from the course of the symptoms being accurately noted; and it is worthy of remark, that icterus, which some have regarded as a symptom of cupreous poisoning, was at no time present. It is unfortunate that the quantity swallowed was not known.

Subchloride of copper. Among the very few cases of poisoning reported in Henke's Zeitschrift der S. A. for 1843-4, is the following. (No. 1, 1844.) A boy between two and three years of age swallowed a part of a small cake of green water-colour, such as is sold in the colour-boxes for children. Very soon afterwards he was attacked with vomiting and coldness of the extremities. Notwithstanding the exhibition of an antimonial emetic, the symptoms continued to become aggravated, and the child died. On opening the body, there was nothing to indicate specially the action of an irritant poison, except a slight congestion in the cerebral vessels. The child, it appears, had swallowed about a scruple of the green colour, which, on analysis, was proved to be the common subchloride of copper. It was remarkable that there was not the least sign of irritation or inflammation in the alimentary canal. Death was ascribed by the examiners to the exhaustion resulting from violent vomiting, and the congestion of blood in the brain thereby produced.

This case, the details of which are rather imperfectly given, shows that the subchloride of copper is a very active poison, and that it may cause death without leaving any signs of irritation in the alimentary canal. It is to be remembered that it is this compound of copper which is often formed in culinary utensils, and which thereby gives rise to accidents when any food containing salt has been prepared in the vessel without proper precautions.

Arsenite of copper. Scheele's green. The dangerous practice of using this powerful poison to give a green colour to confectionary is very prevalent, and accidents are continually arising from this cause. An instance has just been communicated to me, of recent occurrence, in which three lives have nearly been sacrificed, at a school near Manchester, owing to the parties having eaten some ornamented confectionary, which owed its green colour to arsenite of copper. They suffered from violent vomiting, severe pains in the stomach and bowels, and spasms in the extremities. Three animals which ate of the vomited matters were attacked by similar symptoms. It is unfortunate that in this country there is no medical police established by law to restrict the free sale and use of this and other poisons. In this respect the English is widely distinguished from the continental practice. In France and Germany the lives of individuals are closely protected against those accidents which are liable to occur through the ignorance or criminal neglect of others. Here poison is allowed to be sold like sugar or starch; and every child is assumed by the law to be capable of protecting himself! If death ensue from such a cause, we find that a coroner's inquisition and a trial for manslaughter take place, to investigate an event which, under simple medical-police regulations, would not have occurred. More than a hundred lives are yearly sacrificed in England and Wales by the unrestricted manner in which arsenic is sold. The sale of alcohol is rigorously confined by fiscal regulations; and it is impossible to say why some strong restrictions should not be placed on the sale of poisons that can seldom be required by the public for any innocent or lawful purpose. If the sale were prohibited by a penalty, except under the order of a regular medical practitioner, it is quite certain that many lives would yearly be saved, and the painful proceedings connected with these criminal trials would be spared to the country. To those who are inclined to adopt the "*argumentum ad crumenam*" it may be observed, that the law-charges incurred for such inquisitions and trials, would more than pay for the establishment of a national board of medical police.

Arsenite of copper is not the only poison which gives a green colour to confectionary. Chromate of lead, mixed with indigo has also been employed for the purpose. It has been said that there is danger in the use of these compounds for tinting paper, should the paper be subsequently used for wrapping up articles of food. The reader will find in the Annales d'Hygiène (1843, p. 358,) a full account of the composition of these mineral colours, with the police regulations adopted in France on the subject. The green and bright blue papers are condemned as the most dangerous; but, unless the colour be only roughly adherent to the surface, it is not probable that an article of food inclosed in the paper would acquire a poisonous impregnation.

Electrotyped copper utensils. Mr. Warington has lately shown that copper vessels, saucepans, taps, and other articles, which have been covered with a surface of silver by the electrotype process, are liable to be acted on by weak acids, such as lemon-juice and vinegar, when such acids are allowed to remain in contact with them for a short time. It appears that the metallic silver with which they are covered is porous, like a sponge—a fact made evident on slight examination—and in this way the acid liquid permeates the silver, and reaches the surface of the copper. A kind of galvanic circuit is thereby established, which increases the chemical action; so that such vessels, while giving apparent security in their use, are actually rendered more dangerous. The presence of copper in acid liquids kept in the electrotyped vessels, was clearly proved by the usual tests for that metal. The same effects might occur where the liquid contained common salt dissolved.

Antimony. It would appear from the observations of the late Mr. Goodlad (Provincial Journal) of Manchester, and Mr. Noble, that tartarized antimony, even in small doses, is liable to act as a poison on the young. Mr. Wilton records four cases in which prostration and collapse followed the administration of ordinary doses of tartar-emetic to young children. Two of them were fatal. It should therefore be administered with great caution. A case, showing the importance of this remark in a medico-legal view, will be found in the Medical Gazette, vol. xvi, p. 520.

Bichromate of potash. Well-observed cases of poisoning by this compound, which is now extensively used in the arts, are rare; and, therefore, the details of the following case, communicated to the 'Medical Gazette,' (vol. xxxiii, p. 734,) by Mr. Wilson of Leeds, are of great practical interest. A man, ætat. 64, was found dead in his bed, twelve hours after he had gone to rest. He had been heard to snore loudly during the night, but this had occasioned no alarm to his relatives. When discovered he was lying on his left side, his lower extremities being a little drawn up to his body: his countenance was pale, placid, and composed; eyes and mouth closed, pupils dilated, no discharge from any of the outlets of the body, no marks of vomiting or diarrhœa, nor any stain upon his hands or person, or upon the bed-linen or furniture. The surface was moderately warm. Some dye-stuff, in the form of a black powder, was found in his pocket. On inspection, the brain and its membranes were healthy and natural; there was neither congestion nor effusion in any part. The thoracic viscera were equally healthy, as well as those of the abdomen, with the exception of the liver, which contained several hydatids. A pint of a turbid inky-looking fluid was found in the stomach. The mucous membrane was red and very vascular, particularly at the union of the cardiac extremity with the œsophagus; this was ascribed to the known intemperate habits of the deceased. In the absence of any obvious cause for death, poison was suspected, and on analysing the contents of the stomach, they were found to contain bichromate of potash; and the dye powder taken from the man's pocket consisted of that salt mixed with cream of tartar and sand.

It is remarkable that in this case there was neither vomiting nor purging. The salt does not appear to have operated so much by its irritant properties, as by its indirect effect on the nervous system. This, however, is by no means an unusual occurrence, even with irritants, far more powerful than the bichromate of potash.

Antidotal treatment in cases of poisoning. This subject has been lately examined by M. Bouchardat, and the conclusions at which he has arrived for the different irritant poisons are as follows: For ***corrosive sublimate,*** he recommends a mixture of zinc with iron filings, or iron reduced from the oxide by hydrogen, the moist per-(proto-?) sulphuret of iron. The same may be employed in cases of poisoning by copper and lead. In arsenical poisoning, the moist hydrated peroxide of iron, and the moist per-sulphuret may be used. This last preparation, it is alleged, decomposes the compounds of all of these poisons, and should, therefore, be exhibited when the nature of the irritant is unknown. The heavy metallic powders may be given in an electuary; the fauces should be tickled to promote vomiting. It is said to have been found that 100 parts of the powder of iron or zinc was a complete counteragent to fifteen parts of the acetate of copper; but it required a much larger quantity of the moist magma of the per-sulphuret of iron, to produce the same effect. (See Ed. Med. and Surg. Journal, April, 1844.)

An ingenious paper has lately been communicated by Sir George Lefevre to the 'Lancet,' (June, 1844,) in which will be found a summary of the common antidotes employed in cases of poisoning, with some excellent remarks on medical police, in reference to the sale and use of poisons. In a case of poisoning by ***oxalic acid,*** where the usual antidote is not at hand, Sir George recommends the administration of old mortar, finely powdered, or even the plaster scraped from a wall. In respect to other poisons, the only novelty of treatment which appears is in reference to ***prussic acid,*** where he states, that "a solution of sulphate of iron is found to be the best antidote," and even "***ink***" may be administered if the salt of iron should not be at hand.

There must surely be some mistake in reference to the use of sulphate of iron under such circumstances. The sulphate of iron cannot possibly act as a chemical antidote, for in solution it mixes with prussic acid in all proportions, without any chemical change whatever, a fact easily demonstrated by experiment. So, again, ink could have no more effect on the body than so much water, unless, as it sometimes happens, it should contain corrosive sublimate, (put into it for the purpose of preventing mould,) when the effect would be the reverse of favorable. Indeed it is difficult to conceive on what principle, except on the wrong supposition that Prussian blue might be formed by the admixture, such substances could be recommended as antidotes in cases of poisoning by prussic acid.

New antidote to prussic acid. Although neither sulphate of iron nor ink can be considered to possess the least antidotal power in cases of poisoning by prussic acid, yet there is one form in which iron may be used with some hope of success. The production of Prussian blue by an admixture of the protoxide and peroxide of iron, precipitated from the green sulphate by caustic potash, has been long known as an admirable test for prussic acid. The application of these mixed oxides, as an antidote in poisoning by prussic acid, has been lately proposed by Messrs. T. and H. Smith of Edinburgh; and it may be said of this antidote that, whenever it can be administered sufficiently early, there is a very reasonable prospect of its success; for, by mere contact, it speedily converts this formidable poison into the insoluble inert compound, Prussian blue. These gentlemen have not, so far as I know, yet published any detailed account of their experiments; but it is stated, that when the acid with the antidote was given to dogs, the animals lived, while the same dose of acid without the antidote, proved fatal. The method by which Messrs. Smith prepare the antidote is not announced; but I have found that it may be prepared by precipitating, in a closed vessel, a strong solution of the common green sulphate of iron by caustic potash, and washing the oxide repeatedly with water recently boiled, until the alkali and the surplus sulphate are removed.

The following experiment was performed, in order to test the efficacy of this antidote in a chemical view. Four drachms of prussic acid, containing 1 per cent. of anhydrous acid—a dose sufficient to destroy an adult in a very short period of time—were agitated with the mixed oxides of iron, precipitated in the manner described, from a solution of one ounce of crystallized green sulphate of iron. That

Prussian blue was almost instantly formed was evident on testing the mixture. It was allowed to remain an hour digesting; and then distilled at a very gentle heat. The clear liquid obtained by this distillation, was tested both by nitrate of silver and sulphate of iron. These tests had given abundant evidence of prussic acid in a few drops of the poisonous liquid, before the oxides of iron had been added; but now, not a trace of the poison could be discovered, neither by the odour nor by the action of nitrate of silver, which will detect the 1500th part of a grain of anhydrous acid; and for this experiment, as much as one fourth of the distilled liquid was used. No Prussian blue could be obtained, by the use of the sulphate of iron in the usual way, in operating upon another fourth of the distilled liquid; although this test is adequate to the detection of the fiftieth part of a grain of anhydrous acid, in a large quantity of water. It is therefore evident that the views of the Messrs. Smith are strongly confirmed by the results of these experiments; and now two questions will probably arise in the mind of the reader, respecting this antidote: 1. Whether, considering the great rapidity with which this powerful poison operates, any antidote whatever can be of the least service? 2. Admitting that the antidote is administered, is the conversion of the poison to the state of Prussian blue, so complete and so rapid as to counteract its effects?

With regard to the *first* question, it may be remarked, that if in only one case in a hundred, there is sufficient time for its administration, a life may be saved; and much credit is therefore due to the Messrs Smith for the ingenious suggestion of such a remedy. It is true that the symptoms from a powerful dose of prussic acid come on commonly in about ten or twelve seconds; but so long as life continues, there may be a hope of recovery by neutralizing the effects of the poison; and the recent case of Mrs. Belany, who lived twenty minutes, and respecting the death of whom a trial has recently taken place in this metropolis, shows that instances do occur in which there may be time for antidotal treatment. With respect to the *second* question, the answer to this must depend in a great measure on the results of experiments. No inference could be fairly drawn from the administration to an animal of the oxides of iron already mixed with prussic acid; although this was the way in which the alleged iron-antidote, in cases of arsenical poisoning, was, in the first instance, most improperly tested. Such an experiment may prove, it is true, the inertness of the compound formed; but it is quite unfitted to illustrate the antidotal powers of the substance used; for a human being is never likely to swallow, as this experiment assumes, the poison and antidote together. In the case of prussic acid, more than in that of any other poison, it is necessary that the alleged antidote should produce its effects when administered a *few seconds or minutes after the acid has been taken.* On adding a small quantity of the mixed oxides to about one drachm of prussic acid, agitating and rapidly filtering, I found the poison still existing in pretty large quantity in the filtered liquid. I have reason to believe, however, that the chemical change would have been expedited by the use of a larger quantity of the mixed oxides; and as they are inert, there is no objection to their being given in very large doses. At any rate, it must, I think, be admitted, that, both in a chemical and pathological point of view, no remedy for prussic acid has hitherto been proposed, with a greater prospect of success than this possesses If it should fail, its failure cannot be ascribed so much to any defect in its chemical operation as to the extraordinary rapidity with which the poison acts upon the system. The mixed oxides of iron may be prepared from the sulphate, and kept in a closely-stoppered bottle, filled with water previously deprived of air by boiling. Protoxide of iron may be speedily obtained in large quantity by agitating iron filings with a solution of sulphurous acid for a few minutes; filtering, adding caustic potash, and washing the precipitate with well-boiled water. It is important to state, that *peroxide of iron,* (the substance used in cases of arsenical poisoning,) cannot, in this case, be substituted for the mixed oxides; although it may serve to mix with the protoxide prepared in the way just described. The peroxide of iron alone will not form Prussian blue under any circumstances with prussic acid. In poisoning by the essential oil of bitter almonds, or by cyanide of potassium, the mixed oxides of iron are equally applicable; and in one case related in this Report, of poisoning by the oil of bitter almonds, the antidote might, had it been then known, have been used, and have accelerated recovery. It is possible, indeed, that it may become of more frequent use as an antidote to the oil, than to prussic acid. With respect to cyanide of potassium, we may at once administer a solution of green sulphate of iron to the patient, as the oxides are immediately precipitated by contact with the salt, and Prussian blue is formed. In the short account which I have seen, the Messrs. Smith appear to have confined their observations to the effects of the oxide on prussic acid.*

Animal irritants. In a late number of the 'Edinburgh Medical and Surgical Journal,' (July, 1844,) it is observed, in reference to the poisonous properties of the *flesh of diseased animals* used as food, that "in America there are certain regions, extending for many miles in length and some miles in breadth, on the herbage of which, if an animal feeds, its milk and flesh acquire poisonous properties, yet itself enjoys tolerable health. The disease which the use of the flesh or milk of the animals fed on these districts produces, is known over all America by the name of the milk-sickness, or 'trembles.' All the infected spots occur west of the Alleghanies; and it is well known, that of the early emigrants whole communities, on account of the prevalence of this malady in a particular locality, which is generally distinctly circumscribed, were often compelled to seek another; and even at this day, those who venture within the boundaries of an infected district, are constrained, as a condition of their residence, to abstain from the flesh of the cattle living within the same limits, as well as from the milk and its preparations. It appears from the late report of Drs. Hosack, Post, and Chilton on this subject, that in some of these infected districts, the inhabitants, with a recklessness of human life which seems incredible, carry the butter and cheese which they themselves dare not eat to the markets of the towns west of the Alleghanies, and that thus there are frequently produced symptoms of poisoning and even death, for which the medical attendant cannot account, or he is induced to consider as some new or anomalous form of disease. From the same report we learn that the cattle from these districts are sent in great droves over the mountains, but, in order to deceive the buyers as to the place whence they come, they bring them to New York by a southern route, and style them 'southern cattle.' The flesh of these animals produces, in those who make use of it, symptoms of aggravated cholera morbus. The viscera of the animals are often found diseased, and the livers almost invariably so."

Owing to the symptoms of poisoning which have followed the use of such beef, butter, and cheese, the American government caused a medical inquiry to be instituted into the matter; and it is probable that they will adopt the recommendation of the reporters, i.e. prohibit its sale. In the event of this occurring, it has been suggested as not improbable, that this poisoned food may find its way into England, and, from its cheapness, be diffused among the poor. It would therefore be advisable, that practitioners should be on their guard, and note any suspicious circumstances that may arise. As we are without a system of medical police in this country, it is not likely that government will have it in its power to prohibit the sale of such food, until many cases of the serious effects produced by it, have occurred.

There is another more common article of food, namely, *bread*, upon which some observations have been lately made by toxicologists. In the 'Annales d'Hygiène,' 1843, pp. 35 and 347, will be found communications on this subject from MM. Guérard, Chevallier, and Gaultier de Claubry. The changes which take place in the decomposition of flour and bread, and the production of various kinds of *mouldiness*, are here investigated, together with the effects of such bread upon the animal system. It would appear that in some parts of France the peasantry manifest no repugnance to the eating of *mouldy bread*; and that in many instances the practice appears to be attended with no ill effects. The nature of the mould produced, however, is subject to great variation, and it is not improbable, as M.

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* It will of course be understood, that cold affusion with stimulants should be at the same time resorted to; and if the power of swallowing is lost, the antidote may be introduced by a stomach-pump.

Chevallier suggests, that in some cases a poisonous principle is actually developed. In two instances of children, who had partaken of mouldy rye-bread, symptoms resembling those of irritant poisoning, supervened. The countenance was red and swollen, the tongue dry, the pulse quick, there were violent colics, with pain in the head and intense thirst. Vomiting and purging supervened with a state of collapse, but the children eventually recovered. These symptoms were ascribed to the production of "*mucor mucedo*" in the bread. In 1829, alarming effects having followed from the use of a certain kind of bread in Paris, M. Burruel was called upon to determine whether or not any irritant poison had become accidentally intermixed with it. The bread was simply in a mouldy state; there was no trace of poison. It is unnecessary to enter further into this subject; the facts adduced, together with experiments performed on animals, show that bread, in a state of mouldiness, may not only produce symptoms of poisoning, but actually cause death; and as it is impossible to distinguish the noxious from the innoxious kind of mould, the use of all bread in such a condition should be avoided.

Even fresh bread may occasionally seriously affect the body. The brown bread of London has been known to produce vertigo, lethargy, and other unpleasant symptoms, indicative of an affection of the brain and nervous system. This has been ascribed, with some probability, to the "*lolium temulentum*" becoming accidentally mixed with the corn. Rye-bread is not much used in this country, but the presence of the ergot might here, in some cases, account for the symptoms which have been observed.

Sulphate of potash. The question whether this is to be regarded as a poisonous salt, of an irritant nature, has been much debated within the last year among members of the profession, owing to a case which was tried at the Central Criminal Court in October, 1843. (The Queen v. Haynes.) The prisoner had given to the deceased, the night before her death, two ounces of sulphate of potash, dissolved in water; and it was alleged that she had, a fortnight previously to this, taken, in divided doses, as much as a quarter of a pound of the salt. The woman supposed herself to be pregnant, which was disproved by an examination of the body; and it was charged that the prisoner had given her the salt with the intention of causing a miscarriage. After the last dose, she was seized with sickness, and died within a very short time. The stomach was found empty, but highly inflamed, and there was blood effused on the brain. One medical witness referred death to the action of this salt as an irritant poison; the other to apoplexy, as an indirect result of the violent vomiting caused by the salt. The prisoner was acquitted of the charge of murder, but subsequently found guilty of administering the drug with intent to procure abortion. Both of the witnesses admitted that, in small doses the salt was innocent; but that in the dose of two ounces it would produce dangerous effects. A portion of the sulphate in this case was examined by Mr. Brande, as it was suspected that some poisonous substance might have become accidentally mixed with it; but it was found to be pure.

It is not improbable, from the symptoms and the inflamed state of the stomach, that the salt acted here as an irritant poison; and the fact of its being an innocent medicine in small doses appears to be no sound objection to this view; for the same circumstance is observed with respect to many substances, the poisonous properties of which cannot admit of dispute. Some have ascribed the irritant properties of this and other saline medicines—such as cream of tartar, in large doses—to their insolubility, and to the fine spicula of the powdered salt acting mechanically upon the mucous membrane of the stomach. This explanation does not appear sufficient: 1st, because some of these saline medicines, when taken dissolved—such as alum and nitre—have had a similar action; and, 2d, the effects are very different, and far more rapidly fatal than in those cases where mechanical irritants—such as fine sand or iron filings—have been taken. In short, there is no doubt that if the same quantity of the salt were taken perfectly dissolved in water, it would have an equally irritant effect; and sulphate of potash has been known to act in this way, when taken in divided and therefore very soluble doses A case in which it thus proved fatal in *two hours*, is reported in the 'Annale d'Hygiène,' April, 1842. According to Mr. Mowbray (Medical Gazette, v. 33, p. 54), sulphate of potash is a salt much employed in France as a popular abortive. He quotes several instances in which, in large doses, it produced severe symptoms, resembling those of irritant poisoning, and even death. In one case, two drachms acted powerfully; and in another, that fell under his own observation, four drachms of the salt, administered to a lady after her confinement, had all the effects of an irritant poison. The case of Haynes is the first instance in which, I believe, it is publicly known to have proved fatal in England; and it shows that substances, commonly regarded as innocent, may give rise to important questions in toxicology.

NARCOTIC POISONS.

Opium. It has been frequently observed, in cases of poisoning by this drug, that the individual has recovered from the first symptoms, and has then had a relapse and died. There is some medico-legal interest connected with this state, which has been called secondary asphyxia from opium, although there appears to be no good reason for giving to it this name. In December 1843, a gentleman swallowed a quantity of laudanum, and was found labouring under the usual symptoms. The greater part of the poison was removed from the stomach by the pump; and he so far recovered from his insensibility, as to be able to enter into conversation with the surgeon; but a relapse took place, and he died the following night. It is not improbable that, in these cases, death may be occasioned by a portion of the poison which has been carried by the absorbents into the system.

Recovery from a large dose without vomiting. A case occurred at the Westminster Hospital, in December 1843, (Lancet, Dec. 1843,) in which a woman, ætat. 25, was brought into that Institution while labouring under the symptoms of poisoning by opium. She was perfectly comatose, the features devoid of expression, the lips purple, and the pupils contracted to the size of a pin's head. The eyes were everted and fixed. Sulphate of zinc and tartar emetic were given without effect, and the stomach-pump was not brought into use until about an hour after her admission. The contents of the stomach were entirely free from the smell of opium. The woman was kept roused, coffee was administered, and she recovered. It appears she had swallowed *one ounce of laudanum*, but at what time before her admission is not stated.

It is difficult to say on what the recovery of this woman depended, for a very long time had elapsed before the contents were removed from the stomach, and then there was no trace of opium to be perceived by the smell. A better plan for determining the presence of opium in the discharged liquid is to dilute it sufficiently, and observe whether it acquires a red colour with the sesquichloride of iron. This change is always produced where opium is present even in very small proportion, owing to the meconic acid which it contains. The test will act where no odour is perceptible, either from the quantity of the drug being too small, or its being concealed by other odours. It is certainly remarkable that this woman recovered, considering the largeness of the dose and the time which had elapsed before the stomach was evacuated.

Dover's powder. The following case of poisoning by Dover's powder has been reported by Mr. Griffiths, (Medical Gazette, March, 1844.) About ten grains of Dover's powder were given by mistake to an infant seven weeks old, and it died in twenty-four hours afterwards. The following is an account of the post-mortem appearances. The countenance was placid, and the fingers of both hands were firmly contracted. On opening the abdomen, the colon was seen to be distended with flatus; the spleen, kidneys, and intestines were healthy; the liver gorged with blood; the stomach contained a very small quantity of colourless viscid matter. The inner coat was vascular; and at the great curvature as well as in other parts were small patches of highly injected vessels. The lungs were gorged with blood; the upper lobes being infiltrated with a greenish serum. The pericardium was vascular, and contained about a drachm of fluid. The right auricle was empty; the left ventricle contained some thin fluid blood, and a small coagulum. The sinuses of the dura mater were filled with dark coagula; the surface of the brain appeared covered by a complete network of vessels, distended with light coloured blood. On the surface of each posterior lobe of the cerebrum, slight extravasation had taken place. The brain was soft, and the difference of colour between the gray and white matter barely discernible. The vessels in the substance of the brain were gorged with blood, presenting, on section, a thickly-studded appearance—the spots of a deep dull red, and in many places coalescing. There was a small quantity of fluid in each lateral ventricle, and on the floor of each, were large distended blood-vessels. There was serous effusion on the surface and at the base of the brain, to the amount of about half an ounce. The contents of the stomach were carefully analysed, but neither morphia nor meconic acid could be detected.

This case is interesting in several particulars. In the first place, it is surprising that so young an infant should have lived so long after taking a dose equivalent to one grain of opium. Making every allowance for the great vascularity of the brain in young subjects, it appears from the inspection, that the opium had here affected that organ, and caused a general congestion as well as effusion and slight extravasation, which last condition is somewhat rare in poisoning by opium. The non-detection of the poison in the contents of the stomach was sufficiently accounted for by the small quantity of opium in the Dover's powder, and by the length of time which the child survived. The opium contained in ten grains of Dover's powder is equivalent only to about the twentieth of a grain of morphia, and probably about the same proportion of meconic acid. It is extremely rare that opium is found in the stomachs of young children poisoned by small doses.

Dr. J. B. Beck has lately published, in the 'New York Journal of Medicine,' some excellent remarks upon the effects of opium on the infant subject. He shows that while this drug has a much greater effect on an infant in consequence of the greater impressibility of the nervous system, than on an adult, it is at the same time much more uncertain in its operation; and thus is liable to prove fatal in very small doses. Among the instances which he has accumulated, illustrative of the powerful action of the drug, he mentions one where a young child was narcotized by fifteen drops of paregoric elixir. This essay has been republished in the 'Medical Gazette,' for March, 1844, (vol. xxxiii, p. 767.)

Quantity of opium required to destroy life. The smallest quantity of opium in the solid state which has been known to destroy the life of an adult was four and a half grains mixed with camphor. This case is quoted by Dr. Christison. In September, 1843, an instance occurred in this metropolis of a woman, aged thirty-eight, being killed by eight grains of the drug given in two doses. These facts are interesting in a medico-legal point of view, by showing how small a quantity of this substance may, in some instances, destroy life.

Solubility of opium in water. So far as I am aware no experiments have been performed to determine the quantity of this drug taken up by water in the form of infusion. In November, 1843, a case of poisoning by opium was referred to me by Mr. T. O. Duke, of Kennington, in which the question arose. An ignorant nurse made an infusion by pouring hot water on powdered opium in a bottle, and gave, at short intervals, three teaspoonfuls of this infusion to a child aged about fourteen months, and it died poisoned by the drug in about eighteen hours. It was found that the infusion contained only 1·6 per cent. of solid matter, i.e. of the soluble part of the opium; and that the principal part of the meconate of morphia had been taken up, was proved by an infusion subsequently made, retaining only faint traces of that salt.

The results of some experiments on this subject were as follows: Fifteen grains of finely-powdered opium were infused, for twenty hours, with six drachms of boiling distilled water. On examination, the filtered infusion was found to contain 4 per cent. of solid matter, i.e. of the soluble part of opium. In another experiment, opium sliced was employed with water in the same proportions. The quantity dissolved averaged, on several trials, from 3 to 4 per cent. depending on the proportion of water and the length of contact. By boiling the residue in each case, a further quantity of meconate of morphia was obtained, showing that an aqueous infusion, while it will not extract the whole of the meconate at once, will yet take up sufficient to render it actively poisonous to young children.

Prussic acid. It has been a seriously debated question among medical jurists, whether an individual, after having swallowed a strong dose of prussic acid, could retain the power of performing certain acts indicative of volition and the preservation of sense. Two cases have occurred within the last year in England, which throw some additional light upon this important question, on which a charge of murder may sometimes depend. In one case, the deceased, an adult, swallowed three drachms of prussic acid from the phial in which it was contained, while another person was in the room with his back turned to him. This individual was alarmed by hearing the deceased exclaim "it's gone," and in answer to a question put by witness, said, "I have taken it." He was again about to speak, but his articulation failed him, he became insensible and died immediately afterwards.

The other case was referred to me from Suffolk, by Mr. Newham, surgeon of Bury St. Edmunds. In March, 1844, a commercial traveller was found dead in his bed at an inn. The evidence given at the inquest showed that he had died from the effects of prussic acid, and there could not be the slightest doubt that he had taken the poison himself. The point of interest connected with the case is, that when discovered dead, he was found lying on his left side in the natural position of rest, the legs being slightly drawn up to the abdomen; the arms bent over the chest; and although rigid, the hands were not clenched, nor did they appear in any way to have been spasmodically affected. The bed-clothes were smoothly drawn up to his shoulders, and there was no appearance whatever of disorder about them. On a chair beside the bed, at his back, was a phial holding about six drachms, and still containing a small portion of a liquid smelling strongly of prussic acid, mixed with the essential oil of lemons, which had probably been purposely mixed with it to disguise the odour. This phial was found with the cork in it. Mr. Newham correctly observes that this condition of things, clearly indicates a sequence of several voluntary acts performed by the deceased immediately before death; as, for instance, swallowing the acid from the bottle, then corking the bottle, placing it on a chair at the back of the bed, the turning over in bed, drawing up the bedclothes, and composing himself into a position of rest. From the evidence at the inquest, it appeared that not less than three drachms of prussic acid had been taken, and probably even a larger quantity; and the question arose, whether all the events above mentioned could have occurred between taking into the stomach so large a dose of this poison as to cause death without inducing convulsions, of which there were no signs? The fact that this was really a case of suicide, left it beyond doubt that the deceased had, after swallowing this dose, performed the series of acts above mentioned; and it was equally evident that convulsions had not taken place, at least so as to leave any sign of their existence in the dead body.

The reader will observe that this case is very similar in its details to that of Judith Buswell, for the alleged murder of whom, a young man named Freeman was tried at the Leicester Spring Assizes in 1829. (See Med. Gazette, vol. viii, p. 759.) The medical opinions in that case, from a similar series of acts, were rather against the presumption of suicide, and in favour of homicidal interference. It has been supposed, that when a strong dose of prussic acid destroys life so slowly as to give time for the performance of such voluntary acts, this would be indicated by the body being found in a convulsed state; when, on the other hand, death takes place so rapidly that there are no convulsions, then the inference should be that the deceased could not have retained sense or power sufficiently long for the performance of these acts. The above with other similar cases proves, that we cannot trust to an assumed criterion of this kind. There may be no mark of convulsion about a dead body,—circumstances may show that sense, volition, and a power of motion were actually retained for a certain period; and yet all this is compatible with the act being one of suicide from a large dose of prussic acid. We are not justified in inferring that a dose of this kind, when it operates slowly, is always and necessarily indicated by the body of the deceased being found in a convulsed state.

This question has acquired still greater interest from the late trial of Belany for poisoning his wife by prussic acid (Cent. Crim. Court, Aug. 1844.) The prisoner declared that the deceased shrieked, and afterwards told him that she had swallowed some "hot liquid." The medical witnesses are reported to have stated (although only from experiments on animals) that this shriek or cry was the immediate precursor of insensibility, and the last act of vitality,—in short that the power of speech would be then entirely lost. Hence the prisoner's statement would be inconsistent with truth. However strong the circumstantial evidence may have been against the accused, and it could scarcely have been stronger,—this medical opinion is not borne out by observation. In one instance, just related (p. 551), a larger dose of the poison was probably taken; but the deceased was able to answer a question and say, "*I have taken it*," before he became insensible. A very similar case, reported by Dr. Gierl, is to be found in most works on toxicology. These cases then clearly prove that, whether a shriek or cry be a *constant* accompaniment of poisoning by prussic acid or not,—a point which yet remains to be proved,—an individual may speak and even answer a question rationally after having taken the poison, and immediately before falling into a state of insensibility.

A remarkable trial has lately taken place at Chambèry, in which the accused was charged with the murder of the deceased by prussic acid; while, in the defence, it was alleged, that death was owing to apoplexy and not to the poison. (Annales d'Hygiène, 1843, p. 103.) The case presents numerous points of interest in relation to medico-legal toxicology; the symptoms and post-mortem appearances met with in apoplexy, as contrasted with those produced by prussic acid; the value of evidence derived from symptoms in cases of poisoning, as well as that obtainable from the period at which death ensues after the supposed administration; the extraordinary chemical errors that are occasionally made in the analysis of poisons, the witnesses in this case imagining that the presence of poison might be inferred from a series of very doubtful or even negative results. The person charged with the crime was very properly acquitted; for there was no medical proof whatever that poison had been the cause of death, while there was direct evidence of death from apoplexy, by the discovery of a large effusion of coagulated blood on the brain. He appears to have owed his acquittal principally to the care bestowed by Orfila, on the examination of the facts of the case.

Oil of bitter almonds. One case of poisoning by this substance has lately occurred, and is reported by Mr. Smith of Clifton, (Lancet, June, 1844.) A girl, between 8 and 9 years of age, swallowed about a teaspoonful of a mixture sold by druggists as "ratifia," composed of one part of the essential oil of bitter almonds to seven parts of spirit. The quantity swallowed by the patient was equivalent to about *seven drops* of the essential oil. With this datum it will be interesting to consider the effects produced by so small a dose. When seen immediately after the accident, there was complete insensibility; the eyelids were closed, but the eyes were brilliant and glassy, without any mental expression; the pupils dilated; no pulse at the wrist; the carotids beating fully and quickly; relaxation of the muscles of the extremities, but the lower jaw was clenched in rigid spasm. Cold affusion with stimulants, stimulating frictions and emetics, were employed. Vomiting was induced, and the ejecta had a strong smell of prussic acid. In about twenty minutes the pulse returned,—the child opened her eyes, and was able to answer questions.

The quantity of prussic acid contained in the oil, and to which its poisonous properties are due, is said to vary from 8 to 14 per cent. The above case shows that in a small dose it may give rise to very alarming symptoms; and it is probable, that but for the active and prompt treatment adopted this child would have died.

Cyanide of potassium. This salt has of late years caused death in several instances where it has been taken by mistake or in improper doses. A gentleman was killed in France, in 1843, by taking *twelve grains* of the salt, in consequence of some error in the medical prescription. The physician who ordered the medicine, was tried, fined and imprisoned. (Lancet, January, 1843.) Another case occurred at Breslau, in which a man, aged thirty, died in a *quarter of an hour* after taking a dose of a mixture which had been prescribed for him by his medical attendant, under all the symptoms of poisoning by prussic acid. (Henke's Zeitschrift der S. A., 1843, p. 7.) The mistake here arose from those unfortunate changes periodically made in the nomenclature of pharmacopœial compounds, which constitute a matter of regret among ourselves; for such a practice takes away all certainty from the art of prescribing, and leaves the life of the patient and the character of the practitioner in the hands of a druggist, who may be ignorant of the properties of the medicine which he dispenses.

It appears that until lately the yellow ferrocyanate of potash was known in the Prussian Pharmacopœia under the short name of "kali hydrocyanicum," just as it was formerly called, in English, prussiate of potash, and is now termed ferrocyanide of potassium—an objectionable alteration from the term ferrocyanate, because many dispensing druggists might confound the *ferro*cyanide with the cyanide, and dispense the poison for the innocent substance. Of late years, in the Prussian Pharmacopœia, the cyanide of potassium has received the name of "cyanetum kalicum," or, improperly, "kali hydrocyanium." *Fifteen grains* of "kali hydrocyanicum," in a dose, were prescribed by the physician for his patient, he meaning thereby the ferrocyanate of potash. Instead of this, however, cyanide of potassium was sent, and the patient died in a quarter of an hour. The physician adopted and employed the chemical name which was probably current at the time that he studied his profession. The party who dispensed the medicine was undoubtedly to blame; for it appears that he entertained some doubt about the largeness of the dose, and he ought to have known that a dose of such a compound could not be taken by a human being without certainly destroying life. The energy of the cyanide of potassium as a poison depends, in some measure, on its mode of preparation. Some specimens are so impure as to consist almost entirely of carbonate of potash, from which it may be separated by its ready solubility in alcohol. (See Annales d'Hygiène, 1843, p. 404, in which this subject is fully investigated by Orfila.) An opinion formerly prevailed, that the poisonous properties of the salt were destroyed under two circumstances: 1, by exposure to air, in which case it is transformed to carbonate of potash; and, 2, by its being heated, in solution, to the boiling point. In neither case, however, does the salt easily lose its poisonous properties. Orfila found that some which had deliquesced, by exposure to air for a fortnight, still acted as a poison; and the conversion of the salt, at 212°, into ammonia and formate of potash takes place so slowly, under the most favorable circumstances, as not to interfere with this poisonous action. This substance does not therefore become innocuous, as it was formerly alleged, by solution in *hot* water. I have found by experiment that the ebullition of a solution, continued for a quarter of an hour, produced no sensible quantity of formate of potash.

Accidents such as those above referred to often give rise to charges of malapraxis. A case occurred some years since on the continent, in which a physician prescribed three grains of the "murias hydrargyri" for a child. Calomel was then known by the termination "dulcis," and corrosive sublimate by the termination "corrosivus." The dispenser sent corrosive sublimate, and the dose killed the child. The physician was prosecuted for not having been more precise in his prescription; but it is fair to inquire whether a person who would in such a case send three grains of corrosive sublimate, to be taken by a child, was qualified for the dispensing of medicines under any circumstances whatever. Owing to the numerous changes that have taken place in our own Pharmacopœia, it is somewhat surprising that accidents have not occurred. Corrosive sublimate now differs from calomel merely in the prefix "bi," which might be in some cases overlooked. The impolicy of this change is apparent in the fact, that, on a new edition of the Pharmacopœia, if this system of adaptation to ephemeral chemical theories be adhered to, corrosive sublimate will become "chloride of mercury"—the name now attached to calomel; and this latter substance will become a "dichloride." It is the opinion of some distinguished chemists, that what is commonly called peroxide is a protoxide of mercury, and the protoxide is a suboxide. All will agree that, for the safety of life, the names of medicines should be certain and unchangeable, and not vary with the fluctuating doctrines of the day; at any rate, it is a most serious result when the name attached to an innocent medicine at one time, should become applied to a powerful poison at another. Among the late "probability theories," as Berzelius terms them, which have emanated from the Giessen school, is one by which, if adopted, the present system of chemical and with it the pharmacopœial nomenclature will be completely overturned. Thus, an entirely new view is taken of the constitution of salts; and it is said that, instead of sulphate of potash being formed of an acid united to an alkaline base, it is the result of a union between a compound radical, formed of sulphuric acid and oxygen with the metal potassium. Pharmacy should be entirely independent of such hypothetical views; and all changes in the names of compounds should be made only for some very strong necessity, and with the greatest caution. It cannot be supposed that every practitioner throughout the empire should have the time, even if he had the inclination, to make himself master of the various speculations which are continually broached by chemists.

NARCOTICO-IRRITANT POISONS.

Cocculus indicus. Some researches have been recently made by M. Chevallier on the effects of this powerful poison. (Annales d'Hygiène, 1843, p. 339.) It appears that it has been the practice, in some parts of France, to poison fish by a mixture of this substance with crumb of bread, and sell the fish for food; and it is stated that, in many instances, such fish were eaten without any ill effects resulting. This, however, was a matter of accident, and depended on the quantity of drug used; when this quantity was moderately large, the fish acted like a poison on animals. It would appear, from the observations of M. Goupil, that it is only the kernel of the berry which is poisonous, owing to the presence of picrotoxine,—that it is narcotico-irritant in its effects, and that the fish destroyed by it exert a similarly poisonous action when eaten. The woody shell of the berry is not poisonous—it merely operates as an emetic.

It appears that, with respect to this pernicious drug, the French system of legislation is like our own. There is a heavy penalty on the sale of it for certain purposes, but the free importation of it is allowed. The large quantities which are said to be openly and secretly imported into this country, can be applied to no lawful purpose; for the substance is utterly useless, both in medicine and the arts. There is no doubt that it is employed for the extensive adulteration of beer. The proper remedy would be to exclude it altogether: for it is absurd to attempt to prohibit its sale by a penalty, when its introduction has been once permitted.

Cytisus laburnum. The existence of a new and powerful narcotico-irritant poison has been lately announced by Dr. Christison, in the bark of the common laburnum tree. (Edin. Med. and Surg. Journ., Oct. 1843.) It is remarkable that, considering how widely this tree is diffused, and how accessible it is as a poison, as well as the fact that its noxious properties have been known for some time to the vulgar—at least in certain parts of the kingdom,—it has not before received any attention from toxicologists.

The case reported by Dr. Christison came to trial at the Inverness circuit last year. A youth, with the intention merely of producing vomiting in one of his fellow-servants, a female, put some dry laburnum-bark into the broth which was being prepared for their dinner. The cook, who remarked a "strong peculiar taste" in the broth, soon became very ill, and in five minutes was attacked with violent vomiting. The account of the symptoms is imperfect; for the cause of them was not even suspected until six months afterwards. The vomiting continued thirty-six hours; was accompanied by shivering,—pain in the abdomen, especially in the stomach,—and great feebleness, with severe purging. These symptoms continued, more or less, for a period of eight months; and she fell off in flesh and strength. At this period she was seen by a physician, who had been called on by the law-authorities to investigate the case. She was then suffering from gastro-intestinal irritation, vomiting after food, pain in the abdomen—increased by pressure, diarrhœa, tenesmus, and bloody stools, with other serious symptoms. The medical opinion was, that she was then in a highly dangerous state. The woman did not eventually recover until the following April. There was no doubt, from the investigation made by Dr. Ross and Dr. Christison, that her protracted illness was really due to the effects of the laburnum-bark.

Some experiments were then made on the action of the poison on animals. A teaspoonful of the powder of dry laburnum-bark was administered to a cat. Soon afterwards it writhed, apparently in great pain; in a short time it vomited violently, and, although languid and dejected for the rest of the day, it quickly recovered. Sixty-nine grains of the same powder were given to a dog. In ten minutes it whined and moaned, vomited violently, and soon got well. On a second occasion, twenty grains were found to act as a powerful emetic upon the animal. An ounce of the infusion of laburnum-bark, containing the active matter of sixty-two grains, was introduced by a catheter into the stomach of a full-grown rabbit. In ten minutes, the animal looked quickly from one side to the other, twitched back its head twice or thrice, and instantly fell on its side in violent tetanic convulsions, with alternating emprosthotonos and opisthotonos, so energetic, that its body bounded with great force upon the side, up and down the room. Suddenly, however, all movement ceased, respiration was at an end, the whole of the muscles became quite flaccid, no sign of sensation could be elicited, and the animal died within *two minutes and a half* after the poison was injected into the stomach. The body was opened in two minutes more, and the heart was found gorged, but contracting with some force. The stomach was filled with green pulp, soaked with the infusion. No morbid appearance was visible anywhere. In repeating this experiment, one rabbit died in half an hour, another in three quarters of an hour after small doses of the infusion were injected into the stomach; and a third rabbit speedily died, after eating greens merely impregnated with the infusion. In all these instances, convulsions were the leading symptoms produced. The same effects are popularly ascribed to the leaves, young pods, and seeds of the tree; but no experiments were performed with these.

The facts here detailed show that laburnum-bark is a most energetic poison—as powerful, even, as nux vomica. There are no means of detecting the nature of this poison, especially when administered in powder or infusion; or when, as in this criminal case, a decoction of the bark is given in food. The only plan for determining the deleterious properties of the substance, would be by exhibiting a portion to animals. As Dr. Christison remarks, these facts are of considerable importance; and as they relate to a substance so common, and so easily obtained by every one, they ought to be more generally known to the profession than they are at present.

Œnanthe crocata. Another instance has occurred lately of the loss of life among the convicts at Woolwich, by the eating of the leaves and roots of this powerful indigenous vegetable poison. The facts have been communicated to the Medical Gazette, (May, 1844,) by Mr. Bossey. It appears that a party of convicts ate of the root and leaves of the plant while engaged at work. In about twenty minutes one man, without any apparent warning, fell down in strong convulsions, which soon ceased, but left a wild expression on his countenance. Soon afterwards, as many as nine fell into a state of couvulsions and insensibility. The face of the man first seized became bloated and livid; there was a sanguineous foam about the mouth and nostrils; the breathing was stertorous and convulsive; there was great prostration of strength, and insensibility: he died in *five minutes.* A second died, under similar symptoms, in a quarter of an hour, although the stomach-pump was used, and some leaves were extracted with the fluids. A third, who had assisted in carrying the two former, was himself seized with convulsions, and died in about an hour; and soon after him, a fourth died, in spite of the most energetic remedial treatment, by cold affusion, emetics, stimulants, stimulating frictions, and the use of the stomach-pump. Two other cases proved fatal, the one in nine days, and the other in eleven; and in these two cases, there was irritation of the alimentary canal. On inspecting the bodies of those who died quickly, there was congestion of the cerebral vessels, and, in one instance, a layer of extravasated blood was found beneath the pia mater. In the first case, which proved most quickly fatal, the cerebral vessels were not congested. The pharynx and œsophagus had a white appearance, contained some mucus and portions of the root. The lining membrane of the trachea and bronchi was intensely injected with dark blood. The lungs were gorged with fluid blood. The blood in the heart was very black and fluid. The stomach and intestines were externally of a pink colour: the cavity of the stomach was lined with a thick viscid mucus, containing portions of the root. The mucous membrane was much corrugated, and the follicles were particularly enlarged. Similar appearances were met with in all. In the two protracted cases, the mucous membrane of the stomach and bowels was softened and thickened. It had a pink colour externally, but no red appearance internally. The vessels of the brain were congested. In the others who partook of the roots, the symptoms were not so urgent. Under the free use of purgatives, considerable quantities of the root were discharged, and in a few days the men recovered. By a similar accident in 1834, the lives of four men were lost from the action of this vegetable poison.

There is no doubt that the œnanthe is one of the most powerful of the indigenous narcotico-irritant poisons. It destroys life with even greater rapidity than arsenic; for it here proved fatal to a strong healthy man in less than *one hour.* Chemists have not yet ascertained on what principle its active properties depend, but they appear to reside chiefly in the root.

Digitalis Purpurea. The following recent case of poisoning by this plant is reported by Mr. Wilson of Leeds (Med. Gaz., Aug. 1844.) A healthy robust young man, affected with sore throat, was advised to take "*throatwort tea.*" Having filled a quart pitcher with fresh leaves of the digitalis purpurea, he poured upon them as much boiling water as the pitcher would hold. Of this strong infusion he took a teacupful on going to bed which caused him to sleep soundly. In the morning he took a second cupful (the infusion being then much stronger), and went to his employment. He soon felt dizzy and heavy, began to stagger, lost his consciousness, and at length fell down in a state of syncope. On being conveyed home, he vomited severely and complained of extreme pain in the abdomen. When visited he was conscious, complained of great pain in his head,—the pupils were dilated, and the surface was cold, pallid, and covered with a copious perspiration. The pulse was low, about 40 in the minute,—three or four feeble pulsations being succeeded by a complete intermission of several seconds; and each stroke, though weak, was given with a peculiar "explosive shock." There was still great pain in the abdomen, with incessant and violent vomiting, no diarrhœa,—suppression of urine, and an abundant flow of saliva. Brandy and ammonia with warmth were employed, and after reaction had commenced,—purgatives were administered. The man slowly recovered, but the pulse presented its peculiar beat and weakness for several days; and during this time, the man could not bear the upright position.

The symptoms in this case were like those which have been usually observed. It establishes beyond question that salivation may be produced by this plant.

Alcohol. A singular instance is referred to in a late number of the 'Lancet' (April, 1844), in which a child aged 2 years was thrown into an apoplectic stupor, from the alcoholic vapour of eau de Cologne. There is no doubt that the long-continued respiration of the vapour of alcohol or ether might prove dangerous to a child.

PART FOURTH.

Medical Intelligence.

ON DISLOCATIONS OF THE ASTRAGALUS.

REPLY TO MR. TURNER'S RECLAMATION, PUBLISHED IN THE PROVINCIAL MEDICAL AND SURGICAL JOURNAL, AUG. 7, 1844.

In the last Number of the British and Foreign Medical Review, we noticed Mr. Turner's paper on "Dislocations of the Astragalus," in the ordinary discharge of our duties towards our readers. Our observations have appeared to Mr. Turner, hypercritical, uncourteous, unfriendly, unfair, and unjust, and he has consequently published in the 'Provincial Medical and Surgical Journal,' of the 7th of last August, a letter addressed to Dr. Hastings, in which he endeavours to establish these grave charges. Though the imputation of deliberate unfairness is calculated to excite some indignation, our consciousness that the charge is utterly groundless, completely allays any such feeling. Had we, through inadvertence or negligence, unintentionally aggrieved Mr. Turner, we would feel pride and pleasure in frankly acknowledging our error, and we shall proceed calmly and dispassionately to inquire whether we are really chargeable with mistake.

Mr. Turner's first tangible charge is, that we endeavoured to make it appear that he has not done justice to Sir A. Cooper's work on 'Dislocations and Fractures of the Joints.' Mr. Turner is mistaken; we simply averred that he had not availed himself of all the materials applicable to his purpose, contained in that work. If that averment can be interpreted into a charge of injustice, and assuredly it cannot, Mr. Turner has been unjust to himself and not to Sir A. Cooper. The question, however, whether Mr. Turner did or did not omit relevant cases, is a question of fact which we shall presently examine; but we must previously advert to an alleged unwillingness on our part to admit the truth of Mr. Turner's opinion respecting the comparative frequency of simple and compound dislocations of the astragalus: on Mr. Turner's own showing, we have manifested this unwillingness in a very singular manner, for, to use his own words, the reviewer "*confirms*" his opinion "*by additional evidence.*" We might here congratulate ourselves on this admission, that we have added something to the undoubtedly valuable materials collected by Mr. Turner; but letting that pass, we have, it is alleged, been guilty of "servile adherence to authority," in venturing to add that "it remains to be seen, however, whether Sir A. Cooper's opinion may not prove true, if ever a very extensive series of cases is collected." We beg to assure Mr. Turner that we are not slaves either to authority, prejudice, or passion; we are willing to yield implicit obedience to facts, when they are sufficiently numerous to warrant us in deducing from them a definite opinion. So far from servilely adopting Sir A. Cooper's dictum, we have contributed "additional evidence" against its truth; we would much rather be right with Mr. Turner than wrong with Sir A. Cooper; but we still venture to retain our conviction, that statistical conclusions are eminently uncertain, unless they are deduced from a very large number of observations—unless all, or nearly all, the ascertainable facts capable of influencing the conclusion are compared; and it is in obedience to this universally admitted rule of statistical investigation, that while we provisionally admit Mr. Turner's opinion, we hold that the comparison of a very extended series of cases may possibly ultimately disprove it. How the result of a numerical fact can be influenced one way or the other by its having been elicited by the researches of a "provincial hospital surgeon," we confess our inability to understand, and must therefore leave Mr. Turner's observation on that head unanswered.

We have now to deal with the specific question, whether Mr. Turner was right or wrong in omitting from his statistical tables certain alleged cases of dislocation of the astragalus, contained in Mr. B. Cooper's edition of Sir A. Cooper's work, on 'Dislocations and Factures of the Joints.' Mr. Turner admits that he omitted the cases we referred to, and justifies the omission by certain reasons assigned, which it is, of course, our duty to examine; and in so doing, it will, to use Mr. Turner's words, "save time and trouble" to inquire "what is really meant by dislocation of the astragalus;" a point, respecting which, Mr. Turner says, we require information, and although he "may be considered presumptuous in attempting it," he shall endeavour to set us right.

Mr. Turner's definition of a dislocation of the astragalus is this, that *all* the articular surfaces of the bone must be, in some degree, displaced, and consequently, that "if the astragalus retain its natural attachments with the tibia and fibula, but happen to be torn from the os naviculare, or os calcis, or both, it would not be a dislocation of the astragalus, but a diastasis, or disjunction of one, or both of these bones *from* the astragalus. In a word, so long as this bone is in situ, in reference to either of the bones of the leg, or those of the foot with which it is articulated, the astragalus *itself* is not dislocated. . . . . . . A bone that has been dislocated must, to constitute this accident, be ex situ in reference to *all* its articular surfaces, however numerous the articular surfaces may be; so long, therefore, as the astragalus is not severed from the tibia, fibula, os calcis, and os naviculare, or is retained in natural anatomical contact with either of them, the dislocation is of the bone or bones in connexion with it, not of the astragalus itself." Now this language is very precise, admits of no mistake. If we were so hypercritical as Mr. Turner supposes, we might comment on the strange consequences that must of necessity flow from the new doctrine—"*A bone that has been dislocated must, to constitute this accident, be ex situ in reference to* ALL *its articular surfaces, however numerous the articular surfaces may be*"—if we attempt to apply it to dislocations of the long bones: and we might, without the remotest approach to hypercriticism, insist that Mr. Turner means it to be so applied, as a few lines above the last extract he maintains, that according to his explanation, "our views of dislocation become definite, not only with respect to the accidents occurring to the astragalus, but in reference to *every other kind* of dislocation." Let us, however, limit the doctrine to the astragalus; and even then we maintain that Mr. Turner is wrong, and that partial or complete disconnexion of the astragalus from the bones of the leg, is *not* essential to constitute a dislocation of the astragalus; and in support of this assertion, we need only appeal to the whole stream of authorities—J. L. Petit, Boyer, Moufalcon, Savary, Marjolin, A. Cooper, Sanson, Laugier, Roux, &c. But perhaps it may be said—here again is servility to authority; and if so, we reply, that two points have to be considered in a question of nomenclature: 1st. What the received nomenclature *is*; 2, what it *ought* to be. The first point can only be decided by an appeal to usage, that is to authority; and we repeat that the whole weight of authority is with us and against Mr. Turner; for Rognetta alone—the exception that proves the rule—sanctions Mr. Turner's view; and even Rognetta somewhat inconsistently adds, "the essential condition for luxation of the astragalus is, that the head of that bone has left the eliptical cavity of the scaphoid bone." As to the 2d point, viz., what the nomenclature *ought* to be, we are of course dispensed from the necessity of considering that question. We had no choice in our review, save to adhere to recognized usage; had we done otherwise, Mr. Turner would indeed have had cause for complaint; but thus much we must say, that not merely the entire weight of authority, but the whole weight of argument preponderates against Mr. Turner's, or rather, Rognetta's view. Let any impartial inquirer examine Rognetta's own account of the mechanism of anterior dislocations of the astragalus, either in our review of Mr. Turner's paper, or, better still, in the original memoir, and he will have the key to the argument, which irresistibly proves the propriety of the received nomenclature; neither time, nor space, nor fitness of occasion, allows us to enter fully into this branch of the subject, on which we have indeed said enough; but we cannot refrain from remarking, that Mr. Turner violates his own doctrine, so explicitly laid down in the extract above given. He quotes Boyer's last case as an example of dislocation of the astragalus, and actually founds one of his practical inferences on the result of this very case, and yet, according to Mr. Turner's views, this case is *not* one of dislocation of the astragalus, as that bone was not separated from its connexion with the bones of the leg. We have, therefore, here the satisfaction of appealing in our justification to Mr. Turner himself. We shall not retort the charge of ignorance as to what is really meant by dislocations of the astragalus: we should be ashamed to throw such an imputation on Mr. Turner, whom we unfeignedly believe to be a gentleman as eminent in professional attainments, as he is respectable in private character. We are willing to believe that Mr. Turner, in consequence of writing in some hurry, and with more excitement, fell into an inadvertence. It may be, indeed, that Mr. Turner deliberately adopts Rognetta's very peculiar view; but if so, we must be pardoned for remarking, that he should have explicitly declared his deviation from the received opinion in his essay, and should, in his letter to Dr. Hastings, have abstained from imputing ignorance to his reviewer for adhering to known usage.

With respect to the cases in Sir A. Cooper's work, omitted by Mr. Turner: cases 193 and 200 were omitted, we are told, because the connexions of the astragalus with the tibia and fibula remained undisturbed. We have already disposed of that point. Case 192, it is said, is doubtful, but Mr. Hey and Sir A. Cooper both expressly recognize it as a "compound luxation of the astragalus." Case 191, Mr. Turner says, cannot be termed dislocation of the astragalus; and on reconsideration, we admit that, properly speaking, it cannot, but its bearing on the history of the accident is sufficiently important to induce Sir A. Cooper to again notice it under that head, after having previously given the case more fully. Case 194, Mr. Turner informs us, he rejected, as being an example of compound luxation of the tarsal bones; but the prominent fact mentioned in the case is, that the head of the astragalus was displaced, and, therefore, we rank it among dislocations of that bone. Case 196 was passed by because there was comminuted fracture as well as dislocation of the astragalus. But need we remark that fracture is by no means an unusual complication of dislocation of the astragalus, and coexisted in several of the cases adopted by Mr. Turner himself.

Mr. Turner complains that our comments on his selections from Boyer's cases are hypercritical. We expressly said, that the inaccuracy respecting Deniel's case was trifling. Had it stood alone, it would have been indeed hypercritical to notice it; but our argument was a cumulative one, and, therefore, our observation on the case was justifiable as a member of that argument. We are accused of wrongly saying, that Boyer's work is not referred to for that case, but we of course only meant that the specific reference thereto to enable the case to be identified was not given, as sufficiently appears from our previous remark that the case is given as having occurred in Boyer's practice. We are also reproached with having noticed the oversight of Boyer's case of dislocation of the foot, being quoted as one of dislocation of the astragalus backwards, and then commenting on the different view taken of "*these* cases" by Boyer and Mr. Turner, while we were well aware that they "were writing on two different accidents," and that, therefore, "there was no difference of opinion between" them, and that they were both correct in their remarks on the subjects on which they respectively wrote. We acknowledge the dexterity, but we say nothing of the candour of putting the matter in this way. Undoubtedly we were well aware that Boyer and Mr. Turner were writing on two different accidents, for that was the very oversight we pointed out, but they were writing about *one and the same case*, and we adverted to the difference of their views respecting *that case*. Mr. Turner pleads that the omission of the case altogether "would not have detracted, in the slightest degree, from his practical argument;" undoubtedly it would not, as it does not in the slightest degree bear on that argument; but we ask, would not the omission of the case have added something to his accuracy? As to Dupuytren's cases, we neither doubted nor implied any doubt, that twelve cases of dislocation of the astragalus occurred in his practice; we are well aware of the references for that fact given by Mr. Turner, and could point out others to him; but we said, and we repeat, that only five of them are distinctly recorded, and, consequently, to those five alone did we refer, while Mr. Turner quotes but three of them. But we are further charged with hypercriticism, mystification, and injustice, in our comments on Mr. Turner's inaccuracies respecting Dupuytren's cases. We shall content ourselves with asking—was it hypercritical or unjust to observe, that in a table purporting to give the statistical results of the treatment of dislocations of the astragalus, one of Dupuytren's cases is enumerated among those headed "bone allowed to remain in its new situation," whereas, in point of fact, the bone was *excised;* and that another case, placed in the same class, should have been placed under the head "partial reduction." Mr. Turner says, he cannot understand this latter distinction; perhaps not; but if so, why did he adopt the distinct class "partial reduction" in his statistical classification of the treatment and results of cases?

Mr. Turner offers no explanation why he omitted two of Dupuytren's cases. Three of Desault's he did not specially notice, as they were complicated with compound dislocation of the ankle-joint, a circumstance which however, in our humble judgment, greatly enhances their importance; the other four are not alluded to. Hey's cases were omitted, as they are not contained in the first edition of that eminent surgeon's work; we have been ourselves too often embarrassed from a similar cause not to fully admit the validity of this plea.

We have now to deal with by far the gravest imputation brought against us by Mr. Turner,—an imputation affecting our honesty and fairness. It is said that we "selected all the vulnerable parts of the paper," and "*sedulously* avoided the mention of any points of merit." If such were the case, we would indeed feel disgraced; but we firmly, though we trust temperately, deny the accusation. We are accused of not having noticed the chapters on the anatomical relations of the astragalus, and on the forces which produce its dislocations; but does Mr. Turner forget that a portion of our article is devoted to those very considerations in our notice of Rognetta's memoir? Was not our silence as to Mr. Turner's observations on these points sufficiently significant why we were silent? But we are now compelled to say what we then abstained from saying, that we—it may be erroneously, but most assuredly in the best exercise of our judgment, *quantum valeat*—considered and still consider Rognetta's investigations in this respect so greatly superior to Mr. Turner's speculations, that we deemed it no injustice to our readers, and far from unkindness to Mr. Turner, to pass those parts of his paper in silence. Mr. Turner is of opinion, that "we cannot, by any experiment, illustrate or imitate the mechanism of dislocation." (p. 22.) And so long as he retains this opinion, we fear he can scarcely so write on the subject as to satisfy the exigences of modern science. We regret, however, that even where we have awarded what we thought to be due praise, Mr. Turner is dissatisfied. He seems displeased at our saying that his principles of treatment are "on the whole very judicious," though we added, with the courtesy which we felt was due on practical points to a surgeon of his experience and capacity, that where we differed from him it was perhaps erroneously. Mr. Turner moreover accuses us of a "*perversion*" of one of his practical rules. We shall insert the original text, followed by our version of it, and then leave our readers to judge on whom "*perversion*" is justly chargeable.

"*If the astralagus be* PARTIALLY DISLOCATED, *and not twisted round* (*as is often the case when the dislocation is complete,*) *there is reason to hope that reduction may be accomplished.*" (p. 89.)

"Reduction should always be accomplished in partial and simple dislocation of the astragalus; *but if it is impeded by the astragalus being twisted*, *etc. operative interference*," *etc.*

The words printed in italics in the latter extract are suppressed by Mr. Turner. We make no comment on this. Had we been influenced by the malevolent motives attributed to us, might we not have made out a plausible and in many respects a real case, showing that Mr. Turner had been anticipated in his practical views by Laugier, in his excellent paper on dislocations of the astragalus? but we felt that to do so would have been unjust, because we were persuaded that Mr. Turner had borrowed nothing from Laugier; that their points of agreement were but coincidences; and moreover that Mr. Turner had added many valuable practical points, which we took good care to mention in our article, carefully avoiding anything that would seem to question the originality which we thought due to him.

We think we have satisfactorily shown that we were not influenced by any unfair or unfriendly spirit in drawing up our review of Mr. Turner's essay. Indeed Mr. Turner himself impliedly acknowledges the justice of the *tenor* of our remarks, by pleading that his paper was hastily drawn up: but how could we go behind the author's explicit statement that "with a view to correct statistics," he had "made diligent search for published as well as unpublished cases," and given "all the cases" which, "after much labour," he had "been able to collect?" We consequently dealt with Mr. Turner's paper as a statistical one, and ought not to have done otherwise. This is the obvious, the unanswerable reason, why our article assumed the shape it did; and hence also the answer to the complaint that we reckoned Mr. Turner's cases as 44 in all, 16 being unedited. We did so because these cases alone are numbered by Mr. Turner, and these alone are introduced into his statistical tables; and no loophole for complaint is left there to Mr. Turner, for the cases which he does not number, but only mentions *incidentally*, are noticed by us as being *so* mentioned. Mr. Turner asks, "Is the Reviewer serious in recommending me to publish this essay?" That is not exactly our recommendation: but we were and are of opinion that he may convert it into an extremely valuable monograph by remedying certain defects which we attempted to point out. We saw enough in Mr. Turner's paper to induce us to think that he was capable of discharging his task in a manner worthy of his subject and highly creditable to himself. We expressed that opinion in our review, and by that opinion we abide. We are glad to learn that Mr. Turner, though uninfluenced by our advice, is yet induced by other motives to act in accordance with it; and we are gratified that Mr. Turner, despite the asperity of controversy, frankly acknowledges himself obliged to our humble labours for indicating materials which may prove useful to him in the execution of his laudable design.

August, 11*th*, 1844.

ON THE FUNCTIONS OF THE WHITE CORPUSCLES. BY DR. CARPENTER.

To the Editor of the British and Foreign Medical Review.

MY DEAR SIR,—It is not my intention to request a place in your Review for any lengthened reply to the arguments by which Mr. Wharton Jones has attempted to subvert my views on the functions of the White Corpuscles, and has defended his own previously expressed doctrines; for I think that its pages may be much better occupied than with a voluminous discussion of this kind, from which each party will be pretty sure to retire, with an increased conviction of the truth of his own views, and of the weakness of his adversary's position. But I must request the attention of your readers to the manner in which Mr. Wharton Jones disposes of one of my chief arguments,—the *absence* of anything resembling the *red* corpuscles, in the blood of invertebrated animals; and the universal *presence* of floating cells, exactly resembling the *colourless* corpuscles of the blood of vertebrata, in greater or less abundance. On this Mr. W. J. contents himself with remarking (§ 62) that, "considering how very imperfect the knowledge of the blood of the invertebrata is as yet, any analogical argument drawn from it in the present case is of too loose a nature to merit the slightest attention." This is certainly a very convenient manner of answering a troublesome argument; but I shall endeavour to show that it is quite inadmissible in the present instance.

I think that no unprejudiced person can refuse assent to the proposition, that the processes of nutrition must be essentially the same in the invertebrata as in vertebrated animals: their food is the same, and must yield albumen; their blood coagulates, and must therefore contain fibrin; and their tissues have the same microscopical and chemical characters, and may therefore be presumed to originate in the same manner. The chief difference in their nutritive apparatus consists in the absence of any *special* absorbing system; and in the low amount of heat generated by them, except in the class of insects under particular circumstances—for which the peculiar arrangement of their respiratory apparatus fully accounts, as I have shown on a former occasion.

That the blood or circulating fluid of the invertebrata does not contain red corpuscles, and that both in the character of its floating cells, and its imperfect coagulation, it rather resembles the chyle and lymph of vertebrata, was first, I believe, stated by Wagner; and it was on his authority that I introduced the fact into my Report. But during a stay of some weeks by the sea-side last autumn, I had the opportunity of verifying the statement upon a large number of minute annelida, crustacea, and mollusca, in which I could watch the circulation during life; and I have at other times made many similar observations upon the transparent aquatic larvæ of insects. The fact may, I think, be regarded, therefore, as tolerably well established.

Now if we apply the term blood to the nutritious fluid of animals in general, it is evident that the presence of *red* corpuscles, being *limited* to one only of the four sub-kingdoms, cannot be regarded as essential to the character of this fluid; and their function must be one *peculiar* to the group in which they appear. On the other hand, the presence of those floating cells, to which we give the name of *colourless* corpuscles, appears to be *universal;* and their function must be one of a *general* kind. That their use is not to generate red corpuscles is evident from the total absence of the latter. That if either kind of corpuscles be the agents in elaborating the plastic element of the blood, it must be the *white*, appears evident from the same consideration,—this being the most universal of all functions, without which no tissue can be generated. On the other hand, that the *red* corpuscles are subservient to the generation of *heat*,—a function limited to vertebrata, with the exception already noticed,—appears from the constant proportion which exists under ordinary circumstances between the number of red corpuscles in the blood, and the heat developed in each tribe of animals,—a general doctrine which receives important confirmation from recent experiments of Dr. John Davy (Philosophical Transactions, 1844.)

My argument, therefore, is not an *analogical* but a *direct* one; and seems to me to bear more closely on the question at issue than any which has been urged. If fibrin can be elaborated without *red* corpuscles, they cannot be the agents of its production. If, on the other hand, *white* corpuscles present themselves wherever fibrin is in process of elaboration (and I have yet to learn that there is any exception to this generalization), there seems a strong probability in favour of their connexion with the process.

Mr. Wharton Jones seems, by his note to § 45 of the first part of his Report, to think that I have deprived him of his due in placing Wagner and Henle on a level with himself as the enunciators of the doctrine, that the fibrin of the blood is elaborated from the albumen by the agency of the red corpuscles. I can only say that I had fully comprehended this to be Wagner's view, from the perusal of Dr. Willis's translation of his 'Physiology' (p. 448), before the publication of Mr. Wharton Jones's Report on the Blood; nor can I now see how the expression,—that the red corpuscles " bear the same relation to the plasma and its normal composition as the cellular parts of the secreting glands do to the secreted fluids,"—is to be understood in any other manner. Believe me to be, my dear Sir, yours most truly, W. B. CARPENTER.

RIPLEY; *Sept.* 12*th*, 1844.

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1. Observations on the Epidemic Fever of 1843 in Scotland. By W. P. Alison, M.D.—Edinburgh, 1844. 8vo. pp. 80.

2. Thoughts on Physical Education, &c. By C. Caldwell, M.D. With a Preface by George Combe. Second Edition. — Edinburgh, 1844. Royal 8vo, pp. 36. 1s.

3. An Address delivered before the Dublin Obstetrical Society, December 1843. By W. F. Montgomery, M.D. – Dublin, 1844. 8vo. pp. 23.

4. On the best Modes of representing accurately by Statistical Returns, the Duration of Life, &c. By Edwin Chadwick, F.R.S.—London, 1844. (From the Statistical Journal.)

5. Catalogue of the Museum attached to the Class of Military Surgery in the University of Edinburgh.—Edin., 1844. 8vo, pp. 37.

6. Cases of Deformity of various kinds, successfully treated by Plastic Operations. By T. D. Mütter, M.D. — Philadelphia, 1844. 8vo, pp. 38.

7. A Report on the Operations for Fissures of the Palatine Vault. By T. D. Mütter, M.D.—Philadelphia, 1843. 8vo, pp. 28.

8. Cases of Deformity from Burns successfully treated by Plastic Operations. By T. D. Mütter, M.D.—Philadelphia, 1844. 8vo, pp. 24.

9. Zur Vermittelung der Extreme in der Heilkunde. Von Theodor Stürmer, M.D., &c.—Vols. I, II, III.—Leipzig, 1837-9-43.

10. Actonian Prize Essay, Chemistry as Illustrative of the Wisdom and Benificence of God. By E. Fownes.—London, 1844. 8vo, pp. 184.

11. A Pratical Treatise on Midwifery. By M. Chailly, M.D. Translated from the French By G. S. Bedford, A.M. M.D.—New York, 1844. 8vo, pp. 526. With 216 Woodcuts.

12. The Practice of Medicine; a Treatise on Special Pathology and Therapeutics. By R. Dunglison, M.D., &c. Second Edition. In Two Volumes 8vo.—Philadelphia, 1844.

13. Defence of Hahnemann and his Doctrines, including an Exposure of Dr. A. Wood's Homœopathy Unmasked.—London, 1844. 8vo, pp. 92.

14. On the Decrease of Diseases effected by the progress of Civilization By C. F. H. Marx, M.D. and R. Willis, M.D.—London, 1844. 12mo, pp. 102.

15. Graefenberg; or a true Report of the Water Cure, with an Account of its Antiquity.—London, 1844. 8vo, pp. 322.

16. Narrative of a Voyage to Madeira, Teneriffe, &c. By W. R. Wilde, M.R.I.A. Second Edition enlarged.—Dublin, 1844. 8vo, pp. 648.

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